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THE LANCET.

VOL. II.] LONDON, SATURDAY, APRIL 7. [1831-32.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE

IN THE UNIVERSITY OF LONDON,

Delivered Feb. 27th, 1832.

CASE OF ANASARCA AND ASCITES.

Singular general Disposition to Hypertrophy in the Large Viscera.

THE parts before me, Gentlemen, were all taken from a woman who died on the 23rd of this month in Mary's Ward. She was 35 years of age, and had been admitted on the 7th of January with universal dropsy, at least with anasarca and likewise ascites. She stated that she had been ill only two weeks. I suppose she only knew herself to have been ill two weeks, but it is quite apparent that she had been ill much longer than that.

Symptoms.—The belly was very much distended. There was decided fluctuation in it, and consequently no doubt existed of the ascites. With regard to the anasarca, she said that her face swelled at the very first, that one of her first symptoms was the swelling of the face; and it appeared that the anasarca and the ascites began at the same time. It seemed from all this, that she was only labouring under inflammatory dropsy. In that form of dropsy the face almost always swells as soon as any other part; I mean to say when the dropsy is an acute disease. She said she had been ill only a fortnight altogether, and when dropsy is an acute disease, it arises from cold or wet, as most inflammations do, and best yields to bleeding. But the abdomen was exceedingly tender on pressure, so that there was a decidedly inflammatory state of the peritoneum. It is not at all uncommon for ascites to be an inflammatory disease, like any other dropsy, and you may always infer it when with fluctuation of the belly there is extreme pain on pressure. She said that she made but little urine. In inflammatory anasarca, it is common for patients to make as much urine as in health, and sometimes more. On examining the urine, it was found to be albumenous—to contain serum. She had also nausea and vomiting, which, in all probability, arose from the peritoneal coat of the stomach being inflamed. When peritonitis exists, if the peritonitis happen to affect the outer part of the stomach—the peritoneal coat of that organ, then you are almost sure to have extreme vomiting, and, of course, nausea. The bowels, she said, were open; there appeared to be no affection of the inner coat of the alimentary canal, for the bowels were neither relaxed nor confined.

Treatment.—I ordered her to be bled to the extent of twelve or sixteen ounces, accordingly as she might bear it, and the blood proved to be buffy. Four grains of calomel were given her twice a day. I had no fear of increasing the nausea and vomiting by the calomel, because there was clearly peritonitis, as evinced by the extreme tenderness of the whole abdomen; and the calomel, if it acted strongly on the inner surface of the alimentary canal, would in all probability relieve the irritation on the outer surface, for it is by no means uncommon for the inner and outer surfaces of the alimentary canal to be in opposite states. I have seen, on the cessation of diarrhœa, ascites take place; and the best way to get rid of the latter is, to effect a good drain from the inner surface. On the following day 20 leeches were applied to the abdomen, and the effect of all this was, that the nausea and vomiting ceased, and the quantity of urine was increased. She was bled again on the 13th to ten ounces, and as her mouth was tender, I ceased giving her mercury, but exhibited half an ounce of tartrate of potash every day. Some little inflammation of the bronchiæ then came on. There was quick respiration, with cough, and sonorous rattle, and for that she had a blister applied on the chest, the pulse not justifying further bleeding. She had been bled once to twelve ounces, once to ten ounces, had had twenty leeches on the belly, and as the half ounce of tartrate of potash did not purge her much, the quantity was increased to six drachms.

Now the anasarca all went away, excepting a little swelling in the ancles. The ascites likewise diminished considerably, so that the liver could be felt, and now it was found to be enlarged and greatly indurated. It could be felt in the right hypochondrium particularly, as low as the umbilical region. The edge was well defined, and the whole of that organ was excessively hard. It was clear now, that there had been an active acute attack upon a chronic disease,—that the woman must have been some time labouring under enlargement and induration of the liver,—hepatic disease, and from some cause or other, an active inflammatory state of the peritoneum had been set up. You will find this a very common thing in chronic disease, particularly in chronic affections of the heart, and other contents of the chest; but it also takes place in chronic affections of the abdomen. In this case it was evident that a sudden acute attack supervened on a chronic affection. It is necessary to be aware of the possibility of this circumstance, or you might think that it was only the chronic disease getting worse; but if you know when there is organic disease of the heart, that the patient is liable to an attack of active inflammation in the chest, then, when such an occurrence takes place, you will have ascertained the point, and you may be able to remedy it. If you were not aware of this, you might neglect to treat it as inflammation, supposing that it was merely an organic disease getting worse. For instance, you might take it to be only still chronic bronchitis, or phthisis, and then no more could be done, whereas if you be aware that active inflammatory attacks easily seize parts, or the neighbourhood of parts, when there is organic disease, you will be prepared to remedy that in the usual way, and the patient will probably be brought back to the state in which he was before the acute attack came on. The anasarca in this case was got the better of, and the ascites diminished very much.

Now what remained to be done was, to endeavour to soften, and cause absorption of, the enlarged liver. A liver as large as this, one can hardly expect to be reduced, at least not for a great length of time; and if the patient's strength be not sufficient for life to continue for some months, of course there can be no time to effect it. I began to treat this woman with the hydriodate of potash, about seven grains, plentifully diluted, three times a day,—seven grains and a half. She took a drachm of the solution, which contained a drachm of hydriodate to one ounce of water; therefore the eighth part of a drachm. She also rubbed in the ointment of the hydriodate of potash, which consists of a drachm or two drachms, just accordingly as the skin will bear it or not, to one ounce of grease. This was begun as soon as I found the symptoms were sufficiently diminished to make it unnecessary to purge her and give her simple diuretics longer, and as soon as I found the liver was in a state of induration. I began this treatment on the 31st of January, but her strength gradually diminished. She was pale and weak when she came in, and she sank slowly, and died on the 23rd of the month, so that there were only 23 days to take this remedy; and indeed she did not take it for the last two days, on account of the pain in the abdomen returning. The peritoneum was very much diseased, and liable to attacks of inflammation from time to time. That was what had occurred when she came in, and now it again took place. The abdomen became tender and she sank.

Post-mortem Examination.—On opening her there was found a sufficient cause for the exhaustion of her powers,—not in the enlargement of the liver merely, but of the kidneys, and perhaps of the spleen. I think I may say that the liver, the kidneys, and the spleen, were all in a state of hypertrophy. Enlargement of this description, and also induration, of course must have impaired the strength very much. Had there been merely an enlarged liver, her strength might have borne up for some time; but as there was so much morbid process going on in so many parts at once, her strength was gradually exhausted. When the pain was increased in the abdomen, the secretions in the peritoneum augmented again, so that we found several pints of water in her at her death. This was the result of the peritonitis which again occurred before her death. Her strength was so impaired, that no strong means could be adopted to recover them from this attack but a blister.

On opening the alimentary canal, all was found perfectly sound. I mention this, because she had been taking the hydriodate of potash. She never took any large quantity. The utmost that she took was three drachms of the solution, making rather more than a scruple, in the twenty-four hours. The disease was gradually decreasing, but signs of peritonitis came on, and her strength sank, so that there was no time to increase it further, and the medicine was altogether given in too small a quantity, and for too short a time, to do good; but when we consider what an extensive mass of hypertrophy there was, I very much doubt whether it would have done good had her strength permitted us to persevere in its use. The inner surface of the stomach, however, and also of the intestines, was not only quite free from ulceration, but there were no marks of irritation, there was no redness, no great vascularity. On the other hand they were pale, as we often see them in dropsical people. No one could have discovered the difference between her stomach and intestines, and the same parts in many a healthy person.

The vomiting which took place before she died was of the same description as that which occurred when she came in, and arose from an inflammatory state of the peritoneum. The peritoneum, as you see in this specimen, had been in a severe old inflammatory state, there being lymph effused on the surface of the liver. You see that the peritoneal coat of the organ had become quite white from the effusion of lymph, and you see that there are long strings, bands, and adhesions, on the surface. You observe these marks on the concave, as well as on the convex, surface. The liver altogether is of a very large size, but not larger than you might have expected, because it could be accurately felt during life. I do not see, generally speaking, any disease of the organs, except an increased growth and hardness. There is no formation of tubercles, no new deposition of any sort, but the substance of them has become very hard. It is hardly worth while for you to dirty your fingers to feel it, but you perceive that it is hard throughout, from the difficulty with which I make the least indentation upon it. It is so exceedingly hard, that it is almost impossible to make an indentation. On making this free section, you see that the peritoneal coat has become thickened, that it is much thicker than it should be.

The liver itself is very red and firm, but not at all unnatural in its composition. It is a simple case of hypertrophy, just as you observe in instances of the heart. When the heart is hypertrophied, for the most part you see no new deposit, no alteration of structure, except that it becomes larger and thicker than it should be, and, generally speaking, firmer also than it should be. Or, in the case of the heart if the cavity be dilated, there is frequently great additional substance without any increase of the thickness of its parietes, because, being dilated, it would grow thinner if there were no increased deposition. This is simply a case of *overgrown* liver. It is the red part that is chiefly hypertrophied, and you perceive it is exceedingly red, except in one or two parts near the surface, where it is rather paler than it ought to be, but this part is of very small extent.

This is very different from the liver of gin-drinkers. A gin-drinker's liver is generally very hard, but it contains minute granules which are sometimes called tubercles, although the term tubercle is now restricted to a scrofulous tubercle, these minute granules being of a yellow colour, and they are either a new deposit, or hypertrophy of the secreting portion of the liver,—of the biliary portion rather; and the liver, too, is frequently hard, and smaller than it should be,—and not, as I have just said, of its ordinary structure. The case before us, however, I have no hesitation in saying, did not arise from dram-drinking. It has no appearance of that. The liver is frequently altered in this way in women. Sometimes it is very pale, greasy, and rather soft. In females, frequently, without a cause which we are able to discover, the liver will grow to a large size. I have seen it reach into the pelvis in persons who never drank drams, and appeared exempt from the causes of hepatic disease,—who were living quietly in the country, and pursuing the best possible habits. In men disease of the liver far more frequently arises from drinking than not, but in women I see it, far more frequently than not, without a cause that I am able to discover, and frequently too it will arise in children; often in female children. I hardly know whether more girls are affected than boys, but it will often grow in women without any apparent cause; whereas in men, in eight cases out of ten, it arises from drinking.

The spleen is likewise very large; but if we had only seen the spleen enlarged, and no other organ, we should have passed it over, and merely said there was a very large spleen; but when we consider how large the kidney is, we are justified in considering all these organs to be hypertro- still very firm, notwithstanding it has been phied. This great increase in size but not disease in structure exists then. The spleen is taken out of the body, and a section has been made into it some days. The peritoneal coat of it is diseased you see. Here it is exceedingly dense, but the structure is natural; that is to say, there is no new deposit or transformation, and altogether it is very large. We should say this was a very large spleen. When we see the quantity of inflammation of the peritoneal coat outside, the inflammation being evidently the result of a chronic disease, and there being other organs hypertrophied, I have no doubt that originally the spleen was of the common size, and that it has grown to its present magnitude by disease. You perceive that there is a loose spleen hanging to it, and I believe there were one or two others, so that there was multiplication as well as magnification. Then, if you turn to the kidneys, you will see that they are quite healthy, or nearly so, in appearance, but very large in size, and firm. This, which I now show you, is a very large kidney. You seldom see a kidney of this length, and the other, though not so long, is nevertheless a large kidney, and both are exceedingly hard, even now, though they have been laid open and exposed to the air for three days. Here is the cortical part paler in some places than it should be, but altogether I do not know that they are otherwise unhealthy kidneys.

Now here is the heart, and this also may be in a state of hypertrophy. The walls of the left ventricle are thicker than you generally see them, you observe that the cavity is very small, but you observe that the outer part of the heart is exceedingly small. It is a very little heart. Under the circumstances, I should not feel justified in saying that the left ventricle was in a state of hypertrophy, but had the heart been of its usual size externally, I should have said that it was hypertrophied. When you see the heart smaller than it should be outside, I think it likely that the thickness of the walls is merely the result of excessive contraction of the ventricle. The walls being muscular, if they are contracted they will become thick, just as is the case with other muscles; and when you see that the heart is so small on the outside, it would appear that a great contraction of the walls of the ventricle has taken place, reduced its external size, and produced a great thickness within. Upon the whole I think the increased thickness is nothing more than commensurate with the diminished bulk of the organ externally. It may be hypertrophied, but I do not know that it is, and she had no symptom of hypertrophy during life. You will find my view of this point in the work I have published on the diseases of the heart. There was also some disease of the ovary, which I have not yet examined. The uterus appears healthy, and of the usual size. The right ovary is in a state of dropsy. You observe a cyst which appears to be the right ovary. Here is the fallopian tube. Here is the round ligament, and you see the broad ligament. The fallopian tube has grown to the ovary. It has no open extremity. Even if she had not had dropsy of the ovary, the tube would have been of no use. This appearance is said to be very often seen in prostitutes. Here is the inner surface quite smooth. Here is another little encysted tumour which is sprouting forth. The tube would in time have filled and distended with its own secretion. The fimbriated extremity of the fallopian tube is, you perceive, quite adherent to the ovary, and the ovary itself is nothing more than a sac. Here is a third cyst. The ovary has become enlarged, the parietes thickened, and the cavity filled with fluid. The ovary at large appears wasted. You often see this circumstance in the kidney. If the pelvis becomes extremely distended with fluid, the substance of the kidney will at last waste away. The fallopian tube of the left side is also adherent in the same way as the other. You may perceive the tube by its tortuous course. The sides are completely bound down, which has doubtless arisen from inflammation going on to a great extent in that part of the peritoneum, as well as in the upper part over the liver and spleen. This woman would not have brought forth even on this side. She multiplied only in the spleen (*a laugh*), where you perceive there is still one little one. I believe there were two more. The coats of this ovary are very much thickened, but you see a cavity, and here are two vesicles.

This is certainly a very singular case, on account of displaying such a general disposition to hypertrophy in several viscera, occurring as it does in the liver, the spleen, and the kidneys. The case is rendered the more singular, by there being no other disease of structure. There are some diseases in which there is a disposition to hypertrophy of many parts. For instance, in rachitis, the ends of all the bones are disposed to enlarge; even the bones of the head, and in the disease called *cretinism*, there is also a disposition to hypertrophy of the bones of the head. Frequently, too, you observe in this complaint symptoms of hypertrophy of the heart, tongue, &c. and at the same time of the thyroid gland. There are certain states of the system in which there is a great disposition to hypertrophy, but I do not recollect a similar instance to the one before me, of great hypertrophy and induration of several abdominal viscera, without any other change of structure, either transformation or new formation.

Respecting the hypertrophy, I have no doubt that it is of an inflammatory nature. The parts are all firmer than they should be, and they are very red; at least as red as they should be, and there are marks of inflammation on their external surfaces. There is no doubt that hypertrophy of the heart is, for the most part, an inflammatory disease. If you consider what extreme nourishment is, you must see that it must be inflammatory. If a part be nourished more than it should be, there must be great activity of the vessels, more activity of the circulation there than in health, and though it may not be really inflammation, yet it is allied to inflammation; it is an inflammatory state. You may dispute about the word inflammation, but this state certainly gives one the idea of an inflammatory state. I may mention, respecting hypertrophy of the heart, which is the more frequent occurrence, that it is found under the microscope that the fibres of the heart when hypertrophied are redder than they should be, if steeped in water, while a portion of another heart is steeped in other water, the hypertrophied heart reddens the water more than an equal slice from the healthy organ, showing that there is more blood in it, and when it is taken out of the water, still it is the redder of the two; after it has impregnated the water, and made it more red than the healthy heart, yet it is still the redder of the two. On putting it into boiling alcohol, it is found to contain less fatty substance; and besides this, in regard to hypertrophy of the heart, we generally find—more frequently than not,—that it begins as inflammation of the pericardium, and when it does not, it arises from over-exertion of the heart, and this perhaps through some obstruction to the course of the blood through it so that it acts more violently.

When hypertrophy has existed for some time, together with induration, still, at last, the part will become soft. There is no doubt about it, and no doubt sometimes you find the part which is hypertrophied not redder than it should be, but paler. As a general rule, however, when a part is overgrown, if there be no new deposition, if it have not overgrown from new deposition, and there be no change of structure, exactly as is the case here, and which is true hypertrophy, its natural structure remains, you find it only firmer and harder than it should be. But these two circumstances taken together, make it probable that the affection is of an inflammatory nature, and more particularly when you consider that in the outer surface of the heart, for example, and of the liver, the investing membrane generally shows decided marks of inflammation.

ST. GEORGE'S HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

DR. SEYMOUR,

March 27, 1832.

SCARLATINA.

I HAVE now, Gentlemen, a patient under my care with this complaint, and I shall therefore take the opportunity of making a few remarks to you upon the disease. Scarlatina, you know, is an epidemic and contagious complaint,—contagious so far that if two or three persons who have it are confined in the same room, and that room be not well aired or ventilated, a stranger coming into it would be very likely to catch the complaint; but in this hospital the wards are so well ventilated that we are not afraid to place a patient afflicted with scarlatina amongst the other persons in the ward, and we seldom or ever find any ill results occurring from it. The efflorescent or red appearance of the skin is not absolutely necessary to establish the fact of the disease. About the end of the second or beginning of the third day, the symptoms generally increase in violence, and the affection of the throat and fauces is often very severe; if you find the fauces presenting a darkened livid colour without any inflammation being present, you may regard it as a very dangerous symptom,—if the eruption appears in patches, is of a dark colour, and goes off in a few hours, this is a bad case of cynanche maligna. I would refer you to Dr. Fothergill's excellent work on this disease, or to a better author still, Dr. Willan. Such bad cases as those described by Dr. Willan are, however, rare; the case of which I am now speaking is comparatively moderate in its symptoms. Scarlatina has been epidemic in London for the last three months; after the third day you in general have desquamation of the cuticle; the danger is then all over. In young children you sometimes have effusion into the ventricles of the brain, and this I found to be very much the case in my private practice among children about three years ago. You must watch the symptoms of the disease in children very narrowly; for very often, when to all appearance the hour of danger is gone by, and the symptoms are assuming a favourable aspect, you will find effusion in the ventricles of the brain. Where the symptoms are mild you will generally find effusion into the ventricles, and *vice versa*. Effusion is very rare in adults; I once had a case, and the patient recovered her senses as she would have done if she had had an attack of mania; she was first aroused by the sound of the clock, and when she spoke a sentence she hummed over it and left out all the small words. In children you very often have effusion into the cellular membrane, and this you will find to be a very intractable form of the disease; I never saw a case of death from this, but I have seen some very bad cases of it. The occurrence of anasarca in scarlatina, is always to be looked to and guarded against; it is more commonly found as a symptom of the disease in children than in adults. In scarlatina the temperature of the skin is higher than in any other complaint, and in ordinary cases of the disease where you find the skin steadily hotter than natural, we will say at 104° or 106°, and where there is no sensation of cold, the best plan you can follow is to pop the patient at once into a tub of cold water, or put him into a shower-bath, and this you may do twice a day. Dr. Curry, in the Medical Reports, speaks very strongly in favour of the cold-bath in this complaint, and the late Dr. Gregory, of Edinburgh, speaks of the case of his own child who was seized with scarlatina, when the worthy doctor immediately popped the child into a cistern of cold water. The whole account is well worth your perusal, as well for the terse, quaint, yet energetic style in which the doctor writes, as for the professional information which it will give you. The treatment with cold affusion and keeping the bowels open is in general all that is required in the ordinary cases of this disease, and when the patient has recovered give him one or two brisk aperients.

If the heat of the skin is not very great you will in general find the affection of the throat worse, and if the fauces are very much inflamed or ulcerated, you must give gargles, but not stimulating ones, as they will only aggravate, and not remove, the irritability. Emollient gargles should be used, such as are soft and warm to the fauces,—such as warm water, the decoctum hordie compositum, or inhaling the vapour of hot water and vinegar, or holding hot bread and milk in the mouth; each or any of these you will find of use, and with this the bowels should of course be kept open. If you have an opportunity of seeing the case in its early stage, when the throat is inflamed, you will find emetics of great benefit in altering and cutting short the malignant type of the disease, and they also tend greatly to promote an increased action of the absorbents. If the inflammatory symptoms are high and the pulse is full, leeches may be applied to the throat, and hot poultices afterwards, and the leech-bites may bleed into these. You will say perhaps that it is very difficult to keep the bowels open where the patient is not able to swallow, but if you put calomelanos gr. iij on the tongue twice a day, it will surely find its way down into the stomach and keep the bowels open. If the patient sinks, you must give him wine and other stimulants to rouse him. If you have a very bad case, you will find your faith in the antiphlogistic plan of treatment very much shaken. I once saw a very bad case at Lambeth, and I ordered *sulphatis quininæ*, gr. iij, *tertiis horis*, and wine; this plan of treatment was persevered in steadily for three days, and the patient eventually got well. In some very bad cases if you add a little ammonia to the gargle, it will operate as an emetic, without doing the patient any injury. In a paper which I once read, written I believe by a surgeon at Gainsborough, ammonia it appears was given in scruple doses of the carbonate every two or three hours. The patients all recovered, and that very soon too. The ammonia can certainly do no harm, but it is strange how it can be swallowed in such an inflamed and irritated state of the fauces.

HEPATISATION OF THE LUNGS.

This, Gentlemen, is in general the consequence of long-continued inflammation of the substance of the lungs,—when lymph is poured out into the interstitial structure—thickening and altering the structure of the lungs, which effect is termed hepatisation. The consequence of all this disorganisation of parts is that the blood does not circulate freely through the lungs; it is therefore thrown back upon the heart, threatening the patient with suffocation. The results of inflammation are different according to the different structures which it attacks. Pus may be formed in the cellular membrane. Lymph may be poured out in the liver or lungs, but pus may also be effused in the lungs, and when you cut through them puriform matter escapes; in some cases you find abscesses formed in the structure of the lungs, but these, as the result of inflammation, are comparatively rare. In this case of hepatisation of the lungs, we have the second stage of inflammatory action present, and in such a case as this the main object of your treatment should be to draw off the current of circulating fluid in some measure, and to promote free expectoration. Here in this case I ordered venesection, and after that mercury to promote absorption; he was blistered and bled a second time, the blood still showing the buffy inflammatory coat; but the mercury had not then begun to affect the mouth. When he had been in some measure lowered by this plan of treatment, I gave him the digitalis with the tincture of cantharides. The digitalis will act as an excellent diuretic, but only in low and languid states of the system. This case shows how much may be done in a bad case of inflammation of the lungs,—subduing it by other methods than those usually adopted or laid down in books.

INFLAMMATION OF THE LUNGS WITH HÆMOPTYSIS.

This is not a case in which the hæmoptysis arises from the bursting of a tubercle in the lungs, but I believe it possible that the man may have tubercles which, combined with the inflammation, may impede the circulation of blood through the lungs, causing hæmoptysis. Whenever you find a case in which blood is brought up by expectoration, it ought immediately to awaken your attention, for this is frequently I assure you one of the very first symptoms of phthisis pulmonalis, although Dr. Cullen in his Nosology places it as one of the after symptoms of this disease. Well, in a case of this kind you examine the throat, and find the velum pendulum palati relaxed, and of a blueish red colour. Nothing satisfies patients sooner, than to tell them that their complaint proceeds from the throat; but you must look further than this, you must examine him closely, and if you find that his habit of body predisposes him to phthisis, or that any of his family have died of the disease, or that the hæmoptysis has burst out from the breaking of the cellular membrane of the lungs, owing to the violence of the circulation,—you may in your own mind set it down that he will have phthisis. Where you suspect tubercles, and in phthisis at all, be very careful how you give mercury, as it tends greatly to promote suppuration, and it increases bodily weakness and debility. In old persons you will frequently find after an attack of bronchitis, that there is a dull heavy pain diffused over the region of the chest; this is termed peripneumonia notha, or bastard peripneumony. Here you may give the liquor ammon. acet. with the vinum ipecacuanhæ, but you must not bleed; if the patient wastes, give him the balsam of Peru; if after the inflammation has gone off the secretion from the chest remains very great, he must inhale stimulants over the inflamed parts, and take the medicinal gums, of which the flores benzoes is one of the best I know.

In these clinical lectures, Gentlemen, I do not pretend to lay down to you any new rules of practice, but to acquaint you with those remedies, and those fully-approved plans of treatment, which will be most serviceable to you in your own private practice; for it is not he who attends most closely to the physiology and pathology of disease who succeeds the best; but he who has his remedies and his modes of treatment most ready for removing and alleviating those symptoms which he is called upon to relieve.

Breast-milk in Cholera.—It is a curious and interesting circumstance, that the secretion of milk is not necessarily checked in women in the course of the malignant cholera. The breasts may at the time of death be gorged. Mr. Frost mentioned to me a woman whose child was nursed a short time before her death; and Mr. John Fife met with a similar instance.—*Greenhow on Cholera*

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXIV.

THE STRUCTURE OF THE LUNGS—THE PULMONARY CAPILLARY CIRCULATION—THE CHANGE EFFECTED IN THE BLOOD—CATARRH IN THE HORSE, OX, AND DOG—CHRONIC COUGH.

Of the intimate structure of the lungs I have little to say to you, and that little far from satisfactory. They are the seat of the pulmonary circulation. They convey through their comparatively little bulk the blood, and all the fluids beside scarcely become blood, or soon separated from it, which traverse the whole of the frame. Then it is evident that they must contain innumerable blood-vessels, or, rather, that they must, in addition to the ramifications of the air-tubes, consist of a mass or congeries of blood-vessels, or the balance or equilibrium of the circulation could not be preserved. They do so: the only account which I can give you of the structure of the lungs is, that they consist of countless ramifications of air-tubes and blood-vessels connected together by intervening cellular substance.

In order to examine them, however, as minutely as I can, I take the lungs of an ox; an animal in which they are considerably developed, and in which, from his quiet regular way of living, they are generally not much disorganised. If you compare these specimens, taken from the horse, the dog, and the ox, you will immediately perceive how much more perfectly their structure is exhibited in the last, and for the reason that I have stated.

The Divisions of the Lungs.—The lungs form two distinct bodies, the right somewhat larger than the left; and they are divided from each other by that duplicature of the pleuræ which has been already described—the mediastinum. Each lung has the same structure, properties, and uses. Each of them is subdivided; the right lung consists of five lobes, the left of two. These divisions are different in our different patients, but for what physiological reason I am unable to tell. In the horse the right lung consists of three lobes, and the left of two; in the dog the right has five lobes and the left two, and these lobes are far more perfectly separated from each other than in the horse or the ox, for the separation commences even from the bronchial tubes. In the swine there are three lobes in the right side, or we may perhaps say that the right lung of the animal is divided into a great many lobes, for the smaller of the three is partially separated into numerous lobules, differing in number in different swine. The left lung consists of two lobes only, and the scissure between them is not very deep. The variations then have reference to the right lung—the left, in all our patients, has two divisions only. The principal intention of these divisions is probably to adapt the substance of the lungs to the form of the cavity in which they are placed, and to enable them more perfectly to occupy and fill the thorax.

The External Form of the Lungs.—The lungs are suspended from the spine by means of their blood-vessels, the bifurcation of the bronchiæ, and the mediastinum. Their figure is irregular, elongated, flattened, pyramidical; broad and concave posteriorly where they are in contact with the convex surface of the diaphragm, and narrower and approaching to a point anteriorly, where the cavity of the thorax narrows. We recognise in each three surfaces, the costal, the diaphragmatic, and the mediastinal. The convexity of the costal accommodates itself to the concavity of the ribs; the concavity of the diaphragmatic surface answers to the convexity of the diaphragm, and the mediastinum adopts itself to the irregular surface to which it is exposed. The rounded borders, superiorly, fill the longitudinal cavity or sulcus on each side of the dorsal vertebræ; the narrower borders inferiorly bend towards each other, and enclose the apex of the heart; the posterior borders follow the curvature of the diaphragm, and constitute the base of the lungs. The lungs are enveloped, as I have already described, by duplicatures of the pleuræ.

The Internal Structure.—We cut into one of the lobes, and we find it separated into numerous irregularly-formed compartments, to which anatomists have given the name of *lobules* or little lobes. They are separated from each other by cellular membrane. They are distinct from each other; and they are impervious, except through the medium of the air-tubes and blood-vessels which pass into and through them. We examine them more closely, and we can subdivide them, and almost without end; we have smaller lobuli, each surrounded by cellular membrane, each distinct from and having no communication with its fellows. The consideration of some of the diseases of the lungs, and particularly *broken wind*, will convince us of the independence of, or want of communication between, these lobuli. Broken wind is the unnatural running together of several of them.

The Minute Lobuli.—To these lobuli we have traced the ramifications of the bronchial tubes, but no one has demonstrated their entrance into them, nor the actual structure of these minute and ultimate subdivisions of the substance of the lungs. There has been many an ingenious theory about it, and we have been favoured with one or two very pretty diagrams of the bunch-of-grape-like terminations of the air-tubes. They probably have some such termination, but we have no ocular demonstration of it.

The Ramifications of the Blood-vessels.—An account of the blood-vessels of the lungs belongs to the circulatory system, but I may say at present that the pulmonary artery (artery we term it, for it proceeds from the heart, although it discharges the office of a vein by carrying black blood) arises from the right ventricle of the heart; it winds towards the root of the left lung, and there, at the bifurcation of the trachea, divides into two trunks, one of which penetrates each of the lungs. It very soon subdivides, following the divisions of the bronchiæ; it multiplies its ramifications as these tubes are multiplied; and it may be traced in company with them to the lobules which I have just described. Some successful injections have enabled us to trace them further, for the fluid has returned by the pulmonary veins. It has traversed these lobules, and has been conveyed back again towards the heart. We do, in point of fact, find other vessels of different structure returning blood from the lobules. We call them veins because they go to the heart, but they discharge the function of arteries by carrying red blood. They are minute at first; but they anastomose with and fall into each other until they assume a decided character, and constitute the branches and the trunk of the pulmonary veins which empty themselves into the left auricle of the heart.

The Alteration of the Blood in the Lungs.—Gentlemen, I here feel the inconvenience of the plan, which, for the accommodation of some of you who had attended my lectures on the sensorial and circulatory systems, I have in the present session adopted. Many of you have heard nothing of the mechanism of the circulation of the blood, nor the changes which are effected in that fluid, as it traverses certain vessels or certain parts, and therefore I am reluctantly compelled to confine myself to the simple statement, that the change produced as the blood circulates through the capillaries of the lungs, is precisely the reverse of that which takes place in the capillaries of the other parts of the frame. In the general capillary system, the blood is changed from red to black, from arterial to venous, from vital to effete. In the capillary system of the lungs, the blood is changed from black to red; it is enabled again to furnish the materials for the various secretions; it conveys the principle of nutrition to every organ, and it also communicates to each organ that impulse or stimulus which enables it to discharge its function. This all-important change is effected through the medium of the atmospheric air, for we can trace it along the bronchiæ to these lobules, and we have no doubt that it enters them. The delicate membranes forming the parietes of the blood-vessels and the lining, or the wall of these lobuli, although impervious to the air in its compound form, suffer some of its principles to permeate them, and especially when urged by the strong power of affinity; and the oxygen of the atmosphere attracts and combines with a portion of the superabundant carbon of the blood, and the expired air is poisoned with carbonic acid gas; while some of the constituents of the blood attract another portion of the oxygen of the air, which combines with that fluid, and probably gives its distinguishing character and properties as arterial blood.

This, until we have inquired, as we should before have done, and in future will do, into the nature and properties of the blood, is all that I say of the changes produced by the process of respiration.

The Arterial and Venous Circulations.—Then, Gentlemen, it is evident that the circulation of arterial blood has its commencement in the capillaries of the lungs, and its termination in those of the frame generally. The venous blood begins to flow in the general capillaries, and terminates in the capillaries of the lungs. These systems are independent of each other; the fluid is essentially different; the vessels by which it is conveyed are different; but they communicate at these points, and, as I shall have occasion to show, the capillaries (too much neglected in our dissections and our inquiries) are by far the most important portions of the circulation. They are the origin and the termination of the two circulations, and in them the most important phenomena of life take place,—the change from arterial to venous, and from venous to arterial blood.

The fact of the whole of the Blood traversing the Limbs accounted for.—Does it appear singular that the lungs alone should counterbalance the circulation of the whole body? Let it be remembered, that while the lungs consist of a congeries of blood-vessels, or are made up of them, this fluid is distributed very sparingly to many parts of the frame. The cellular membrane, and the ligaments and tendons, have not much of it, and the bones have still less; while the integument has scarcely any. This will materially lessen the comparative quantity. In the frame there are various circumstances of retardation; the vessels take a tortuous course—they run in opposite directions—they wander through many a gland, and the blood is purposely delayed for the accomplishment of many an important function; but the vessels of the lungs take a direct course; there is nothing to divert or to retard the current, so that a considerable portion of the blood which they contain enters the heart at each dilatation of the auricle, while a far smaller proportion of the blood of the frame generally is then received. That which is deficient in bulk is fully supplied by the directness and shortness of the course, and the rapidity of the current; beside, the lungs are close to the heart; all its power is felt by them, while the blood has begun to loiter in its course before it reaches many of the distant portions of the frame.

Transmitting the whole of the Blood, the Lungs are easily inflamed and disorganised.—If, however, it be true that the pulmonary circulation counterbalances that of the whole frame beside, and even of that portion of itself which belongs to the general capillary system, we shall be enabled to account for the prevalence of disease in this organ above any other, and especially in that shamefully-abused animal the horse. When we consider the extra labour of these vessels, which is so often required, to supply the increased consumption of arterialised blood in rapid progression; the irritation to which the membrane of the bronchial tubes is in some measure exposed, notwithstanding the guard of the larynx and the Schneiderian membrane, from contact for so many hours, with the irritating and empoisoned air of a filthy stable; and more especially when we regard all the sympathies which such an organ must have with every other, and particularly the sympathy which daily experience tells us it has with the functions of the skin, and the discharge of the perspiration, sensible and insensible, can it be wondered at that it is so subject to inflammation, so difficult to subdue, and often so fatal! Inflammation is increased action of the capillary vessels; this organ is made up of them, and they become engorged, and the substance of the lung increases under inflammation with a rapidity unequalled in any other part. The walls of these vessels are exceedingly delicate; they are of a gossamer structure, in order to admit of the escape of the carbon, and the imbibition of the oxygen, and when they are distended with blood, it is scarcely conceivable how soon it begins to filter through them. We hear it in the very act of filtering, if we apply our ears to the side of the animal; and it penetrates into the interstices of the cellular tissue, and the whole substance is gorged and infiltrated. These considerations sufficiently explain the frequency and obstinacy and fatal termination of thoracic inflammation in the horse, and prepare us for that which, leaving for a while our anatomical detail, and our necessarily imperfect physiological inquiries, next presents itself to notice, namely, the diseases of these portions of the respiratory apparatus.

CATARRH IN THE HORSE.

I have already directed your attention to inflammation of the Schneiderian membrane, or that which lines the nasal cavity. Coryza is attended by a slight defluxion from the nose, now and then a yet slighter weeping, and some increased labour of breathing, from the uneasiness which the animal experiences from the passage of the air over the naturally sensitive, and now more than usually irritable surface, and from the air-passage being diminished by the thickening of the membrane; but when this is a simply local inflammation, attended by no loss of appetite, or increased animal temperature, it may speedily pass over.

Symptoms of Catarrh.—In many cases, however, the inflammation of a membrane naturally so sensitive, and rendered so morbidly irritable by our absurd treatment, rapidly spreads, and involves the fauces, the lymphatic, and some of the salivary glands, the lymphatic glands of the lower jaw, and the throat, and the parotid gland, and also the membrane of the larynx. We have then increased discharge from the nose, greater redness of the pituitary membrane, more defluxion from the eyes, and loss of appetite from a degree of fever associating itself with the local affection, or apparent loss of appetite from pain in swallowing, and if the animal feels this he will never eat. Cough now appears more or less frequent or painful, referable generally to the upper respiratory passages; but no great degree of fever or acceleration of the pulse, or heaving of the flanks; there is, however, more or less defluxion from the eyes.

Causes of Catarrh.—Catarrh may arise from a thousand causes. Membranes subjected to so many sources of irritation soon become irritable. Exposure to cold or rain, change of stable, change of weather, neglect of grooming, change of the slightest portion of clothing,—many circumstances apparently trifling, and which they who are unaccustomed to horses would think could not possibly produce injurious effect, are the causes of catarrh. When there is anything about the animal that favours this general febrile action or local determination, catarrh can scarcely be avoided. In the spring of the year, and while moulting, almost every young horse has a cough, and in the dealers' stables, where the process of making up the horses for sale is carrying on, there is scarcely one of them that does not cough.

Treatment.—In the majority of cases, a few warm mashes, warm clothing, and a *warm stable*, a fever-ball or two, with a drachm of aloes in each, and a little antimony at night (in almost any of its combinations, but of which the antimonial powder is probably the best), will usually set all right. Indeed all would soon be right without any of our medicine, and much more speedily and perfectly, than if we had foolishly given those cordials of which grooms and farriers are so fond.

Caution.—We are now approaching, Gentlemen, a class of diseases in the treatment of which a great deal of tact is required. You would do very wrong if you treated every case of common cold in this cavalier way; and you would be equally reprehensible, and get into sad disrepute with your employers, if you made a serious business of these every-day complaints. Your duty to your employer will lead you to adopt every proper measure to prevent the malady from increasing, and to restore health as speedily and with as little expense as you can; and, on the other hand, the duty which you owe to yourselves, the preservation of your professional reputation, will urge you, while you do not make an unnecessary or palpable job of the case, yet never to hazard the occurrence of a worse disease by apparent or real negligence. Nineteen horses out of twenty with common catarrh will do well, but in the twentieth case a neglected cough may be the precursor of bronchitis, pneumonia, and death. Then, although you may not do much, you will always ponder the case well. If there is no acceleration of pulse, no heat of mouth, or redness of nasal membranes, or soreness of throat, or loss of appetite, you may tread the ground lightly, especially if the owner is a niggard. But you must make yourself sure of all this; you must remember with what kind of animal you have to do, and to what a sad degree he may be predisposed to serious thoracic affection. You must recollect, and experience will soon convince you of this, how insidiously these chest complaints creep on,—how frequently inflammation is established before you are aware of its existence. If there is the least fever, bleed. The surgeon would never be justified who suffered a common cold attended with heat of mouth, or indisposition to food, to pass without the abstraction of blood. You need not make your patient quite a sick horse, but you should be always on your guard.

Caution against Physic.—Never give a physic-ball in catarrh. Let no wish of the master or the groom induce you to do it. You know not what sympathy may exist between the portion of membrane already affected, and the mucous membrane generally; and I shall have again and again to prove to you that in severe thoracic affection, or in that which may soon become so, a dose of physic would be a dose of poison. There are few more fatal, and I am sorry to say more frequent instances of mal-practice than this. Thousands of horses are lost by physicking under catarrh. Some of my young friends saw with me a case a few days ago in which a horse was murdered by the administration of a dose of physic under what appeared to be common cold.

Treatment resumed.—If, however, careful investigation assures us that there is no affection of the lungs,—that the disease has not proceeded beyond the fauces, we may with advantage unite *small* doses of aloes to our nitre, emetic tartar, and digitalis. To evacuate the intestinal canal, and to reduce the fæcal discharge to a pultaceous form, must ever be beneficial, if we so adjust our doses as to stop there; but if we proceed one step further dangers environ us.

Sore-Throat.—If catarrh is accompanied by sore throat; if the parotids should enlarge and become tender (there are no tonsils, *amygdalæ*, in the horse), or if the submaxillary glands should be inflamed and tender, and the horse should quid his food and gulp his water, this will be an additional reason for bleeding, and also for warm clothing, and a comfortable stable. I do not mean a hot stable, in which the foul air is breathed over and over again, but a temperature somewhat above that of the external air, and where that determination to the skin and increased action of the exhalent vessels of the integument may take place, which in these cases are so desirable.

Liniments.—Some stimulating liniment may be applied over the inflamed gland, consisting of the turpentine tincture of cantharides diluted with five times its quantity of spermaceti or neat's-foot oil; strong enough to produce considerable irritation on the skin, but not to blister, or to destroy the hair. An embrocation sufficiently powerful, and yet that never destroys the hair, consists of equal parts of hartshorn, oil of turpentine, and camphorated spirit, with a sixth part of laudanum.

CATARRH OR HOOSE IN CATTLE.

Cattle, and particularly cows, are very subject to catarrh. They are so when most judiciously treated, and especially those that are highly bred, and bred for several generations *in and in*. This produces a delicacy of frame and constitution, which renders them unable to struggle with any kind of hardship. When cattle are crowded together they are seldom without hoose. I have seen the vapour produced by their perspiration steaming from them hour after hour; the cow-house has been heated many a degree above the surrounding air; but this has been neglected and scarcely observed, because the fæces of this animal have no offensive smell, and the urine is comparatively devoid of ammonia. The cows are then let out for an hour or two to grass, or to water, when they inevitably take cold.

The negligence of the owner.—I know not any disease by which cattle are so seriously injured, or which is eventually so fatal to them, as hoose or cough, yet neither the herdsman nor the owner pays any attention to it. The animal may cough from week to week, and no one takes notice of it, until the quantity of milk is seriously decreasing or the cow rapidly losing flesh, and then medical treatment will generally be unavailing. The disease has reached the chest, the lungs are seriously affected, and the foundation is laid for confirmed hoose or phthisis.

Hoose often caught at straw-yard.—Many a sad and eventually fatal cold is taken at straw-yard, and particularly by young cattle. The food is scanty—not sufficient to afford proper nourishment, or to keep up the natural warmth, and the more forward drive the others about, and scarcely permit them to obtain a fourth part of their share. The depressing effects of cold and wet, and hunger and fear, so debilitate these poor beasts, that the slightest catarrh fastens upon them, and produces serious and often irremediable pulmonary affection.

Treatment. — You would bleed here if there is the slightest degree of fever. You would remember the inflammatory character which almost every complaint speedily assumes in these animals, and the uniform violence of the fever if it is once suffered to get head. The horse is rarely subject to any disease, which in the speed with which it runs its course, and its frequent fatality, bears the slightest resemblance to the inflammatory fever of cattle.

Having bled, you would certainly physic, for you have an aperient which, although comparatively inert in the horse, is certain and mild and safe in its operation on cattle, I mean the Epsom salts. The medium dose is a pound. Fever medicine would follow the action of the purgative, composed of the same drugs, but administered in half the quantity; and to this should be added warmth, warm mashes, warm drinks, warm gruels, and a warm but well-ventilated cow-house.

CATARRH IN THE DOG.

Dogs and especially those that are petted, are subject to frequent cough and catarrh. The disease assumes according to circumstances two very different forms, requiring a material difference of treatment. Sometimes there is a husky cough, not to so great a degree as in distemper, but followed by the same apparent effort to get something from the throat, or the same attempt to vomit, and the ejection of a small quantity of mucus, either simply frothy or discoloured with bile. This proceeds from obstruction of some of the air passages, and oftenest of the superior ones. An emetic will clear the fauces, and the violent pressure on the thoracic viscera will force out a portion of the spume which clogs the bronchial tubes. Cough of this kind, and attended, as is generally the case, by little fever, requires nothing more for its cure than the exhibition of a few mild emetics; and the emetic for the dog, surest and safest in its operation, consists of equal portions of calomel and emetic tartar, and given in doses varying from one grain to one grain and a half of each.

The harsh hollow cough is attended by more inflammatory action. Emetics are useless here. The depleting system must be pursued. This cough, so loud and so harsh, will yield only to the lancet and purgatives, assisted by sedative medicines composed of nitre, antimonial powder, and digitalis. An opiate (the syrup of white poppies) given at night will often be useful.

CHRONIC COUGH.

Cough in all our patients becomes occasionally chronic. This is the occasional sequela of simple catarrh, but much oftener of an acute disease of some portion of the respiratory apparatus; it is especially the consequence of pneumonia and bronchitis. It continues weeks and months and years, or even during the life of the animal; and it is aggravated by every change of weather, and exposure to cold. It is sometimes connected with other diseases, as worms, but oftener it is found when the animal is otherwise in perfect health and full condition, and the breathing undisturbed. It is heard when he first begins to stir in the morning—when he drinks, or when he is first taken from the stable into the cold air. Weeks and months pass, and it does not seem to be followed by any bad consequence; but it indicates irritability somewhere—a predisposition to take on inflammatory action,—and, if the animal is closely watched, the cough will be evidently aggravated, and even serious inflammation of some portion of the respiratory tubes will be produced by causes which would not affect another horse. It is often a nuisance to the rider, and it is extremely unpleasant in the stable.

Treatment.—How shall we get rid of it? Ay, there is the rub. It is unaffected by the means which we should adopt in common catarrh; it is one of those chronic irritations over which depletion has no power.

We must endeavour to get at the history of the case—we must find out the seat of the previous disease—we must particularly observe whether the wind is perfectly sound—whether the cough is aggravated by sudden exertion—whether pain is felt when we press on any part of the trachea or larynx.

If we can fix upon the part there may be some hope. An external stimulus, applied in good earnest, may act as a counter-irritant. If the seat of the complaint is obscure, we have little to guide our treatment, and the result will probably be unsatisfactory. We may however attend to the diet. We may feed well, but not too well. These chronic local irritations will sometimes cease when we can produce a healthy state of the constitution generally; but debility may in different ways, yet with almost equal power, increase the complaint. Carrots, a mild and nutritious food, and particularly salutary in affections of the respiratory organs, may be tried, or a six weeks' run at grass in the spring, a better, far better alterative than our whole Pharmacopœia affords.

If these fail, medicine will be generally thrown away. I have however seen good effect from half doses of the common fever-ball made up with green Barbadoes naphtha. I am far from regarding this mineral production as the panacea which some have represented it to be, but I have seen it, and not unfrequently, useful here.

Chronic Cough in Cattle.—Chronic hoose in cattle is a serious complaint. The beast that labours under it rarely fattens kindly, and it is too frequently the foundation of pulmonary consumption. This disease is principally or almost always referable to the lungs, and mostly to the bronchial passages in these animals, and should be treated with half doses of the medicine that was recommended for catarrh, and combined with the naphtha. The case should not be abandoned while the affection is confined to the lungs, but when other organs begin to be involved, and particularly when dysentery accompanies chronic hoose, all hope is fled.

ON THE

MODE OF ACTION

OF THE

PNEUMOGASTRIC NERVE.

By James Carson, *Jun., M.B.T.C.D., Liverpool.*

A memoir having the above title was published by MM. Breschet and Edwards, in the 7th volume of the *Archives Générales de Médecine.* In their paper these physiologists confined themselves to the investigation of the influence of the pneumogastric nerve on the function of digestion. By a series of well-arranged experiments, they were enabled to reconcile the discrepant effects observed, on one side, by Blainville, Philip, and Hastings, and on the other by Magendie, of the section of this nerve. In admitting the restoration of the injured function when a stream of galvanism was directed through the nerves, they at the same time showed that the same restoration was effected by its mechanical irritation. And in depriving the nerve of its supposed chemical influence in digestion, they have proved that it aids that function, merely by influencing the muscular motion of the stomach by which the food is brought in contact by renewed surfaces, with the digesting surfaces of that viscus. Analogy would lead us to suppose that the function of this nerve as it is distributed to the lung was equally simple. That it is not a chemical influence which is exerted by it in the changes which take place in the blood and the air, may be argued from the fact, that the fibrils of this nerve are distributed over the whole of the bronchial tubes, whereas these changes are admitted to be effected in the cells or the more minute tubes alone. In the following paper a part of the mechanism of respiration will be pointed out, I believe not hitherto noticed, over which this nerve presides, and whose derangements are sufficient to account for the serious results of its section or lesion.

In the splendid work which Russessen has published on the minute anatomy of the lungs, he has depicted a series of muscular fibres investing the bronchial tubes. These he has represented as not only existing in the larger tubes, but also extending to the more minute bronchiæ. In the mammalia, an evident provision is made for their contraction, in the deficiency which exists in the posterior parts of the cartilaginous rings of the larger bronchiæ. I shall assume their existence in the smaller tubes, depending on the authority of Russessen, and on one or two phenomena only explicable on this supposition. The function of these muscles has not been hitherto, I believe, satisfactorily explained. Some anatomists have argued against their existence from their utter uselessness, and others have merely admitted them to explain some of the phenomena of disease. The following appears to me the proper explanation of the part which they perform in the mechanism of respiration.

The cavity of the lungs into which the air enters may be divided into two parts. If the boundaries of these be not pointed out by their anatomical characters, a physiological distinction must be admitted between the tubes conveying the air and the cells in which the changes take place in that fluid. Around the tubes alone I should suppose the muscular fibres to exist. It is to them that the pneumogastric nerves are distributed.

In inspiration these muscles, in concert with the other respiratory muscles, contract. They produce the effect of diminishing the cavities of the tubes, while the diaphragm, intercostals, and other inspiratory muscles, increase the general capacity of the lungs. The result of these combined motions is a dilatation of the cells, and thus a greater surface of their lining membrane by which the air is changed is exposed to a greater quantity of air. The dilatation produced in the cells by this mechanism is greater than that which would be produced on the supposition that the tubes were inert, by the quantity by which the muscles diminish the tubes, together with the quantity by which the latter would have been increased in the general dilatation of the lungs. If we consider the great proportion which these tubes form of the capacity of the lungs, the effect produced cannot be inconsiderable. Whatever be the view of the mode by which the changes are induced in the blood by the air, the condition of parts in this way produced is undoubtedly the most favourable for effecting them. The mixture, moreover, of the fresh with the polluted air is more thoroughly effected by the mechanism in question. If the above theory be correct, I think it is not difficult to explain the function of the pneumogastric nerve in the lungs. Distributed to the muscular fibres of the bronchiæ through their contraction, it renews the air, and increases the surfaces of the cells by which it is exposed to the blood, performing an analogous function in respiration to that which MM. Breschet and Edwards have shown to be performed by it in digestion. The lesions of this nerve will be attended with consequences proportioned to the importance of the functions over which it presides. Dr. Philip was so struck with the resemblance of the effects of its section to asthma, that he did not hesitate to attribute the spasmodic and continued asthma to a neurosis of this nerve. And if we consider that a constant attendant on these diseases is a severe fit of dyspepsia, we may recognise the lesion of the nerve in that of both the functions over which it presides. These diseases I should suppose to depend on paralysis of the muscular apparatus which I have above explained; and their difference, which is merely one of degree, depends on the greater or less extent of that paralysis. Cullen has attributed the phenomena of spasmodic asthma to a spasm of the bronchiæ, to which explanation Laennec has acceded, pointing out these muscles as its seat. In favour of this opinion Dr. Forbes, in his paper on Asthma, published in the "Cyclopædia of Medicine," has cited the stethoscopic phenomena. In the paroxysms of this disease the respiratory murmur is not distinguishable. This total absence of sound must depend, on the supposition of spasm, on a complete occlusion of the tubes in which the sound is formed, otherwise the diminution of these alone would be attended with much increased sound. But it is well known that the sufferers in this disease are conscious of the entrance of the air into the lung, and therefore the diminution of the tubes could not be complete. But suppose the disease to depend on paralysis of these muscles, then the air, instead of passing through diminished tubes, enters into a comparatively free space formed by the dilated tubes, and the resistance to its passage is so small as not to excite the least sound. The dyspnœa arises from the incomplete development of the surfaces of the cells, as explained in the former part of this paper.

There are two phenomena which I shall cite as proofs of the muscularity of the bronchiæ being, I believe, only explicable on that supposition; viz. puerile respiration and the active dilatation of the lungs observed by Laennec.

Puerile respiration is known to take place when a lung, or part of a lung, is rendered impervious to air. In the sound lung the respiratory murmur increases to such a degree as to resemble the sound of breathing in infants. This lung is said to take on a compensating action. The proper explanation of it is this: in order that a larger surface of the cells of the sound lung may be exposed to the atmospheric influence, the tubes are proportionably diminished. It is to the increased resistance which they oppose to the passage of the air that the sound is to be attributed.

Laennec has observed, that if the parietes of the chest be removed in a living animal, although by this means the mechanism by which inspiration is generally supposed to be performed be destroyed, the lungs, during the violent efforts which the animal makes to inspire, are dilated.

This he has explained by supposing an active expansive power in the cells themselves. As I am not aware of any analogous process in the animal economy, I have less hesitation in suggesting, as a better explanation, the following:—During inspiration, all those muscles which are associated in this act still contract. Among these are the muscles investing the bronchial tubes. By their contraction a portion of the contents of the tubes, greater in proportion to the violence of the inspiration, will be thrown into the cells, and the dilatation which takes place in these gives the appearance of a general dilatation in the whole lung.

March, 1832.

THE

PRELIMINARY DIARRHŒA AND CONSECUTIVE FEVER

OF THE

MALIGNANT CHOLERA;

FEATURES ALIKE, OF THAT DISEASE, IN INDIA AND ENGLAND.

To the Editor of THE LANCET.

SIR,—Observing with surprise, by your report of the proceedings of the *Westminster Medical Society* on Saturday evening the 10th instant, that an attempt was then made by a gentleman of so justly-esteemed literary reputation as Dr. James Johnson, to foster the unaccountable delusion, of late very generally entertained, that consecutive fever is unnoticed in the published accounts of the epidemic cholera of India, you may perhaps deem it worth while to point the attention of your readers to the following extract from the Report of the Bengal Medical Board, published at Calcutta so long since as the year 1819.

After giving a minute history of the manner of attack and *general* symptoms, the Report enters into an elaborate detail of the *varieties* that the symptoms would occasionally present, and then gives the following luminous description of what ensued when the system succeeded in "rallying from the rude shock it had received."

"When the disorder ran *its full course*, the following appearances presented themselves:—What may be termed the cold stage, or the state of collapse, usually lasted from 24 to 48 hours, and was seldom of more than three complete days' duration. Throughout the first twenty-four hours, nearly all the symptoms of deadly oppression, the cold skin, feeble pulse, vomiting and purging, cramps, thirst, and anguish, continued undiminished. When the system showed symptoms of revival, the vital powers began to rally, the circulation and heat to be restored, and the spasms and sickness to be considerably diminished; the warmth gradually returned, the pulse rose in strength and fulness, and then became sharp and somewhat hard; the tongue grew more deeply furred,—the thirst continued, with less nausea,—the stools were no longer like water (*seethings of oatmeal* the Report describes them in its general account),—they became first brown and watery, then dark, black, and pitchy, and the bowels during many days continued to discharge immense loads of vitiated bile, until with returning health the secretions of the liver and other viscera put on a natural appearance. The *fever, which invariably attended this stage of the disease, may be considered to have been rather the result of nature's efforts to recover herself from the rude shock which she had sustained, than as forming any integrant and necessary part of the disorder itself.*—It partook much of the nature of the common bilious attacks prevalent in these latitudes. There was the hot dry skin; foul, deeply-furred dry tongue; parched mouth; sick stomach; depraved secretions, and quick variable pulse, sometimes with *stupor, delirium, and other marked affections of the brain.* When the disorder proved fatal after reaching this stage, *the tongue, from being cream-coloured, grew brown, and sometimes dark, hard, and more deeply furred; the teeth and lips were covered with sordes;* the state of the skin varied, chills alternating with flushes of heat; the pulse became weak and tremulous; catching of the breath, great restlessness and deep moaning succeeded, and the patient soon sank insensible, under the debilitating effects of frequent dark, pitchy, alvine evacuations."

The Medical Board of Bombay, finding the report of that of Calcutta in general faithful, and strictly according with the symptoms of the disease observed within the limits of their jurisdiction, adopted it *verbatim*; but to the above passage they have appended the following commentary: "We shall only therefore mention, that the subsequent fever which it appears has generally accompanied it in Bengal, has been but little, if at all, observed on this side of India; and, as we have before noticed, may be owing in a great degree to the more extended influence of those causes which are known to produce the bilious remittent fever as an epidemic in the Bengal provinces, than on this side of the Peninsula; for it can scarcely be supposed that a disease so uniform in its attack and in its course should, as it were, deviate from itself in any considerable degree, without the agency *of some local cause*; and *we entirely agree in the opinion, that it cannot be fairly considered as forming a part of it.*"

Now, Sir, supposing the circumstances in the history of this interesting epidemic had been reversed,—that it had first broke out in this country,—that after an interval of several years it had found its way to India, its progress being traceable in a continuous line from Southwark to Bombay,—and that the authorities in the latter place had found the description of the disease published in London so true to the letter as to adopt it as perfect, but that consecutive fever had been seldom if at all observed to attend it at Bombay,—I ask, could they have worded a passage more sensibly or appositely than the above? For Bengal read "Great Britain," and for the bilious remittent fever as an epidemic in the Bengal provinces, read "fevers of the typhoid type as epidemic in England," and the parallel will be perfectly complete. I would further ask, Has consecutive fever been as "invariable in its occurrence" in Europe, as from the official report it would appear to have been even in Bengal?

What I have quoted, Sir, will, I trust, prove sufficient to undeceive your readers, if there be any yet ignorant of the fact that this fever is not a feature of the malady which has sprung up since the disease has made its irruption into Europe. I have taken these passages from the 64th No. of your contemporary, the *Edinburgh Medical and Surgical Journal*, published in July 1820, in the consecutive volumes of which, from the year 1819 to the present time, your readers will find a series of able articles and communications on the subject of this pestilence which will amply recompense the trouble of a reperusal, and will I doubt not convince most of them, as I own it has done me, that the disease we have now in this metropolis is perfectly identical with what was once the scourge of India;—and this independently of the expressed opinions of the numerous medical gentlemen who have recognised in the prevailing epidemic a perfect identity of symptoms with those of which they have seen so much, and treated so many cases, in the eastern hemisphere.

I have already trespassed so much on the valuable space of your Journal, that I fear to request any further indulgence. The following passages, however, extracted from the pages of the work I have named, are so characteristic and striking, as bearing on the identity and disputed points in the history of the diseases, that I trust you will deem them worthy of insertion.

Describing the symptoms of the collapse stage, the Bengal report says,—

"The action of the heart and arteries now nearly ceased; the pulse either became altogether imperceptible at the wrists and temples, or so weak as to give to the finger only an indistinct feeling of fluttering. The respiration was laborious and hurried, sometimes with long and frequently broken inspirations. The skin grew cold, clammy, covered with large drops of sweat, dank and disagreeable to the feel, and discoloured,—*of a blueish-purple or livid hue.* There was great and sudden prostration of strength; anguish and agitation. The countenance became collapsed, the eyes suffused, fixed, and glassy, or heavy and dull; *sunk in their sockets and surrounded by dark circles, &c.*"

The first notice of the novel characters the disorder had assumed since 1817 that I find, is contained in a communication by Dr. Anderson to the editor in the number for July 1819. The history of the symptoms transmitted at this early period is in exact accordance with all others that have subsequently appeared,—"the matter passed (says this gentleman) is watery and free from bile, like barley-broth, rather muddy, sometimes glary like white of egg;" and again, "After some hours the pulse sinks, the motion of the heart becomes fluttering, and soon ceases to be perceptible; the extremities become cold, the skin *shrivelled* from want of blood in the vessels, the countenance collapsed, the eye sunk and glazed, &c." Further on, "The disease most frequently makes its attack after the patient has been asleep for several hours, when it may be supposed that those reservoirs of nervous influence, the ganglions, are in some measure exhausted, and unsupported by the exertions of the brain and spinal marrow, at this time recruiting themselves in sleep. The whole train of symptoms is of the nervous order, and not inflammatory, &c." Dr. Anderson is here illustrating the doctrine by him first propounded, for aught I know to the contrary, *that the cause* affects primarily the system of the par vagum and ganglions; and among numerous facts adduced in proof of his hypothesis, this gentleman adverts to the following: "In this deep distress and agony, it is wonderful to see how clear the understanding generally continues to the last, and the senses unimpaired."

Again, Dr. Rankin, in a communication published in the 74th No. of the same journal (January 1, 1823) after remarking the absence of bile in the evacuations, &c., goes on:—"The symptoms are infinitely various in degree, though perhaps never in kind, except from some internal complaint or other casual agency. In all the cases which I had an opportunity of seeing, the disorder was preceded by general lassitude, frequently ending in shivering, resembling an ague fit." "The eyeballs sink into the sockets; the extremities grow cold; and the skin, clinging as it were to the bones of the face, gives it those ghastly appearances which constitute the *facies hippocratica.* In this stage, the pulse, feeble from the beginning, is seldom perceptible at the wrist." This gentleman's communication is perhaps the more valuable from a circumstance he mentions at its commencement; viz., that it relates chiefly to the phenomena of cholera as they appeared in the town of Jessore, where he then resided, he having had but little opportunity of consulting other members of the profession, and offering only an exposition of his own practice and opinions in treating the disease.

It has been more than hinted, that premonitory symptoms were not observed in India. Take the following passages in refutation.

"The disease sometimes at once, and as it were momentarily, seized persons in perfect health; at other times, those who had been debilitated by previous bodily ailment. Sometimes the *stomach and bowels were disorded for some days before the attack,* which would then in a moment come on in full force, and speedily reduce the patients to extremities."—*Bengal Report.*

"The attack of this epidemic is usually sudden and violent, but in a great majority of instances not without some premonitory symptoms. It is frequently preceded by a simple diarrhœa, continuing several days, and still more commonly by other slight affections, which are more characteristic of the disease. Most commonly it is in the middle of the night, or early in the morning, that these ominous disturbances are first felt." —*Orton.*

"The disease," says Mr. White, "commonly begins with a watery purging, unattended with griping or any pain. At an interval of from half an hour to five or six hours; and sometimes without any interval, the patient vomits a white fluid, uncombined in any instance with bile, of which there is abundant evidence in every quarter. The spasms in the division of the army from which this description is drawn, made their attack at no determinate period of the disease, but in general not for many hours after the commencement of the vomiting and purging."—*Dr. M. Good's Study of Medicine*, vol. i.

In conclusion, Sir, I would request the doubters of identity, if such there still be any, not to waste their time in vain brawling and idle disputes, but quietly to retire to their closets, and read, mark, learn, and inwardly digest, what is written in the early history of this epidemic, and then decide for themselves if there is any circumstance, feature, symptom, character, point of practice, or doctrine, of which we have heard since the irruption of the disease into Europe, that has not its counterpart in the long-since-recorded annals of its ravages in the East.

With many apologies for this very long respass,

I am, Sir,
Your very obedient Servant and
constant Reader,
W. G. W., M.D.
———— near London,
Mar. 28, 1832.

ESSAYS
ON
ANIMAL ACTION.

By William Dobson, *Esq., Surgeon.*

No. 1.

Introduction.

The nature of muscular motion affords a theme of as interesting and useful a kind as can engage the attention of the physiologist. By the agency of the muscles we move from place to place, many of the vital operations are performed, the blood is circulated, and respiration is effected. And the investigation of muscular action will assist us to obtain more correct ideas as to the manner in which certain actions in a healthy body are performed, enabling us thereby to account more satisfactorily for a vast variety of pathological phenomena which have hitherto been totally inexplicable.

However abstruse may be the subject, and however little may be known of it, its investigation is one of imperative necessity. If we cannot discover the essence which directs the various animal movements, we may, by a careful generalization of the facts already ascertained, and by the discovery of new ones, establish a system on a more certain basis, and one more harmonious in its superstructure, than any which now exists. The facts which have been acquired relative to muscular motion, are few. We know that a muscle possesses the property of shortening and elongating itself; that certain muscles contract in obedience to the will, and that others are removed from its dictates; that a few are neither entirely voluntary nor involuntary, and that there are certain muscles which play a particular part when the breathing is disordered; and which on ordinary occasions, exhibit the emotions of the mind. Hence a division has obtained of grouping the muscles into classes, consisting of the voluntary, involuntary, mixed, and respiratory. The essential property of a muscle is contractility, yet we notice a remarkable difference in the habits of executing this action; and we therefore presume that there *must* either be a difference in the mechanism of the parts, or that the moving forces must be dissimilar. But previously to our investigating this circumstance, we must examine what a muscle is, what is its mechanism, and how it is related to, and dependent on, the other parts of the body.

Prior, however, to these inquiries, it may not be impertinent to allude to a few of the means we possess for unravelling this intricate subject. These appear to be, demonstration, analogy, and pathology.

1. *Demonstration;* or, experiments on animals. This strikes the inexperienced observer as a necessary agent, as a means which ought not to be omitted in the analysis of animal action. I concede its paramount utility in a number of cases, but equally deny its applicability to the present subject. I have myself performed numerous experiments, and assisted at others, and have had reasons to ascribe to them a certain value; but on the present occasion, where the situations are so complex and indeterminate, to divide a nerve or a muscle, or to irritate them with a knife to see their actions, would be no better than the conduct of the boy who broke his instrument to find the seat of the musical sounds. By experiments we have collected a number of facts; need we specify those of Bell and Magendie, and others? I would not wish to attempt to undervalue, nor to declaim against, the use of experiments, but merely to allude to their limited efficacy—to their want of invariable applicability, and the injury which must result from the neglect of other means. However potent an instrument experiment may be in the hands of the chemist, it is not so available by the physiologist. The former may employ it, so that he can explore with almost miraculous certainty the nature of his materials, while the latter can only expect to arrive at conclusions far from satisfactory; his deductions will be conjectures, and his theories, fanciful hypotheses.

2. *Analogy.* This is our next instrument. This term has an extended signification, and can be as extensively employed, but in this and the following essays we shall confine it to the comparison of structure, and similarity of phenomena, conjoined. If we look into the wide field of nature, we every-where perceive the applicability of this principle to the disco-

very of the nature of things. When a plant is examined, it is found to possess certain external and internal characters, and observation shows that it buds and blossoms at a certain period of the year; and that it is a nutritive or a medicinal, an inert or an active agent. The system of Jussieu is based on this principle. The zoologist applies it to explain the similitudes and dissimilitudes of the animal kingdom. And it equally claims the attention of the philosopher who investigates animal organisation in its minuter parts. If we find two parts in an animal bearing a strict similarity in their external appearances, and in their internal mechanism, ought we not to inquire whether they do or do not perform similar offices?

3. *Pathology.* This aids in the elucidation of natural action, but the evidences which it affords are only *negative*: therefore, not so ponderous in the delicate scale of science as *positive* testimony. Pathology has been lauded by some as the only sure guidance to physiology; but pathology alone would be an "ignis fatuus," which would often mislead; it must be conjoined with demonstration and analogy. These, when extensively applied, are the talismans by which the operations of the frame may be discovered.

ON THE STRUCTURE OF MUSCLE.

I do not conceive it pertinent to my object to dwell on the minute muscular structure. I merely state, that there is a certain fundamental arrangement of matter, disposed in a series of fibres, which are placed in a parallel direction, and united by cellular tissue, permeated by blood-vessels, nerves, and absorbents, constituting a separate and distinct viscus, hence entitled a muscular organ. Lewenhoek indulged in some vague speculations regarding the ultimate organisations of a muscular fibril, as he did on the globules of the blood; but without arriving at any satisfactory conclusions. His microscopic investigations have led physiologists to believe, that a muscular filament consists of a tube containing little circular balls, which have been designated muscular globules.

Redness of colour is unquestionably no criterion or characteristic quality of a muscle. In the mammalia it is always present, but in birds, especially the domestic fowl, the muscles are nearly white, wher eas in birds of the field they approach towards a scarlet colour. In short, exercise modifies materially the colour of the mucles. Every one is aware of the difference of colour in the wild and the domesticated rabbit. The cause is clearly owing to the greater quantity of blood which circulates through the muscles of those that sustain great muscular expense. Yet in fish that employ great muscular exertions in diving through the dense medium in which they reside, we find the muscles are usually white. There are a few examples of red muscles in fish; we may instance salmon.

The epicure may perhaps find in this circumstance a reason for his preference to particular dishes!

ON THE PROPERTIES OF MUSCLE.

Before the time of Haller little was understood respecting the action of muscles. His philosophical mind when directed to this subject, soon perceived a prominent phenomenon, a consciousness of impressions, and he denominated it "*irritability.*" Haller conceived, and he has many eminent proselytes, that irritability resides in the muscular fibres themselves. But many have impugned this idea. They imagine that this conscious or perceptive principle is confined exclusively to the nerves. To me their arguments are as conclusive as human research can arrive at. But how uncertain experimental data are in this case, is nowhere more clearly exemplified. How are we to decide whether a muscular fibre could or could not undergo the charge of contraction, without a conjunction with some nervous filament? Each muscle receives a nerve, or more, and it is a fair presumption, that each of its fibres does the same, and is diffused throughout. Hence when we irritate a muscular fibre, its nervous filament is necessarily stimulated. It is clear that direct experiments on the part can be of no avail, the two tissues being so inseparably blended. How are we to determine? By circumstantial evidence, and by balancing the arguments and facts which have been advanced on both sides.

The grand argument of Haller in proof of the irritability of muscle is founded on the fact, that when a muscle is severed from the body, and then cut or irritated, it contracts, thus exhibiting a power of feeling impressions. Denying that this sensibility or irritability resides in the nerves, he makes a distinction between the *vis insita* and *vis nervosa.* The former he conceives to be inherent in the muscle, and gives to that organ its irritability; whilst the offices of the vis nervosa, he presumes, are to convey to the mind impressions, and to reconvey the mandates of the mind or volition.

The objectors to the Hallerian doctrine appeal to numerous acknowledged facts. If the nerve going to a muscle be divided, the muscle loses entirely its voluntary contractility, the will may dictate, but it will not be obeyed: but if, as occasionally happens, the divided nerve reunites, voluntary motion is again renewed. Yet this fact weighs nothing; for the muscle will contract, when stimulated by a mechanical or chemical agent. Does not this depend on the irritability in the muscle itself? The answer is, no? The nerves which are *contained* in the muscle still preserve their perceptibility, and thence induce the muscle to move when the nerves are roused into action. That irritability does reside in a muscle no one will deny, and that the nerves are the reservoir is equally as palpable.

Then, irritability is a term used to denote the existence of two vital properties in a muscle, viz., sensation and a something which causes the muscle to move.

Contractility, or the capability of a muscle to shorten itself. This term includes two phenomena, viz. the states of contraction and relaxation. Now, when a muscle contracts the centre swells and the extremities become more approximated, and in a short time the action ceases, and the muscle is restored to its natural state. It has been asked, Is the relaxation an active or a passive act? It appears, certainly, to be nothing more than a passive act: when the cause which produces the contraction is removed the effect ceases: when the will discontinues its application, relaxation uniformly follows. But with reference to irritability; how is the question to be settled? Even admitting the nerves to be the receptacles of irritability, or perhaps speaking more intelligibly, the residences of that principle which cognises impressions, and induces voluntary movements; yet there most indubitably exists, in the muscular fibres, an aptitude to be impressed, a perceptiveness of impressions, and a reactive power manifestable. The cause or causes of sensation and motion have not been ascertained. Hunter's idea, that because some analogy subsists between those principles and the galvanic aura, he went so far as to presume, that galvanism might be the agent by which the brain communicates with the sensitive and motive organs. Dr. Wilson Philip has attempted to establish an identity beween the nervous fluid and galvanism. But I conceive, with many others, that his premises are erroneous, and therefore his deductions untenable. This we shall have to discuss in a subsequent paper.

How wonderful if the galvanic aura be a modification of the mind! We know it to be a powerful agent; both nature and art amply testify. In the arts it performs curious and important operations; but how terrible are some of its operations in nature! Earthquakes and volcanoes, the terror of the ignorant and the wonder of the wise, are its products; by it the most stupendous rocks have been cleft in twain, and cities, once famous perhaps for their genius and legislation, engulfed in the abyss of the earth.

How opposite the effects! Can the galvanic aura *feel*? And can it carry to the brain, the great emporium of the mind, a knowledge of surrounding impressions? If it can, then it is assuredly a mental attribute: for to feel is to know. To perceive implies a perceptive principle, which is identical with the mind. How produced we will leave to the materialist and the theologian to explain. It is sufficient for us to know the facts, that in the nerves of sensation resides a sentient principle, which conveys to the brain a knowledge of surrounding impressions; and a motive principle in the motive nerves, fulfilling the dictates of the mind.

ATTEMPT TO SHOW THAT THE MALIGNANT CHOLERA AND THE "CONGESTIVE FEVER" OF ARMSTRONG ARE THE SAME DISEASE.

To the Editor of The Lancet.

Sir,—After the numerous and some ably-written articles on cholera, it may appear tedious and presumptuous in me to offer an opinion on the subject; and did I not feel that I was doing an act of justice to the memory of another, I might still remain silent.

In reading many of the articles and reports of the discussions on this all-engrossing medical topic, I have sought in vain for any allusion to the opinions of the late Dr. Armstrong. But I observe with regret that most of the writers have been so intent in thrusting themselves before the public, that they have made the interests of science subservient to their own selfish purposes. They have ranged themselves into different classes, and fought for *triumph* not for *truth*—they may obtain an ephemeral notoriety, but will be buried in oblivion when the panic, which called their puny powers into action, shall have ceased.

How great and how pleasing the contrast, to turn from these men and contemplate the genius of Armstrong! Pursuing the study of medicine in the true spirit of the Baconian philosophy, he, exerting the powers of a master mind, emancipated himself from the shackles of antique dogmas, scholastic prejudices, and a slavish regard for names, and resolved by accurate observation and induction to arrive at *truth*. By applying the principles of that great man, to whom philosophy owes so much, to the science of medicine, Armstrong has gone far in dissipating fanciful systems and false theories, pulling down ancient fabrics, and laying the foundation of a temple to medical science, which is only to be entered through the portals of truth.

After witnessing several cases of the disorder now prevalent in this country reputed to be cholera, I am convinced that it is the disease so ably and accurately described by Armstrong under the name of *congestive fever*. He contended that congestive fever proceeded like other fevers from *common* and *peculiar* causes, and has pointed out the symptoms which distinguish the congestion in the different organs.* In the *brain* by giddiness, prostration of the muscular power, weakened and oppressed pulse, &c. In the *spinal chord* by convulsions, spasms of the extremities, &c. In the *heart* and its associated veins by a load and sensation of great weight in the region of the heart, and want of power to breathe; there is no distress in respiration, but an inability to do so. In the *bronchial membrane* and lungs by a weak respiration, and a violet or leaden hue of the countenance. In the *liver* and its associated veins by nausea, retching, and vomiting; distension of the epigastrium and diarrhœa, the stools being like rice-water. In all the forms of congestive fever the skin is cold and the tongue is moist, and covered with ropy mucus. The remote causes of this form of fever are described to be *depressing agents*, and the predisposing cause, *debility*. Knowing to what an extent these causes exist among the poorer class of our fellow creatures, we shall not be at a loss to determine why this disease has been so prevalent among them. The disease now prevailing in this country appears in mild cases to be congestive fever proceeding from common causes, and in some of the more severe to be congestive fever proceeding from peculiar causes, or, as Armstrong has termed it, *congestive typhus*. In reading the description given by Drs. Barry and Russell, in which they say, when reaction comes on, the patient labours under a continued fever not to be distinguished from ordinary fever, which sometimes terminates in perspiration, and sometimes in a fever much resembling typhus, we must be still further convinced, that the disease vaguely and improperly called cholera is merely a prolongation of the first or cold stage of common or typhus fever.

If this view of the subject be correct, we do not stand in need of any more sanitary regulations than are required in typhus fever, which few will believe to be contagious, except to the predisposed, where the rooms of the sick are filthy and ill-ventilated.

In conclusion, I cannot avoid expressing my regret at the manner in which this subject has been discussed by the profession, and at the lamentable want of confidence which the public consequently feel in medical men and medical measures. I would advise the members of our profession for the future to discard all prejudices, petty jealousies, and party animosities, and cultivate the study of medicine in the true spirit of philosophy, by which means alone we can advance our science, elevate ourselves, and effectually benefit our fellow-creatures. I am, Sir, yours, &c.,

W. Simpson.

Hammersmith, March 29, 1832.

EXTRAORDINARY NARRATIVE.

[Under the above heading a letter appears at page 441 of our present volume, written by Mr. Hoare, a most respectable surgeon of Warminster, detailing particulars of a young man of the name of Higgins, who, though fully endowed with the power of speech, would never speak to his father, nor, as then believed by Mr. Hoare, to any other male person.* We this week insert a farther communication from that gentleman on the subject, the public press in his neighbourhood having, as he observes, treated the statement alluded to as too strange to be credible.]

To the Editor of The Lancet.

Sir,—My communication (see Lancet, December 24, 1831) respecting the young man of the name of Higgins having occasioned many inquiries, much sarcastic observation, and many expressions of incredulity, I took the liberty of writing to the

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* See Lancet, vol. v. p. 40.

* We reprint the following portion of Mr. Hoare's letter.

"The wife of a Mr. Higgins, a farmer of Baltonsborough, near Glastonbury, Somerset, having brought him three daughters in succession, and no son, he was so disconcerted at the repeated disappointment, that he vowed, should his next child be a daughter, he would never speak to her. On the approach of his wife's fourth confinement, he repeated this vow. To his great joy, his wife gave birth to a *son*, and nothing occurred to lessen his satisfaction, until the child began to speak. To his astonishment and distress he then found, that, while the boy would readily address his mother and sisters, and indeed any female, nothing could induce him to utter a word to his father, or any male person. This singularity continued during the whole of his father's life (30 years)."

Rev. Dr. Colston on the subject, who has kindly favoured me with the following circumstantial reply. This, if not sufficient to convince the incredulous, will at least exonerate me from the imputation of having published an extraordinary statement on light grounds. I am, Sir,

Your obedient servant,
JOHN HOARE, Surgeon.
Warminster, March 17, 1832.

"Lydford Rectory, Somerton,
March 4th, 1832.

"SIR,—Immediately on the receipt of your letter I sent for Eli Higgins, a tenant of mine, and the man who has excited so much interest from his inability to speak to persons generally. It is true he never was able to speak to his father, who resorted both to violent and kind means to induce him to do so, without effect. With his mother and sisters he frequently conversed without any extraordinary effort, and these were the only females with whom he ever held conversation; but he admitted, on my examining him minutely on the subject, that he had spoken to the Rev. Mr. Bowen, of Bath, and Mr. Beauchamp, a land surveyor, of West Pennard, in this county, about four years since; but he was unable to account for this exception, as he wished much to speak to every one, more particularly his father, who has been dead nearly six months. After a lapse of three days he could speak with fluency to every one, and immediately after the funeral of his father he called on me to take the small estate that had been occupied by him, and entered fully into conversation; which greatly astonished me, as I have known him from his earliest days, and made many attempts to get him to speak to me; once in particular, wishing to know the hour of the day, I asked him for his watch, which he gave me, and I threatened to keep it if he would not ask for it, but in vain. He admits from what he had heard, that he was strongly impressed with the idea that on the death of his father he should have power to address every one without difficulty, which has proved to be the case. He had heard of the rash vow made by his parent when his mother was pregnant with him, that if the child were a girl he would never speak to it, but he could not attest to the truth of this from any authority but general report. Eli's appearance is delicate, and does not indicate strength. There is something very peculiar in his manner of using his mouth when he speaks, and the length of his jaw, from the point of his ear to his chin, is very striking; his voice is pleasing, and his manner of conversation sensible for his station in life. He is a hard-working, industrious man, and has always exerted himself to the utmost to promote his father's interest. You may make use of these particulars in any manner you please, and I remain, Sir, yours faithfully,

"W. H. COLSTON.

"To John Hoare, Esq., Surgeon,
Warminster, Wilts.

"Below is the writing of Higgins, which I thought might be interesting to you.

"'Eli Higgins Aged 35 years in the parish of Baltonsborough in County of Somerset.'"

WESTMINSTER MEDICAL SOCIETY.

Saturday, March 31st, 1832.

Dr. SIGMOND in the Chair.

DISCUSSION ON THE CHOLERA.—QUESTION OF CONTAGION.—CASES AT THE BOROUGH HOSPITALS. — CHARGES AGAINST THE BOARD OF HEALTH.

Dr. WEBSTER asked if there were present any medical officer connected with the parish of St. James's, as he wished to call their attention to a case of supposed cholera which had occurred in that parish; in fact, just outside the walls of the room in which he was speaking. The patient, a working man, was seized last Saturday and died on Sunday. He was of dissipated habits, lived in a cellar, and had been subject to a bowel complaint. At six o'clock on the Saturday morning he went to his work, and on his way drank three pints of new ale, or wort; soon became cold and weak, and returned home before seven, when he had some rum. The symptoms were then much worse; he was cold, sick, and purged. He saw the patient at 10 o'clock on Sunday morning, and at 11 he died. Not a single other case had occurred in the neighbourhood, although his mother, brother, and father, were living in the same room with him. The disease, therefore, was not contagious. Besides this there was a school of thirty children, kept on the landing-place, only one of whom had been taken ill, and that one was absent on the day in question, on account of indisposition. The cholera, therefore, was not contagious. The workmen also at the place where the patient was first seized had remained healthy. The case was worthy of attention. Before sitting down he would read a statement of the number of medical cases admitted within some few years past into the institution to which he was attached. Statistic observations might throw much light upon the character of the present epidemic.

	Med. Cases.
In 1825 admitted	195
1829	203
1830	198
From Feb. 15, 1831, to the present time	183

Mr. Walker was present at the examination of the case related by Mr. Webster,—a case which Dr. Daun and some other gentlemen had pronounced to be as decided a case of malignant cholera, as they had ever witnessed. As to the man's previous life, he certainly had been in delicate health; he had been subject to stricture and retention of urine, which however had little to do with a disease of this sort. It was also true that he was drunk two or three times during the week in which he died, although he was actually reputed previously sober. Perhaps from being unaccustomed to drinking, a very little had so affected him. All had agreed that it was a marked case of cholera.

Dr. Johnson would advert to some circumstances which had taken place very lately in Guy's hospital. He did not speak from personal observation, but on very good authority, that of Dr. Bright. He had heard, then, that six or seven cases of cholera had happened in that hospital, each in different wards, without any connexion. Amongst those attacked, some were nurses, who had not been attending upon any fever patients. Now those who knew the situation of Guy's Hospital, would be inclined to agree with him that this circumstance was strongly corroborative of the idea that some epidemic influence was prevailing which struck individuals, independently of all contagion.

Mr. Greenwood thought that the fact of there being complete communication and constant intercourse between the wards of Guy's Hospital, put a decided negative on Dr. Johnson's argument. But why these representations of epidemic influence? How extensive must be the malaria! The disease was manifesting itself contemporaneously in several distant parts of the globe. Speaking of this simultaneous development of the cholera in far-separated places, he would point to an excellent illustration afforded by the triumph of medical chemistry. He alluded to the investigations of Drs. Rose and O'Shaughnessy, who had decidedly proved the chemical identity of the cholera of Berlin and this country.

Dr. Gilkrest said, that to Dr. Johnson's statement it might be added also, that the cholera had appeared in St. Thomas's Hospital, where the cases were not only in different wards, but in opposite wings of the building. He had made particular inquiries into them, and could discover no trace of contagion. Touching the question of contagion, however, he would then state a fact lately related to him, of five persons in one family in Dublin, who were successively afflicted with the toothach!

Dr. Granville had come thither that evening absolutely full to choking with matters relating to the Central Boards of Health, and the sooner he could relieve himself of his burden the better. He had been very busy in the examination of the official documents published by the Boards, which he really found overflowing with contradictions. In making an epitome of these inconsistencies, he disclaimed feelings of ill-will towards any party; and would therefore proceed at once to exhibit what might be aptly called "cholera cross-readings," or "a contrast between words and deeds." (Here the speaker entered into an examination of the "manifestoes" and other official documents issued by the London Boards of Health, and comparing them with extracts from the periodical called the *Cholera Gazette*, and the reports of Drs. Russell and Barry, as commissioners in Russia.) His object was to show, that the opinions of the members of the boards were at variance with the directions given by the boards themselves, and that the sentiments respecting the principles of the disease, published by them at one time, differed materially from those of another period. He laid particular stress upon the statements of the period of incubation of cholera, and the diversity of doctrine regarding the *atmospheric* and *contagious* conveyance of the seeds of disease. Little wonder was there therefore that *the Society* should have gone on debating all these questions ever since last October, without being enabled to come to a satisfactory decision. In conclusion, he adverted to the facilities which Drs. Barry and Russell represented themselves as having enjoyed through the kindness of the Russian physicians when at St. Petersburgh. But in England, when Drs. Barry and Russell had like opportunities of affording facilities to others, not the slightest aid was afforded to gentlemen for enabling them to investigate the cholera cases, even the doors, in some instances, being shut against them.

Dr. Fergusson could not suffer this reflection on the Board to pass without notice. The members of that body were far from objecting to the visits to cholera hospitals; and when the deputation from the Cholera Association waited on the Board, the members assured them that it was out of their power to give permission to do so. The obstacles were created by the local officers. He (Dr. F.) was one of the deputation, so also was Dr. Granville, which gave him the more surprise at Dr. G.'s accusation.

Dr. Granville stated that he was by no means desirous of throwing the blame on the Board. His censure was directed against the officers in the parishes.

Mr. Greenwood thought that Dr. Granville had garbled some statements, and wholly omitted others, which were essential to the bearings of the case. He thought every reliance might be placed on the statements of Drs. Barry and Russell, even if they had occasionally differed. Their reputations were staked on an adherence to truth as to the question of contagion. All the medical men who had witnessed the diseases in India and England participated in a belief of it. As to the self-elected little coterie, the *Cholera Association*, one of them, Dr. Johnson, was an advocate of "contingent contagion;" but what the precise value was of the word "contingent," when tacked to that of contagion, he knew not. There was no acknowledged contagious disease amongst the whole order of exanthemata, which had not the same claims to "contingency." He had that week witnessed in a small court in Wigan-street a circumstance strongly corroborative of the doctrine of contagion. A woman was taken ill of cholera at No. 13, and died. A man, who lodged there, left the house and removed to No. 4, where he also was seized with symptoms of the cholera, but upon calling that evening he (Mr. G.) could not carry his investigation farther in consequence of the crowd collected around the house. And here he would take the opportunity of contributing his censure against those individuals who, by their letters in the public newspapers, had conduced to goad the people to mob those who, in the discharge of a benevolent duty, visited the afflicted victims of this disorder.

Dr. Johnson said that one of the gentlemen alluded to by the last speaker, Mr. Orton, before he left India, wrote a report to the Madras Board of Health, in which he gave a decided opinion that the disease was *not* contagious. That was written when the facts were before him, but in his closet in Europe he thought right to retract the opinion. He would mention a fact relating to this question which was not likely as yet to be known to the members. The proof sheets of the present *Edinburgh Medical and Surgical Journal* had been sent him, and he found that although a previous number had contained a long and talented article arguing strongly in favour of contagion, yet in this present journal the tables were completely turned. The editor had made a tour, and the facts which he had observed at Sunderland, Edinburgh, Musselburgh, and elsewhere, had made him a decided *non*-contagionist. Here then was one of the most influential medical journals converted, and by philosophic argument endeavouring to carry the same conviction home to the minds of others. This example he would particularly recommend to the attention of Mr. Greenwood.

Mr. Orton being particularly called upon, adverted to Dr. Johnson's remarks respecting him (Mr. O.). He admitted that he had affirmed it as his belief that cholera was not contagious; but this was a hasty decision, and one formed after only three weeks' experience of the disease. He had, however, since seen the disease in England, and from the facts he had observed, his conclusion was that the cholera was contagious, or communicable from man to man.

Dr. Johnson. From facts witnessed since you left India?

Mr. Orton said he deemed the mass of evidence on the *contagion* side of the question, perfectly irresistible. A particular instance had occurred to him when travelling a short time since. A woman in the coach who was returning from the north was suddenly attacked with vomiting, purging, spasms, and excessive debility. Her legs became blue. Some of these symptoms did not appear till after she had been bled. They appeared so much like cholera that he inquired if she had been near any persons affected with that disorder, when it was found that a gentleman had been travelling with her who had said that he came from Newcastle; that, moreover, upon her complaining of the cold, this gentleman had offered his cloak which she accepted, and wore for some time. The woman was confined at an inn, where the chambermaid who waited on her also was seized with the same disease.

Mr. Samuel remarked that a gentleman, a member of that Society, who at the first commencement of these discussions had avowed himself from all he had read to be a contagionist, had now *seen* the disease and declared his opinion that it was *not* contagious. (*Name, name.*)

Dr. Sigmond as chairman observed that the Society were desirous of being favoured with the name of the gentleman alluded to.

Mr. Samuel.—It is Dr. Sigmond. (*A laugh.*)

Dr. Sigmond was wroth at this, and resented such a reference to the chairman. He considered it unfair thus to allude to an individual while occupying the office of president. (Mr. Chinnock accordingly took the chair for a time while Dr. Sigmond entered into an exposition of his opinions respecting contagion, and the reasons which had induced him to adopt them.) He had investigated the disease with zeal, and the result had thrown doubt upon the judgment to which reading in the first place had led him; though even now he could not say that his mind was exactly made up on the subject; yet, as far as he had been enabled to observe the disease, it certainly appeared to be non-contagious. The case of the man Sandiland had been held out as a special proof of contagion, but it seemed that the man had had a relapse since February 28th, which quite altered the face of the circumstance.

Mr. Samuel disclaimed all intention of saying anything that would be unpleasant to Dr. Sigmond.

Shortly after the meeting separated.

THE LANCET.

London, Saturday, April 7th, 1832.

The hospital surgeons' method of answering the charge of neglect, brought against them by their dissatisfied and indignant pupils, is not without precedent, as the records of our criminal courts furnish innumerable examples of similar ingenuity; but it is not the less worthy of admiration because it does not possess the merit of originality. The pupils allege that the *dead-house* is scarcely ever visited by their instructors (instructors! heaven bless the mark), and that when *post-mortem* examinations actually take place under the superintendence of the surgeons and physicians, they are either conducted in private, or are instituted without any previous notice addressed to the students generally. What is the reply of the medical officers! Thus it stands. "Nothing is stated respecting *post-mortem* examinations in the 'Prospectuses' issued previously to the commencement of each medical session." Worthy of Ikey Solomons himself! True, the papers published by several of the hospitals speak not in decided terms of the transactions of the dead-house; but in the absence of all written contracts, cannot these men feel and understand that there exists such a thing as an *implied obligation*? There is no necessity, however, for making an appeal to a feeling of honour in an affair of this kind. The words of the "bond" are amply sufficient for one purpose, because it cannot be denied, except by men who appear to consider that the robbery of the student is the most important part of their professional duties, that the pathological information derived from the labours of the dead-house, is the great, the only solid basis upon which the surgical practice of the hospital can be founded. Is the subject of *pathology* excluded from lectures on surgery? Does it form none of those essays which are designed to teach the *science* of chirurgery? At some of the hospitals, however, the important operations of the "dead-house" *are* awarded a place amongst the terms held out in the printed *prospectuses*. In those of St. Bartholomew's for example we find:—"Morbid inspections, as opportunities occur; at one o'clock." What an engagement! At "*one* o'clock"—at mid-*day* or mid *night*? Or, on *what* days? Why, of course, "as *opportunities* occur." Ingeniously-contrived loop-hole! So that, if there be no inspections at all, they first refer the student to the terms of the prospectus; and, secondly, vomit forth the denial of there being *any* "opportunities" at all. "Relations interfere"—"The friend object"—"The bodies are taken away suddenly by undertakers;" the evasions are easily concocted; and, in fact, *any* excuse is offered as a cover for a disgraceful breach of engagement. As Bartholomew's is one of the hospitals so bitterly complained of by the students, it may be useful, if not instructive, to hear the language which Mr. Lawrence addressed to his class in relation to this shamefully-neglected branch of study—morbid anatomy:—

> "It must be the first business of the medical student to learn the structure of the body in its living actions; that is, to study

man in the state of health. These are the objects of the two sciences which are denominated anatomy and physiology. He *then* proceeds to the observation of *diseases*; he watches the circumstances under which they *arise*; he follows their *progress* and *termination*; he *explores* the *organic changes* which they *produce*, and learns to *connect* these changes with their appropriate *external signs* or *symptoms*; deriving from these *comparisons* the means of distinguishing the *exact seat* of disease, and predicting its course and event. He is *now* prepared to employ the external agencies, the *outward* and *inward* remedies, or the operative proceedings which may be necessary for removing disease and restoring health. When disease is studied in *this manner*, in reference to the whole body, it constitutes the science of *general pathology*."—*Introductory Lecture, p.* 35. Lancet, 1829—30.

In another part of his discourse Mr. Lawrence observed:—

"But the study of anatomy and physiology does not make us acquainted with *disease*: you may know the *structure* and *functions* of an organ *perfectly*, and yet be ignorant of its *diseases*. How then are these to be learned? *Not from* LECTURES, nor from *writings*; but by studying the great book of *nature*. You must frequent the *hospital* and the *sick chambers*, and observe diseases for yourselves. *Lectures* and *books* are AUXILIARIES, and, *under certain circumstances*, very useful ones; but they are of secondary importance, compared with the actual observation of disease."

"To know disease, then, you must see, examine, and closely watch patients; you must observe the *origin* and *progress* of the altered functions *during life*, and *then investigate, after death*, the *changes* produced in the *organisation*. * * * * *Demonstration* of the phenomena of disease on the patient by the *teacher*, and the actual observation of them by the *learner*, are the *only means* by which *real knowledge* can be acquired. The *facts* thus presented to the *senses* make a stronger impression than *any description*, even by the ablest *lecturer* or *writer*; while the information which a person *thus* acquires for himself from *nature*, can *always be depended on*, and is *never forgotten*. Between him who has only *heard*, or *read*, and one who has *seen*, there is the same difference in point of knowledge, as between a person who has merely perused the description of foreign countries, and another who has actually visited them."—*Ibid.* p. 40.

Nothing could be more truly or more happily expressed. But it is to be deplored that Mr. Lawrence prefers *precept* rather than *example*. If, in neglecting the duties of the dead-house, a man were ignorant of the injustice and cruelty he was inflicting upon the pupil, some excuse might be offered for him; far otherwise is it however with respect to that officer who, while he professes a thorough knowledge of the sacred character of the task which his station imposes upon him, makes little or no effort to execute it, although his labours are constantly invited by a ready and bountiful command of all the essential requisites for their complete success.

In a letter addressed to us by a pupil of St. Bartholomew's, the intelligent writer complains, "That from the 1st of October last up to the 20th of March, only *two post-mortem examinations* have been duly and publicly announced; that the *objects* in employing medicinal agents are not explained; that the student has to run with a multitude of others from one bed to another merely to see a nod or a wink from the surgeon, and to hear an occasional ejaculation—'Ah!' and that, with the exception of the regularity observed in the delivery of some of the courses of lectures, the whole establishment furnishes a correct notion of chaos." This communication is one of at least fifty received from the students of Mr. Lawrence's hospital. Another of the complainants declares, "That although he has directed an anxious eye to the door of the dead-house during the whole of the session, he has not seen the surgeons enter that apartment more than three times." Three times in *six* months! And can these officers have the unblushing effrontery to pretend that in thus neglecting to demonstrate the changes produced by disease, they violate no engagement with the students? Impossible. The impudence of even the Bats is not equal to it. Justly has it been remarked, that information derived through the *eye* is far more lasting than that acquired through the ear. Compare a well-conducted inspection in the dead-house with the hour's chatter of a "regular lecturer" in his theatre, and of what value, of what practical importance or utility, does the latter appear? The worth of "chatter," and no more. Mr. Lawrence has well explained this point in his comparison between the actual traveller and the mere *reader* of *travels*. We strongly suspect, however, that at the time he was making this just reference he had forgotton his own resemblance to Gulliver. With the day-book of *travels* in one hand, and the interesting, instructive *country* in the other, he vends the *story* and retains his grasp of the fact. In a word, with the *country* as perfectly at his disposal as the paltry work which pretends to give a description of its beauties and peculiarities he presents to his pupils the lying records, and withholds the demonstrative evidence of fact. He will neither journey for his own amusement, nor does he care to let others travel for their instruction. He praises, in order that he may enhance the value of that which it is not his intention to bestow, and labours to demonstrate the worthlessness of those wares for which he is in the habit of making such a liberal charge.* O, monopoly! thou unsightliest of devils; how we execrate thy name and practices!

It has been often remarked by gentlemen, who have had opportunities of observing the system of medical education pursued in the great hospitals on the continent, that, were *English* students aware of its immense advantages, they would not submit to the tyranny and exactions of which they are now the victims. This is altogether erroneous, and the individuals who hazard such an assertion, know nothing of the difficulties which students have to encounter in offering any resistance to the trammels imposed upon them by their relentless "hospital" instructors. Are not the *surgeons* of the hospital those identical individuals who figure as the Council of the College in Lincoln's-inn Fields? Are not the *medical* teachers intimately interwoven in the net of professional practice with the Court of Examiners of Apothecaries Hall? If then the student openly demand a restitution of his rights with regard to *post-mortem* examinations, or the remedy of any flagrant abuse, what is to protect him from the malice of his examiners when he applies for his diploma or license? Knowing, and justly dreading, the danger to which they may be exposed, in the event of their being at open war with their teachers, the students are obliged in a great measure to put up with the burden of existing evils, rather than incur the penalty of greater miseries. They know that the examinations at the College and the Hall are conducted *secretly*,—that the public are not present,—that the press is not present,—in fact, that there is nothing to shield them from the malignant ire of roused, indignant monopolists. Considering, therefore, that it would be an act of cruelty to impose upon the students the task of bringing their teachers to a just discharge of their duty, we shall direct the artillery of the community against the common mercenary traders in wounds and groans. We will see whether public opinion will not force them to comply with the conditions of the bond; and in order to bring upon these bats and corruptionists their due share of public odium, we will, next week, furnish a general view of the manner in which the duties of hospital surgeons are discharged in the great eleemosynary institutions of the continent.

In closing this article, we once more commend to the attention of the reader, and particularly to that of the St. Bartholomew students, the extracts from Mr. Lawrence's lectures, which have been herein submitted for their perusal. Once more do we entreat of them to reflect upon what that hospital surgeon *himself* says of the great value of post-mortem inspections.

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* Surgical Lectures, St. Bartholomew's. Terms Two Courses (regular) £8. 8s.

Sir A. Cooper's charge for perpetual attendance upon *his* surgical lectures—not the worst in the world—was £5. 5s.

The cholera has at length made its appearance in the heart of France. As yet, however, we have obtained a knowledge of the fact only through those un-exact channels of communication the newspapers. We abstain therefore this week from making any comment, or attempting to furnish any information on the subject, desiring rather to wait until we are in possession of such authentic documents as we can place with confidence before the medical public. In the mean time we cannot but express the gratification we feel at the noble manner in which the Parisian faculty have prepared themselves for the calamitous visitation. We hear of no disputes, ignorantly based, and obstinately defended, amongst them, relative to the question of "identity." No ill-timed or unavailing repining of the mercantile classes—no inflammatory addresses from the public press, denouncing cholera as a "humbug," and medical men as impostors. On the contrary, men in all departments of the profession have applied themselves to arrest the progress of the disease, and improve the condition of the poor, with a zeal and energy which contrast most strongly with the tardy and beggarly manner that has been exhibited on this side of the channel.

The non-professional French journalists express surprise that the disease has commenced in Paris instead of at some of the frontier towns. Did the French surgeons, who were lately in England, residing in the very centre of infection, undergo strict quarantine before re-admission to their own capital? There are men who will hasten to discuss the appearance of the disease in Paris weeks before even the great outlines of the question will be prepared for examination—fools who, we doubt not, have hours since rushed with their morsels of imbecility to the press. Men of true science, and anxiety for the truth, will be patient.

The Anatomical Pathology of the Malignant Cholera, illustrated by a series of Cases, with Dissections, &c. Collected and arranged by P. H. GREEN, Bachelor of Arts and of Medicine; Sen. Scholar of the Univ. Dublin, &c. 8vo. pp. 54. Highley, 32, Fleet-street.

WHILE the malignant cholera was confined to the British possessions in India, the anatomical relations of the disease were little understood; but as the complaint continued to advance westward, abundant opportunities have occurred for investigating the pathological nature; decidedly one of the best monographs on this subject was a small tract, published by the Medico-Chirurgical Society of Moscow, and entitled *Disquisitio Anatomico-Pathologica in Hominibus Cholera-Morbo extinctis.* This has been taken by Dr. GREEN as the ground-work of a pamphlet on the anatomical pathology of cholera. He has added some cases with dissections performed in Berlin, Sunderland, and London, affording altogether an exceedingly clear and comprehensive view of the morbid appearances observable in persons who have been cut off by the disease in its different stages. It contains an account of twenty-one dissections, in eighteen of which the contents of the vertebral canal, with the other cavities, were carefully examined; each dissection is preceded by a brief account of the symptoms and treatment, and the cases are divided into two classes, according as death occurred in the primary or secondary stage of the disease. At the conclusion of the pamphlet, we have a summary of the appearances divided into three classes:—

1st. Those constantly seen in every case when the patient died during the stage of collapse.

2nd. Appearances less frequently seen, but not universally.

3rd. Appearances less frequently found, and usually attendant on the secondary stage.

Under the first class of appearances the author comprehends the six following:—

1. Some lividity of the surface of the body, especially of the nails.

2. Rigidity of the body, particularly of the lower extremities.

3. Dark blood abounding in the veins.

4. *Dark-coloured blood in both cavities of the heart and in the arteries, particularly those of the base of the brain.*

5. Proper vessels of the heart distended with blood.

6. Urinary bladder contracted, and containing little or no urine.

These are post-mortem appearances, which the author says are universally attendant on death occurring during the stage of collapse. We think, however, he should have omitted the contracted bladder, for though seen in a large majority of cases, it is not *universally* observable.

The following extracts relate to a subject of considerable moment, and may be quoted both for the permanent practical value and interest which attach to the subject of it.

Our readers may perhaps be acquainted with the case of the idiot boy who died in the Newington workhouse on the 18th ult. The jury assembled to inquire into the cause of death, pronounced a verdict of—"Died from apoplexy," in opposition to the evidence of the parish surgeon. These same worthies have since been figuring in the public prints, abusing the conduct of the surgeon as "ungentlemanlike," in attempting to differ in opinion from them, and offering again to swear to the truth of their former judgment. It happened, rather unfortunately for the discretion of these gentlemen, that two physicians and four surgeons were present at the dissection, and that all declared there was no apoplexy. We have been assured by one of them that he examined every portion of the brain and spinal marrow with the most scrupulous accuracy, without detecting any trace of effused fluid, or other appearance which could warrant such a deduction. The following is the description of these parts taken from Dr. Green's pamphlet:—

The body of the idiot boy was examined on the 21st, at 12 o'clock p.m., by Dr. Green, in presence of Dr. O'Shaughnessy, Monsieur Dalmas, Messrs. Evans, Kingston, Lowne, and Vogan, surgeons.

"*External appearances.*—Face swollen and bloated; lips livid, body rigid; the upper extremities semiflexed, and extremely stiff; nails blue; no lividity of the other parts of the body.

"*Cavity of the Cranium.*—The proper vessels of the dura mater injected with dark blood; a great deal of fluid blood in the sinuses; the superficial vessels of the brain also much gorged, and the arteries at the base of the brain contain dark blood. The cerebral substance, when cut into, does not show any bloody spots; a small quantity of serum in the lateral ventricles, and the vessels of the choroid plexus, much distended with venous blood; the substance of the cerebrum and cerebellum firm and healthy; at the posterior part of the right lobe of the cerebellum, a small portion of the medullary substance near the surface was very hard: not yielding between the fingers when pressed, and resisting the knife when divided; the internal appearance of this hardened portion was healthy.

"*Vertebral Canal.* — The dura mater lining the vertebral canal in a normal state; a quantity of clear fluid in its cavity; thickening and opacity of the arachnoid at the commencement of the cauda equina; venous injection of the cellular tissue, between the bodies of the vertebræ and the posterior common ligament; so great near the cauda equina as to approach extravasation; the substance of the spinal marrow, throughout its whole extent healthy.

"*Cavity of the Thorax.*—Lungs perfectly healthy in colour and structure; the posterior vessels of the heart distended with dark blood; the right side of the heart flaccid, containing a quantity of black fluid blood; the left ventricle hard, contracted, its substance slightly thickened, containing a small quantity of fluid, exactly similar in colour and consistence to that in the right ventricle; no fibrinous concretions in the ventricles; all the valves healthy; the thoracic aorta contained some dark fluid blood; sympathetic nerve in the chest healthy.

"*Cavity of the Abdomen.*—External view of the intestines;—Stomach distended with gas; the colour of the greater part of the intestines pinkish; the extremity of the ileum, transverse arch of the colon and rectum, closely contracted; the peritoneal coat of the intestines covered with a viscid secretion discoverable only by the touch; the internal membrane of the stomach pale, and lined with a creamy matter; light red injection of the venous branches of the small intestines through their whole tract; the mucous membrane of the large intestines pale, and not injected; the intestines contain a gruel-like fluid, which shows an alkaline action upon test paper; liver healthy; the gall-bladder contains some green fluid bile, but no trace of bile in the stomach or intestines; the urinary bladder contracted, though not extremely so, pale and corrugated on its inner surface, which is lined with a yellowish matter; the kidneys gorged with venous blood; when the mamillary processes are compressed, a yellow matter issues forth; the same kind of matter is contained in the ureters; it appears similar to that which lines the bladder; the same kind of matter also found in the vas deferens; the abdominal aorta contained some dark fluid blood; nerves and ganglia healthy."

Any one at all conversant with the pathology of the cholera, will see that these appearances were precisely such as we would expect to find in the body of a cholera patient. The presence of the yellowish matter in the mamillary processes of the kidneys is a curious circumstance, which has not we believe been noticed by any one before Dr. Green. If it be similar to the yellow matter lining the ureters and the bladder, it shows that the kidneys separate this yellow matter instead of urine from the blood; and, probably, that the remnant of thick fluid in the ureters and bladder is not the product of their mucous membrane as had been supposed, but a secretion from the kidneys. The paralysis of the bladder, occurring during the first stage, and giving rise to an enormous collection of water in this viscus during the second stage, is another circumstance pointed out by the author as worthy of attention.

The pamphlet concludes with some remarks on the pathological cause of cholera, and on the theories advanced by Baron Dupuytren, M. Delpech, and Dr. Hope. Dr. Green conceives he is warranted by the preceding cases, by the results of his own experience, and that of many of the continental anatomists, to conclude, that the existence of dark blood in both sides of the heart and arteries, is the pathological cause of cholera, because it is the only appearance, of any importance, *constantly* observable. He states this as a simple fact, derived from anatomical investigation, without attempting to build up any theory of his own on the circumstance. The opinions of Dupuytren, Delpech, and Hope, are then examined. The author says, that the same objection holds against all, namely, that they are founded upon appearances which are not constantly observable, and would therefore lead to the absurdity of admitting a consequent without an antecedent. Dr. Green considers the theory of Baron Dupuytren conjectural, because the latter had not seen an example of the true disease. With respect to the opinion of Delpech, he says, that since the promulgation of the professor's ideas concerning the ganglionic nerve, he has examined carefully, *propriis manibus*, the bodies of five persons who have died of cholera. In four the semilunar ganglia was healthy; in one the left was somewhat injected. The inflammatory notion of Dr. Hope is perhaps treated somewhat too severely, but the author of this work has acquired a very good right to be critical on an occasion in which he evinces so intimate an acquaintance with his subject.

Our approval of Dr. Green's little work, however, must not be taken on inference only. It deserves and must obtain in the profession general circulation,—to aid it in which we now give the pamphlet our warmest recommendation. Of the thousand and ten brochures which the cholera has called into existence in Great Britain, it ranks among the ten, while the thousand are fast going down the stream of time, and falling into the pool of Lethe. In short, the man who boasts of his reading on the subject of the cholera, (as five out of every six of the numerous controversialists at present engaged in its discussion invariably do,) and at the same time has neglected to peruse the memoir of Dr. Green, must consider he has passed by very important means for regulating his own decisions on the disease, and influencing the opinions of others. Dr. Green is no child in anatomy, nor is his pamphlet a mere rattle for gaining public attention. On the contrary, we believe we may say, with confidence, that his statements are to be relied on as the result of as careful, accurate, and competent examinations as have hitherto been made in any quarter into the mysteries of this awful and malignant disease.

LONDON HOSPITAL.

SCIRRHUS OF THE MAMMA. EXTIRPATION —DEATH.

March 26, 1832.—Sarah Palmer, ætat. 47, married, mother of eight children, the youngest sixteen months old, was admitted into this hospital on the 22nd instant, under the care of Mr. Scott, labouring under scirrhus of the left mamma. She states that the disease first commenced about eight months since, but without her being able to assign any particular cause for its formation. On examination the nipple was found retracted and puckered, the diseased part hard and lobulated, with the upper portion presenting a dark livid appearance.

The operation of extirpation was this day performed in the following manner. The extent of the morbid affection being carefully distinguished, an incision was commenced on the brachial side, and carried downwards till the knife made a point directly opposite on the sternal side, describing in its course a segment of a circle. A similar incision was then made in the same direction on the superior side of the nipple, and carried onwards to the point where the first incision terminated. The gland was then dissected away from the subjacent muscles, great care being taken that no morbid or indurated part should remain. The time of the operation was prolonged in consequence of there being three indurated glands in the axilla, which were carefully removed. After the parts were removed, it was found necessary to tie upwards of ten vessels. The edges of the wound were then brought together by one suture, and retained by straps of adhesive plaster, and the whole was kept *in situ* by a compress and roller. After the operation she had *tinct. opii* ♏.·xx.

27. Has passed a tranquil night, pulse 78, soft; tongue clean, bowels not open. *Ol. ricini* ℥ss *statim sumend. Mist. effervescens* omnis 6tis hor.

28. Pulse 84, fuller; tongue clean; bowels open three times; skin dry. Slept tolerably well last night. To continue the effervescing mixture. Eight p. m. complains of severe pain in the left side; thirst, with difficulty of breathing; countenance anxious; pulse 94, small; to have 30 leeches to the side.

29. Passed a restless night; pain in the side and difficulty of breathing very much increased; and at eight a. m. died.

FATAL CUT THROAT.

March 26. At seven this morning, a man was admitted who had made a most determined attempt to commit suicide by cutting his throat, the incision being just above the thyroid cartilage, separating the cornuæ from the os hyoides, cutting into the pharynx, and dividing a few of the fibres of the sterno-cleido-mastoidei muscles. The dresser succeeded in injecting some barley water into his stomach by passing the gum elastic tube of the stomach-pump through the wound down the œsophagus into the stomach; but on attempting this afternoon again to pass it, so much irritation was caused by the instrument that the experiment was immediately desisted from, and an enema of barley-water, with *tinct. opii*, ♏ xxx, was ordered instead.

27. Passed a very restless night. The whole of his time appears to be passed in prayer. Respiration difficult, with mucous rattle; intense thirst; is evidently sinking. The enema has been repeated three times. Eight p. m. is rapidly sinking; pulse 97, small; respiration extremely difficult; intense thirst; skin hot and dry.

28. Passed a very restless night. At half past six he died.

ABUSE IN THE LIBRARY REGULATIONS.

To the Editor of THE LANCET.

SIR,—Knowing you to be the ever strenuous supporter of our rights, and the expositor of our wrongs, I have to call your attention to a piece of illiberality practised on the lecturing pupils of the London Hospital by the two principal surgeons of that institution. The illiberality complained of is exhibited in the following regulation:—"That no pupil be allowed to subscribe to the library unless he enter to the hospital practice." About a year ago the law was put aside, but since the opening of the new room* it has again been brought into force, acting as a trap to induce pupils to enter sooner than their arrangements would have otherwise led them to do.

Publicity will no doubt effect the object I have in view, and therefore forbearing from further remarks,

I have the honour to be
Your obedient Servant,
A LECTURE PUPIL.

ST. BARTHOLOMEW'S HOSPITAL.

REMOVAL OF AN OSSEOUS GROWTH FROM THE UPPER JAW-BONE.

MARY ROSE, ætat. 50, admitted March 22nd, is a woman of strong constitution and enjoying excellent general health. On the left upper jaw there is a tumour springing from the alveolar process, on each side, to the extent of an inch and a-half forwards from the pterigoid process. This growth is of about six years' duration, and she ascribes its origin to the irritation caused by a decayed tooth. It flanks the molar teeth, which are partially embedded in, and overlapped by, its substance. The diseased mass has been the source of little pain since the first period of its growth, which has been very gradual. The inconvenience, however, caused by it in the functions of mastication and articulation, is extremely annoying to the patient, and she is quite prepared for undergoing an operation. As a preparatory step, Mr. Lawrence deemed it expedient that the embedded teeth should be extracted, since their fangs might interfere with the line of division. This was therefore accomplished on the 30th by Mr. Rogers, dentist to the hospital, who therein displayed much skill, making use of the *forceps*, the superiority of which over the key was in this instance peculiarly manifest. Saturday last was the day of operation, when, the patient being laid supine on the table, Mr. Lawrence commenced by making an incision on the affected side, from near the angle of the mouth across the cheek to the lower edge of the zygomao. The incision was curved, the convexity being upwards, avoiding the parotid duct. The arteries now began to spout forth most copiously, and some little time elapsed before they were secured; about a pint and a half of blood, we should think, being lost. Ligatures having been placed upon three, the operator proceeded with a scalpel to divide the soft attachments surrounding the bony growth, and then began to use the saw, apparently in an oblique line from the second bicuspid tooth towards the pterigoid process. After this the tumour was separated from the healthy bone with Liston's nippers, the division running just below the floor of the antrum. Some portion, or a spicula of bone, still remained, which however was quickly detached with an elevator. During these proceedings great inconvenience seemed to arise both to patient and operator, from the former being continually obliged to vomit forth coagula, which escaped into the passages during the hæmorrhage.

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* They were in debt 30*l.* at that time.

The edges of the incision were approximated and secured by a hair-lip suture at the angle of the mouth, and by three common sutures. Strips of plaster were placed in the intervals, and the patient was taken to Sitwell's ward; the operation, which was well performed, having lasted between thirty and forty minutes. The tumour was found to be of an osteo-sarcomatous description.

April 2nd. The patient since the operation has being going on very well. On Saturday she was somewhat troubled with the vomiting up of the blood; was ordered *pulv. opii*, gr. i. Yesterday, Sunday, she took two pills composed of—

Hyd. submur. gr. ij;
Ext. colocynth. comp. gr. viij.

Bowels opened by an enema. Pulse tranquil; no fever.

Thursday, 5th. Much the same. Says she feels comfortable. Wound looks well; it is healing nicely by the first intention. There is some heat about it, however, for which a piece of linen is applied wet with a cold lotion.

It is often very difficult for the great mass of the pupils to obtain, at these operations, a view either of the instruments used, or the mode of employing them, so great being the number of the INTRUDERS. Indeed it is here no uncommon circumstance to observe the surgeons and "recognised" assistants kept back during an operation, their places being usurped by persons at present entirely unconnected with the surgical practice of the hospital.

HOSPITAL TEXTS *v.* HOSPITAL PRACTICE.

Text.—"Morbid inspections, as opportunities occur; at *one* o'clock."—*St. Bartholomew Prospectus.*

Practice.—Case of fracture of the lower third of the femur, fracture of three ribs, and injury to the spine; admitted March 20th, death on the 28th. "Morbid inspection" at *twelve* o'clock; *no notice.*

Ibid.—Calculus in the bladder. Death on Monday, April 2; morbid inspection at half-past *eleven*: *no notice.*

CROWDING THE OPERATING TABLE.

To the Editor of THE LANCET.

SIR,—A regulation nearly to the following effect is hung over the table in the operating ward of St. Bartholomew's: "In order to prevent *interruption* at operations, the area round the table is preserved for the surgeons and the dressers of the operator *only*." This regulation is never adhered to, and indeed seems placed there as a mere mockery, for last Saturday it was so crowded that scarcely one half of the pupils could see what was going on, and after uselessly endeavouring for some time to remove the obstructions by crying out, "Heads," &c., a slip of paper was sent down requesting that the above regulation might be attended to, when a Dr. Harris, *not* I believe a *surgical* pupil, upon having it put into his hands, immediately tore it up, and deliberately scattered the remnants in our faces, as if in utter defiance of our requests and our rights. The pupils are not the only parties inconvenienced. The surgeon is inconvenienced, and last, though not least, the poor patient. Should anything occur during an operation not exactly looked for or expected, or should the surgeon ask for any instrument or thing not in the hands of the house-surgeon, great *interruption* must of necessity occur, and much time must necessarily be lost before the operator can be supplied with what he required to carry on the operation, owing to the assembled M.D.s, and Pures who are closely huddled together peeping over one one another's shoulders, standing between the operator and the table upon which the instruments are placed, so that the house-surgeon has to force his way backwards and forwards through them.

I am, Sir, your obedient servant,
March 20th. One of the Incommoded.

*** The proceeding of the "Dr Harris" was most impudent and indecent, but probably his nose was at the moment beyond the reach of either of the gentlemen to whom the insult was offered, and the "Doctor" felt that he could tear the note with safety.

GRIEVANCES OF THE PUPILS.

To the Editor of The Lancet.

Sir,—Some time ago I entered as perpetual pupil to the surgical and medical practice of St. Bartholomew's Hospital, thinking that by so doing I should be entitled to visit all the patients when either the surgeons or the physicians attended the hospital. Judge of my surprise then, when one day as I was with Mr. Lawrence in the out-patients'room, and he had finished seeing the *men*, and was about to commence prescribing for the *women*, he walked to the door, opened it, and observed, that as the ladies were about to make their appearance *he must request all gentlemen but the dressers to retire.*

Now, Mr. Editor, I should like to be informed why Mr. Lawrence acts so differently from the other medical men connected with the institution, and whether he has a right to compel the gentlemen to quit the room at his pleasure.

The other complaint I have to make, and which is one of much greater consequence to me, is the abolishment of the "casualty" patients who used to attend at the appropriate wards every morning from ten to one o'clock. I must confess that the atmosphere I had to breathe on those occasions was far from a pleasant one; but the information I obtained from them, (and which I was in the habit of doing daily,) amply repaid me for the temporary inconvenience. It is now, Sir, nearly three months since the doors were closed, at which time it was given out, that *in a few days a new and larger place would be ready for the reception of the indigent poor.* But, alas! day after day, week after week, and month after month are passing, and neither is a new casualty ward yet open; nor, as far as I can ascertain, is there any probability of there being one at present. Allow me to point out one of the consequences of this neglect. Why, the hospital square of a morning is thronged with poor people, who come under the expectation of receiving advice and medicines; some of them from a long distance, *all* of them *obliged* to return home with empty bottles, and suffering in all probability an aggravation of their diseases by the fatigue they have undergone. But this is not only inflicting a great cruelty on the poor, but it is an imposition on the whole body of pupils, more especially the dressers, whose duty it is to attend the patients in rotation, and for the promise of which they pay so dearly.

Pray, Sir, how are these evils to be remedied?

I am your obt. and humble servant,
One of the Aggrieved.

CORRESPONDENTS, &c.

Mr. H. The notice in our last was intended to have been accompanied by the letters in our present Number.

Sylvanus will find the best *practical* articles on the subject in Sir Astley Cooper's Lectures, and under the head of "Lumbar Abscess," in Mr. S. Cooper's Dictionary of Practical Surgery. With regard to the use of exercise and rest, the adoption of either must be governed by the stage of the disease.

We have not yet had time to peruse the work on the function of the spleen.

Maga is deceived. The papers have *not* been destroyed; they have only been unemployed.

An Hibernian. The charter is in full force; hence its acts retrospectively. Unfortunately, therefore, there is no remedy for Y. R., except in an alteration of the law itself. Had he a diploma from one of the continental universities, he might succeed in obtaining a license from that crumbilng institution in Pall Mall East.

Juvenis. The Apothecaries Act was passed in the spring of 1815, and came into operation on the 1st of August in the same year. The Company of Apothecaries and the Junto in Lincoln's-Inn-Fields are two distinct assemblies, possessing also entirely distinct powers,—one under a charter, the other under the act just mentioned.

Argus. It does not merit notice. Should the reptile thrust its head out of its hiding-place, so as to prove really annoying. an event not very probable, why then we may put our toe upon it, and crush it. The thing has venom, but no teeth.

THE LANCET.

VOL. II.] LONDON, SATURDAY, APRIL 14. [1831-32.

STEPHEN'S HOSPITAL, DUBLIN.

CLINICAL REMARKS

BY

MR. CUSACK,

ON A CASE OF CIRCUMSCRIBED BRACHIAL ANEURISM,

Cured by Compression and Digitalis.

Case.—J. M., æt. 26, a countryman, was admitted into this hospital on the 11th of October, under the care of Mr. Cusack, for circumscribed brachial aneurism at the bend of the right arm.

He states, that on the 29th of last month, being unwell, he applied to a "bleeder" in the country, for the purpose of having some blood taken from the arm. Nothing remarkable occurred during the operation, except that the blood was propelled to a much greater distance (13 feet) than usual from the wound. On the next morning the entire of the palmar surface of the upper extremity, from the wrist to the axilla was swollen, and of a purple colour. The bandage applied by the "bleeder" feeling tight, he caused it to be loosened, and directed the patient to keep the limb wet with vinegar and water. A week subsequent to this period, the swelling being much diminished, and the purple colour being in some parts changed to a yellow hue, he observed a small beating tumour immediately beneath the wound in the integuments. As this remained stationary, he applied, after some days, to a neighbouring surgeon, who at once detected the nature of the accident, and sent him up to this hospital. There is now (11th of October) a firm pulsating tumour, about the size of a pigeon's egg, in the site of the wound made by the lancet. Pressure on the brachial artery in the middle of the arm completely stops pulsation in the aneurism, and reduces it in size; there is considerable ecchymosis throughout the arm generally; the wound has not healed, and the skin around it appears thinned. The pulsations of the radial artery on this side are not near so strong as in the opposite arm.

A roller was applied, with a moderate degree of pressure, from the fingers up to the axilla, a compress of lint having been previously laid upon the tumour. He was desired to keep the limb around the tumour constantly wet with cold water. A state of perfect quietude was enjoined, and the patient was put on low diet.

15. The extravasated blood is almost removed; the tumour remains unaltered; roller reapplied; cold water to be continued, and the state of rest, and low diet, to be strictly observed.

17. Tumour firmer than at last report; the remainder of the limb has regained its natural appearance; the integuments round the wound are more firm. Ordered to have ℥xx of blood extracted from the opposite arm, and to take ten drops of the tincture of digitalis three times a day; low diet and cold water to be continued; bandage reapplied.

18. Pulse less frequent than yesterday; continue the digitalis, cold water, and low diet.

20. Pulsation in the tumour diminishing; it feels more firm, and does not subside when pressure is made on the brachial artery; wound healed; pulse 70; bandage reapplied, a piece of sponge being substituted for the lint. Continue remedies.

22. Pulsation in the tumour very indistinct; the tumour itself is firmer and smaller than at last report; pulse 66; sponge and roller reapplied; tincture of digitalis increased to twenty drops three times a day; cold water and low diet to be continued.

28. Pulsation in the tumour nearly gone; the swelling is diminishing very rapidly; pulse at the wrist more strong than at last report; roller and compress reapplied; dose of tincture increased to gut. xx, secundâ quâque horâ.

Ten P.M. Has taken six doses of the tincture since morning; pulse 40 and weak; not much giddiness of the head, but scintillæ are flying before the eyes. No sickness of stomach.

30. No pulsation in the tumour; the

skin over it quite sound, and not adherent to the swelling as it was at first; bandage and compress reapplied; digitalis to be continued.

November 2. Bandage reapplied yesterday; complains of its being tight; digitalis to be continued.

3. Tumour much diminished; the arm has regained its natural appearance; the pulse at the wrist strong and natural; digitalis to be omitted; roller reapplied.

21. From the time of the last report the tumour gradually subsided; there is now no trace of it. Dismissed the hospital cured.

Clinical Observations.

In performing the operation of venesection at the bend of the arm, there are a variety of ill consequences which will now and then ensue, even in the hands of the most skilful surgeon. The chief of these are, "inflammation of the vein," a "wound of the nerves," which in delicate females in good society is sometimes followed by the most distressing symptoms, indicative of great nervous irritation by "inflammation of the semilunar fascia of the biceps;" but the most important is the "wounding of the brachial artery," which latter circumstance may occur in one of the two following ways:—1st. By the vein and artery being transfixed by the same thrust of the lancet; or, 2ndly, by the vein rolling on one side, while the lancet passes through the integuments, and wounds the vessel without touching the vein. When the artery and vein are both wounded, a tumour sometimes forms between them, which is a diverticulum to the blood flowing in the artery, accompanied by a thrilling and hissing noise; this constitutes that form of aneurism which has been denominated "aneurismal varix." Or we may have no tumour formed between the artery and vein, but adhesion takes place between their coats at the points of the corresponding wounds, while the anterior wound in the vein corresponding to that in the integuments heals; there is thus a direct communication established between the artery and vein, which gives rise to that kind of aneurism which authors call "varicose aneurism." When the artery of the arm has been wounded directly, a "circumscribed aneurism" may be the consequence, as in the case we are about to consider; or the blood may be diffused extensively throughout the cellular membrane, causing enormous distension, which is generally greater above than below the elbow: this disease is the "diffused aneurism" of surgical writers.

In all cases of the kind which we are now considering, our attention should be directed to the external hæmorrhage, which we must repress; sometimes both to the external and internal hæmorrhage, that we may prevent the blood from becoming diffused throughout the cellular membrane. I shall on the present occasion confine myself to the history and symptoms of the present case, but I thought it right to premise so far, in order that I might be better understood by the junior members of the class.

The "bleeder," as he is called in the country, was aware of the nature of the injury he had inflicted, and of the consequences which would ensue; he therefore immediately applied pressure to the part, in order to arrest the hæmorrhage; yet the bleeding went on internally in a slight degree. Why was this extravasation slight, and why was not the blood diffused to a greater extent throughout the limb? The reason is, that the pressure employed, formed a cyst of the cellular membrane, which was circumscribed by the adhesive inflammation, and gave to the tumour as firm a pulsation as an artery. This is the least formidable kind of injury occurring after the operation of venesection.

In bleeding, a great deal of caution is necessary, and although we may cry out against a person who has the misfortune to wound the artery, yet we find the same accident happen in the hands of good surgeons. It remains for us to consider how far the treatment adopted in the present case has proved beneficial, how far the consequences have been satisfactory, and also to point out the nature of the disease, and the differences between it and a common encysted tumour.

We are not to imagine that every tumour which conveys to the touch the sensation of pulsation is an aneurism. How then would a surgeon know whether this was or was not a case of aneurism? By simply attending to the history of the case, and the nature of the symptoms above detailed; fluctuation was distinct; pressure on the artery above the tumour diminished it in size, and put a stop to all pulsation; by removing the pressure, the tumour returned to its original dimensions, and pulsation again became distinct; but if it were an encysted tumour, we should have had none of these symptoms. If you examine the state of the pulse at the wrist in the affected side, you will find it considerably weaker than in the opposite wrist, because the blood is diverted from its natural course into the aneurismal sac. This is not the case in encysted tumours. By raising them up with the fingers, we do away with all pulsation, which is a tremulous motion rather than an actual pulsation, but we do not affect the original size of the tumour; it is therefore necessary to make a careful survey of the symptoms, and of the history of the case, that we may be enabled to form a just and accurate diagnosis.

Treatment.—What mode of treatment would you adopt in a case of circumscribed aneurism? We find that a roller and compress, with everything which tends to diminish the impetus of the circulation, to obliterate the sac, and to prevent inflammation, are the best remedies we can adopt. In applying the bandage commence at the fingers, for if you neglect this, the hand will become swollen and œdematous, and you had better bandage each of the fingers separately, then include them in the roller which you intend to carry up the arm, and when you come to the bend of the elbow, place a piece of sponge on the tumour; let the pressure be light at first, if not, you are apt to excite inflammation. After this has been done, place dossils of lint in the line of the artery leading to the tumour, for the purpose of diminishing the impetus of the blood which flows through the artery and feeds the aneurismal sac.

The constitutional treatment will have no effect, unless you attend particularly to these circumstances. We are now to determine what this constitutional treatment should be. Perfect rest. Bleed the patient in the opposite arm, if repeated hæmorrhages have not taken place previously; for it often happens that hæmorrhage will occur ten or fifteen times, when the orifice of the wound has not closed—in such case the pulse will be 120, there will be great thirst, &c., and even though the pulse be hard and jarring, yet it would be improper to bleed him while labouring under this kind of fever, which has been called the hæmorrhagic. You should therefore bleed the patient, if the above circumstances do not contra-indicate it; confine him to a low diet, giving such food as will not increase the action of the heart and arteries, and which does not contain much nutriment. You should also administer the tincture of digitalis, if there be no organic disease; it reduces the frequency of the pulse, and is a valuable remedy when the heart or lungs are not diseased. In this man it produced the desired effect, for we find that when first administered, his pulse was as high as 112; but when he was brought under the effects of the digitalis, his pulse was reduced to 40, to that quiet state most favourable for the accomplishment of a cure; accordingly we find a manifest change takes place in the tumour, the blood becomes more firm in it, the tumour begins to subside, the pulse at the wrist becomes more obvious, and of its original standard. These latter effects do not take place until the tumour becomes strong and firm, because, previous to this change, the blood continues to flow into the aneurismal sac, but the recovery of the pulse shows it begins to take its natural course.

It is a remarkable thing, that he did not complain of giddiness in the head, or sickness of the stomach, but merely of scintillæ flying before his eyes; these are usual symptoms, and the surgeon commonly asks his patient if he feels any of them; after which lowering of the pulse ensues, which always proves successful, as it did in the present case. But we are to observe, that every case of this disease is not *one* fit for the above treatment. For example, if there be any appearance of inflammation in the tumour, the requisite pressure would only cause pain, and it would be folly to use it until the pain was reduced, and all inflammatory action in the tumour removed. You see, therefore, if you employ pressure in such cases as we have supposed, you would only aggravate the symptoms, and set aside all possibility of accomplishing a cure; it is therefore necessary to remove all pain, œdema, &c., consequent on the inflammation, before the means of radical cure are applied, otherwise the disease will go on from bad to worse, and you will have to regret the impropriety of your treatment when it is too late. I do not recollect a single case of circumscribed aneurism, which did not yield to the treatment I have described, and although an unpleasant disease, it is not so formidable as might, at first sight, appear.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

MADE BY

MR. BRODIE.

Saturday, March 31, 1832.

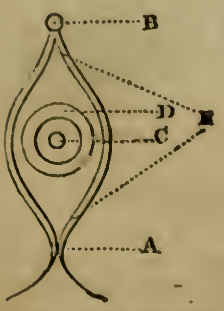
FRACTURE OF ROUND BONES.

It is said, remarked Mr. Brodie, that fracture of the olecranon takes place from the same cause as fracture of the patella, viz., from the violent action of the muscles. I do not believe this to be the case myself, for I have found frequently that in fracture of the olecranon there has been a severe blow upon the part. I do not think that the muscles of themselves possess sufficient power to effect it without. In the os calcis too it is said, that fracture is the result of violent muscular action; but I think that if this were the case, the tendon of the muscle would give way sooner than the bone.

DIVIDING THE SPHINCTER ANI.

Mr. Brodie performed this operation for the cure of fistula in ano. The instrument case was opened, and Mr. Brodie asked for a *straight probe-pointed bistoury*, when Mr. Fernandez, the house-surgeon, replied, *that there was not such an instrument in the hospital.* "Can such things be, without our special wonder!"

Mr. Brodie said, that the curved bistoury did not do in these cases; it cut more like a saw than a knife, and he (Mr. Brodie) was eventually obliged to perform the operation with *one of Blizard's lithotomy knives.* The muscle was completely divided, and the wound dressed with pledgets of lint dipped in Ruspini's styptic. Mr. Brodie afterwards remarked, "that formerly, in the treatment of fistula in ano, he experienced great difficulty in curing it, owing to the number of sinuses that formed. No sooner had he opened one than another presented itself; they seemed almost to be made by magic, till at last in those cases he divided the sphincter ani muscle quite through, and this completely cured the disease. There are other cases, however, gentlemen, (continued Mr. B.) in which you may divide the sphincter ani with very great advantage and benefit to your patient. There are cases where ulcers form at the *back part* of the rectum in the gut *above* the sphincter ani. These cause much trouble, pain, and inconvenience to the patient; but if you take a bistoury and divide the muscle through, you will give your patient immediate ease and relief, and gain great credit by the operation. In some cases, too, you will find the sphincter ani enormously enlarged. This arises generally from habitual costiveness, and the consequent necessity of taking violent purgative medicines. You will find this very common in women. I once attended a gentleman with such an enlargement of the sphincter ani. I divided the muscle, and it completely cured him. I used formerly to cut the muscle backwards, towards the os coccygis; but I then found some difficulty in getting the wound to heal. I now always divide the muscle *laterally*, which heals very well; and if you will give me a pencil and a piece of paper I will show you why." Mr. Brodie then sketched the following diagram:—



"You know that the gut is surrounded by two sets of muscular fibres, *the circular* and *the longitudinal.* Now suppose *A* to be the os coccygis; *B* the raphe of the perineum; and *C* to be the gut surrounded by *D* the circular, and *E* the longitudinal fibres. Well, if you divide the muscle posteriorly towards *A*, you divide the circular fibres of the sphincter; but your knife slips through between the longitudinal fibres of the muscle, but does *not cut them.* Now if you cut laterally, you will at once perceive that you must divide both sets of fibres, the circular and the longitudinal."

CONVULSIONS FROM INJURY OF THE HEAD.

April 2.—In the case of a patient who had received a blow on the head, which had been followed by convulsions for which he had been bled with relief, Mr. Brodie asked Mr. Fernandez of what such convulsions were symptomatic. "Laceration of the brain," answered Mr. Fernandez. "Not always," remarked Mr. Brodie, "for you will generally find that they are caused by extravasation of blood between the bone and the dura mater. Bleeding will always stop these convulsions; but, what is very singular, in some cases, where, from injury of the head unaccompanied by convulsions, the patient has been repeatedly bled and brought low, you will find them come on, and if you continue to bleed, they will become worse; but if you stop the bleeding and give the patient nourishment they will go off." There was a case in this hospital, under the care of Mr. Keate, where the patient had convulsions. Mr. Keate trepanned the skull over the spot where the blow was received; and a small portion of blood, about a teaspoonful, was found extravasated under the bone, between it and the dura mater.

BLEEDING FROM THE EAR.

In a case of this nature, Mr. Brodie asked Mr. Fernandez where, in such cases, the blood came from. Mr. Fernandez very sagaciously, in answering the question, said that it depended upon whether the blood was venous or arterial. In the truth of this remark, Mr. Brodie perfectly coincided, adding, "that if the blood discharged from the ear were arterial, it would then come from the tympanum—if it were venous, it would come from the lateral or the cavernous sinus."

INJURY OF THE SPINAL MARROW.

In injuries of the spinal marrow, gentlemen, it is very remarkable, but patients will not bear bleeding as they will in injuries of the brain. Paralysis will not come on in some cases till some days after the accident has been received. I knew a gentleman who received a violent fall, he walked home very well, and in him the paralysis did not come on till three weeks afterwards; in general, also, you will find that the urine does not become alkaline till some time after the injury.

SALIVARY FISTULÆ.—AMPUTATION OF THE LOWER JAW.

I used formerly to make setons for the cure of *salivary fistulæ*, but I find that they are best cured by the mild application of lunar caustic, so as to form a very slight slough, and in two cases out of three this is the practice which I now follow, and I find it succeed very well. These remarks were made by Mr. Brodie in reference to a patient whose lower jaw he amputated a few weeks since for fungus. The operation was performed on the 5th of March; the man has not had one bad symptom since, with the exception of a slight cough, which soon got better on the application of a blister to the chest. The wound has united very readily by the first intention, with the exception of a slight fistula, which is being touched with caustic every other day. After the performance of the operation, Mr. Brodie remarked, that "after amputation of the lower jaw, the patient's situation is not so very deplorable as might at first be imagined, for that in process of time (about three months) a ligamentous band of union is generally formed connecting the cut ends of the bone with one another, in the same way as in fracture of the patella. (The man was this day made an out-patient; there is very little deformity to be seen, and the case is one reflecting the greatest credit on the skill and science of the operator.)

FORMER CASES.

*** The non-syphilitic case of disease of the bones of the nose (which was detailed in a former Number) is greatly improving under the use of mercury. The patient's mouth has been affected, and the disease is gradually disappearing.

*** In the cases of hysterical affection of the knee-joint, and of aneurism of the aorta, which were formerly adverted to, the first of these has got well under the use of valerian; the latter patient expressed a wish to leave the hospital, adding that she would rather die at home, but that she hoped her body would be examined after her death.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXV.

LARYNGITIS—TRACHEITIS—ROARING.—THE VARIOUS CAUSES OF ROARING.—TREATMENT.

Inflammation is not long confined to the upper air-passages; other portions of the membrane are gradually involved. Here are several specimens of laryngitis. The membrane of the arytenoid cartilages, and of the whole of the larynx, are here deeply ulcerated. We gradually lose the inflammation in the trachea, but the horse died from the effect of general irritation, the source or focus of which was in the larynx.

Symptoms of Laryngitis.—It is only lately that laryngitis has been recognised and described as a distinct disease by the practitioner of human medicine. It is not then extraordinary that we have heard so little about it in the horse. Here is a proof of its existence in that animal, and as a distinct disease; but it has not been examined with sufficient frequency or care, to enable me, so satisfactorily as I could wish, to describe its distinguishing symptoms. Its approach is often insidious, scarcely to be distinguished from catarrh; but with more soreness of throat, and less enlargement of the parotid glands. There are more decided and violent paroxysms of coughing than in common catarrh, attended with a gurgling noise, heard at a little distance from the horse, and by auscultation decidedly referable to the larynx. The breathing is shorter and quicker, and evidently more painful than in catarrh; the membrane of the nose is redder; it is of a deep modena colour, and the horse shrinks and exhibits great pain when the larynx is pressed upon. The paroxysms of coughing become more frequent and violent, and the animal appears at times almost suffocated.

The value of Auscultation.—And now, Gentlemen, permit me to direct your attention to that with regard to which veterinarians have been strangely silent—of the aid of which comparatively few avail themselves,—but which I have no hesitation in affirming to be one of the most valuable of all the means we possess of discovering the seat, intensity, or results of the diseases of the respiratory passages,—I mean auscultation, or the application of the ear to the thorax or the throat of the patient, and the observation of the sound emitted by the passage of the air through different portions of them. The stethoscope would be an inconvenient instrument for us,—we could not perfectly adapt it to any part of the surface we wish to examine. The side of the face and the ear we can accurately adapt to any part.

Then what we have first to do, and that with which I would urge you to make yourselves perfectly acquainted, is the sound produced by the rush of the air through the various passages, when the animal is in health and quiet; and varying in each according to the caliber of the tube, its situation and depth from the surface. The sound of health being known, a departure from it will be immediately recognised, and experience will gradually teach us the disease, or the degree of disease, which, affecting the caliber of the tube or the nature of the secretion, produces a variation in that sound.

The Respiratory Sound belonging to the Trachea.—The healthy sound proceeding from the trachea is that of a body of air passing uninterruptedly through a smooth tube of somewhat considerable caliber. I know not what better to liken it to than the sound of your forge-bellows, when not too violently worked. Indeed it is the very sound, somewhat varying however in different horses, according to the size of the windpipe, and most certainly varying if the horse is, although in a very slight degree, disturbed. Gentlemen, do not treat this with the derision and contempt with which I am ashamed to say I have seen auscultation regarded by some, and so regarded because they knew nothing about it. Put it to the test, I ask no more.

Applied to Laryngitis.—I will suppose that you are acquainted with the healthy sound. You apply your ear to the wind pipe, the base of it, and the centre of it, and you find the bellows are moderately working. The air rushes in and out without interruption. There is no disease there, and there is no disease below. You gradually creep up towards the head, and you soon begin to recognise a little gurgling rattling sound; and as you approach the larynx it is more decided, mingled with an occasional whizzing, whistling noise. What surer proof can you have that here is the impediment to the passage of the air, proceeding from thickening of the membrane and diminution of the passage, or from increased secretion of mucus which bubbles and rattles as the air passes? By the degree of rattling or of whistling you will judge which cause of obstruction preponderates. In fact you have discovered the seat and the state of the disease. Try this for yourselves, and value or discard this mode of exploration, according to the result.

Treatment.—We have then evident inflammation of the larynx, and the course to be pursued is sufficiently plain. We must bleed, and that from the jugular; we shall then have the combined advantage of general and local bleeding. We must bleed copiously; depending indeed upon the degree of inflammation, but yet copiously, for we have to do with inflammation of a *mucous membrane*; and that which we do we must do quickly. We shall not have opportunity when the disease has altered its character, and debility succeeds. We must do that which I have already urged upon you, and shall again and again, not make up our minds to any determinate quantity of blood, and leave our assistant or the groom to abstract it, but to bleed ourselves, and until the pulse flutters, or the constitution is evidently affected.

Next we must give the fever medicine already recommended; the digitalis, nitre, and emetic tartar, with aloes. We may give aloes safely, because we have ascertained that the chest is not yet implicated; and to this we must add, and immediately, a blister, and a sharp one too. We are sure of the part, and we can bring our counter-irritant almost into contact with it.

Necessity of prompt Measures.—Inflammation of the larynx, if not speedily subdued, produces sad disorganisation in this curiously formed and important machine. Lymph is effused,—morbidly adhesive, and speedily organised; the membrane becomes thickened, considerably, permanently so; the submucous cellular tissue becomes œdematous; the inflammation spreads from the membrane of the larynx to the cartilages, and so we shall have difficulty of breathing, and at length confirmed roaring.

Inflammation of the Trachea.—Inflammation of the membrane of the larynx, and especially having run on to ulceration, as you see in this specimen, may rapidly spread and involve the greater part or the whole of the lining membrane of the trachea. Auscultation will tell you when this is taking place, for the free and uninterrupted, but sonorous rush of air, will be changed for wheezing and rattling, and that in proportion as the cause of either sound exists. If the disease is extending down the trachea, you must follow it; your blister must reach as low as you can detect the rattling sound, and something beyond; your fever medicines must be administered in somewhat increased doses, and your bleeding must be repeated, if the state of the pulse does not indicate the contrary.

Generally speaking, however, although the inflammation is now approaching the chest, its extension into the trachea is not an unfavourable symptom. It covers a more extended surface, and this is not so intense or untractable; it is involving a part of the frame less complicated, and where less mischief can be effected. True, if you neglect the case, it is showing a disposition to spread that must terminate fatally; but it is coming more within your reach, and more under your command, and the proper means being adopted, the change is rather a favourable one.

The disorganisations produced in the trachea are similar to some which I have described in the larynx. The same formation of organised bands of coagulated lymph, the same thickening of membrane, diminution of caliber, and foundation for roaring.

ROARING.

This will be the proper place to speak of that singular impairment of the respiratory function, recognised by the name of roaring. It is an unnatural, loud, grunting sound made by the animal in the act of breathing in quick action, or on any sudden exertion. If the horse is briskly trotted on a level surface, and more particularly if hurried up hill, or if he is suddenly threatened with a stick, this peculiar sound will be heard, and cannot be mistaken. When dishonest dealers are showing a horse that roars, but not to any great degree, they trot away gently, and as soon as they are too far for the sound to be heard, show off the paces of which the animal may be capable, and on returning gradually slacken their speed when they come within a suspicious distance. This is sometimes technically called " the dealers' long trot." On carefully listening to the *sound*, it will appear that the roaring is produced in the act of inspiration, and not in that of expiration.

It is Unsoundness.—Roaring is a very unpleasant thing to the rider, and it is unsoundness too. It is the sudden and violent rushing of the air through a tube of diminished caliber, and if the impediment, whatever it is, renders it so difficult for the air to pass in somewhat increased action, sufficient air will not be admitted to give an adequate supply of arterialised blood in extraordinary or long-continued exertion. Therefore, as impairing the function of respiration, although sometimes only, on extraordinary occasions, it is unsoundness. In as many cases as otherwise it is a very serious cause of unsoundness. The roarer, when hardly pressed, is often blown even to the hazard of suffocation. There are cases on record of his suddenly dropping and dying, when urged to the top of his speed.

Roaring does not always materially injure the Horse.—We must not however take it for granted that the roarer is never good for anything. There are few hunts in which there is not a roarer who acquits himself very fairly in the field; and it has so happened that the roarer has been the very crack horse of the hunt; yet he must be ridden with judgment, and spared a little when going up hill. There is a village in the West Riding of Yorkshire, through which a band of smugglers used frequently to pass in the dead of night, and the horse of the leader, and the best horse of the troop, and on which his owner would bid defiance to all pursuit, was so rank a roarer that he could be heard at the distance of a quarter of a mile. The clatter of all the rest scarcely made so much noise as the roaring of the captain's horse. When this got a little too bad, and he did not fear immediate pursuit, the captain used to halt the troop at some convenient hay-rick on the roadside, and having suffered the animal to distend his stomach with this dry food, as he was always ready enough to do, he would remount and gallop on, and, for a while, the roaring was scarcely heard. I am not bound satisfactorily to account for this, but the loaded stomach now pressing against the diaphragm, that muscle had harder work to displace the stomach in the act of enlarging the chest, and producing an act of inspiration, and accomplished it more slowly, and therefore, the air taking longer time to rush by, the roaring was diminished. I will not stop to calculate what must be the increased labour of the diaphragm in moving the loaded stomach, nor how much sooner the horse must be exhausted. This did not enter into the owner's reckoning, and probably the cruel application of whip and spur would deprive him of the means of forming a proper calculation of it.

Eclipse was a " high-blower." He drew his breath hard, and with apparent difficulty. The upper air-passages, perhaps those of the head, did not correspond with his unusually capacious chest; yet he was never beaten. It is said that he never met with an antagonist fairly to put him to the top of his speed, and that the actual effect of this disproportion in the two extremities of the respiratory apparatus was not thoroughly tested.

Causes of Roaring.—Then roaring proceeds from obstruction in some portion of the respiratory canal, oftenest in the larynx, for there is least room to spare, that cartilaginous box been occupied by the mechanism of the voice; next in frequency it is in the trachea, but, in fact, obstruction any-where will produce it. Blaine, quoting from the French journalists, says that a piece of riband lodged within one of the nasal fossæ produced roaring, and that even the displacement of a molar tooth has been the *supposed* cause of it. Polypi in the nostrils have been accompanied by it. Mr. Sewell found, as an evident cause of roaring, an exostosis between the two first ribs, and pressing upon the trachea; and Mr. Percivall goes further, and says that his father repeatedly blistered and fired a horse for bad roaring, and even performed the operation of tracheotomy, and at length the roaring being so loud when the horse was led out of the stable that it was painful to hear it, the poor animal was destroyed. No thickening of the membrane was found, no disease of the larynx or trachea, but the lungs were hepatised throughout the whole of their substance, and many of the smaller divisions of the bronchiæ were so compressed that they were hardly pervious.

Bands of Coagulated Lymph.—A frequent cause of roaring is the bands of coagulated lymph, of which I have already spoken, morbidly viscid and tenacious from disease, adhering firmly on one side, and by some act of coughing brought into contact with the other side, and there adhering, and vessels running into them from the membranes, and they thus becoming gradually organised. At other times there have been rings of coagulated lymph adhering to the lining of the trachea, but not organised. In either case they form a mechanical obstruction, and will account for the roaring noise produced by the air, in somewhat hurried respiration, rushing through the diminished caliber. Here is a specimen of the transverse bands.

Tracheotomy as a Method of Cure.—A veterinary practitioner resorted to the operation of tracheotomy, to find out and get rid of this band. He fortunately cut down upon it, and removed it, and the horse was no longer a roarer. The case was published, or gradually became known, and many others resorted to the same operation, but without success. Mr. Percivall bears a little too hard upon this operation and the operator, and says that "all the credit due to the first operator, arose out of the dexterity he evinced in the operation; and that no little discredit was due to those who were silly enough to suppose that they might make a similar discovery wherever they chose to cut a hole into the windpipe." Now with submission to my valued friend, I do think that if the cause of roaring exists in the trachea, we may discover the very seat of it, and "cut a hole into the windpipe" directly upon it. We have that almost unerring guide, auscultation, which was unknown when his excellent lectures were penned. We can in a manner assure ourselves of the spot whence the sound proceeds, and, consequently, where the obstruction exists. We may not find a band there, it may only be thickening of the membrane, but in obstinate and very bad cases the experiment may be worth a trial.

Thickening of the Membrane.—I have just alluded to this as a cause of roaring, and I am inclined to believe that it is a more frequent cause than the transverse bands, or coagulated rings. Observe this specimen. Here is sad ulceration, and considerable thickening. Look at this; there is no ulceration, but the membrane is thickened to a very great degree. It is double or treble its natural depth. The caliber of the trachea was here materially contracted. Look at this third preparation. Here is not so much thickening, but it is where it can least be borne. It is on the membrane covering the chordæ vocales. It lessens a passage already narrow enough, and it interferes with the requisite freedom of motion of these delicate portions of the mechanism of voice. If, for ordinary purposes, they can still contract or dilate the entrance into the body of the larynx, they cannot sufficiently expand the opening when an additional supply of air is requisite in extraordinary exertion. It is thus perhaps that roaring is the occasional consequence of strangles and catarrh, and other affections of the superior passages; and even as the result of pneumonia, this sensitive membrane is more likely to be inflamed and indurated than that which lines the tube of the trachea generally.

Ossification of the Cartilages.—There is scarcely a horse of five or six years old who has not a portion of the thyroid cartilage ossified; in some cases the greater part of the cartilages are becoming bony, or sufficiently so to weaken or destroy their elastic power, and consequently to render it impossible for them to be freely and fully acted upon by the delicate muscles of the larynx. The cartilages no longer freely moving, the air-passages cannot vary in size as the circumstances of the animal may sometimes require. Chronic cough occasionally terminates in roaring. It is as likely to be inflammation of the membrane of the larynx as any part of the upper respiratory canal; and the membrane cannot be subject to chronic inflammation, without that inflammation being propagated to the cartilages, the natural consequence of which is the deposition of bony matter in them.

Coughing the Horse.—Some have imagined that the dealers' habit of *coughing* the horse, i. e. pressing upon the larynx to make him cough, in order that they may judge of the state of his wind by the sound of the cough, has produced inflammation about the larynx, which has terminated in or assisted in producing roaring. That pain is given to the animal by the rough and violent way in which the object is sometimes attempted to be accomplished, is evident enough, and this must in process of time lead to mischief; but I can scarcely think sufficient inflammation and subsequent ossification of the cartilages to be a cause of roaring. The foundation of roaring, the ossification of the cartilages, previously existed, and produced the difficulty of compressing the larynx, and thus exciting the horse to cough.

The Disease of Draught-Horses generally.—There can be no doubt of the fact that the majority of roarers are draught-horses, and horses of quick draught. They are not only subject to the usual predisposing causes of this obstruction, but there is something superadded, something resulting from their habits or mode of work,—not indeed necessarily resulting, but that which the folly as well as cruelty of man has introduced, I mean the system of tight-reining, the continued and painful pressure upon the larynx, by reining in the head of the horse, and teaching him to bear himself well.

The Mischief of Tight-Reining.—I acknowledge that to a certain extent the curb-rein is necessary; without it we should have scarcely any command over a wilful horse, and it would need a strong arm always to guide even the most willing. Without the curb-rein the horse would carry himself low, he would go carelessly along, he would become a stumbler, and if he were disposed at any time to run away, the strongest of us would have no power to stop him. But there is no necessity for the tight rein, and for the long and previous discipline to which the carriage horse is subjected. There is no necessity that the lower jaw, whether the *channel* be wide or narrow, should be so forced on the neck that the larynx and the portion of the windpipe immediately beneath it should be flattened and bent and twisted in the strangest way, and the respiratory passage not only obstructed, but in a manner closed. Here we have a plain and palpable cause of roaring. The mischief is usually done when the horse is young. It is done in some measure by the impatience of the animal unused to control, and suffering pain. In the violent tossing and return of his head he bruises the larynx, and produces inflammation, while the constant pressure would flatten and distort. The head of the riding horse is gradually brought to its proper place by the hands of the breaker, who skilfully increases or relaxes the pressure, and humours and plays with the mouth; but the poor carriage horse is confined by a rein that never slackens, and his nose is bent in at the expense of the larynx and windpipe; and the injury is materially increased if the head is not naturally well set on, or the neck thick, or the jaws narrow. Here are strange and almost incredible distortions of the larynx. Here are also some engravings of the larynx in its natural and deformed state, which do credit to the ingenuity of Mr. Sewell.

Crib-biting supposed to be a Cause of Roaring.—Connected with this is the common notion that crib-biting is a cause of roaring. That is altogether erroneous. There is no possible connexion between the complaints: but one of the methods that used to be resorted to in order to cure crib-biting might be a cause of roaring, namely, the strap so tightly buckled round the upper part of the neck as to compress, and distort, and paralyse the larynx.

The Absorption of the Muscles of the Larynx.—Here, Gentlemen, is a very singular preparation. It is the larynx of a roarer. You perceive the muscles of the larynx on one side as large as usual or even larger; on the other side they have dwindled away and almost vanished. There is a little distortion of the larynx here, but no twisting, or it is out of its place, so that the muscles of one side could not act, and were absorbed for want of use. How do we account for this? Why, loss of muscular power and consequent wasting of muscular substance may be the result of the loss of nervous influence as well as the interposition of some mechanical impediment to the action of the muscles, and the French have a theory to which specimens like this give an appearance of probability.

Deficiency of Nervous Influence.—I must here recall your attention to what I stated in a former lecture of the source whence the muscles of the larynx derived their power. We threw these muscles into two classes, the dilators and the constrictors;* they were both supplied with nervous influence from the par vagum, or eighth pair of nerves, one of the respiratory nerves of Sir Charles Bell, and the pneumo-gastric nerve (a better name) of the French. But they were supplied from different branches of this nerve, the constrictors from the laryngeal branch, and the dilators from the recurrent nerve. French physiologists imagined that if the pneumo-gastric nerve could be compressed at any part between the laryngeal and recurrent branches, the power of the dilators would be taken away on that side, and the constrictors acting on both sides, roaring, from the partial obliteration of the air-passage, would be the inevitable result. In conformity with this they have fancied that they have discovered, in horses that had been destroyed for roaring, little ganglions pressing upon the pneumo-gastric nerve at different parts of its course down the neck, and depriving the recurrent nerve on one side of its power.

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* Temporary indisposition prevented a personal revise of the report of the Lecture alluded to, as seen in THE LANCET of Feb. 18th; and from the singular transposition of the terms constrictors and dilators, a totally different view was given of the distribution of these two branches of the eighth pair of nerves.

I am not aware that any English veterinarian has observed this, or indeed put himself in the way of observing it. It is a very ingenious theory, and deserves serious investigation. It is possible that the pressure of the collar, as well as that of the lower jaw, may on this supposition have something to do with roaring.

We cannot, however, account for the absorption of *all* the muscles, plain enough in this specimen, from the loss of power in the recurrent nerve; the constrictors ought to be bulky and florid. We must leave this appearance unexplained, unless we have recourse to the pressure of some ganlion or other body *above the origin of the laryngeal branch*, of which there is no proof.

These cases of absorption of the whole of the muscles on one side, while the larynx remains perfect, are exceedingly rare. In the far greater number of cases there is much distortion, rendering the muscles on one side useless, and therefore causing them to waste away; and even here, if we look at the inside of the larynx from below upwards, there the distortion is plain enough. The wasting of the muscles, therefore, is the effect, and not the cause of that which produces roaring.

I might add to this long list of supposed causes of roaring the opinion of Mr. Parkinson, who says that "the complaint is said to be in the kidneys, which seems probable from the symptom produced by a stroke across the loins, which will make him roar out; the lungs are also affected, as the wind is so bad. There is no cure, though the horse will live long and eat much, but he will do scarcely any work, and therefore is worth little more than for dogs." You may form your own opinion of this, but I am assured that you will agree with me as to the last cause of roaring, which I shall mention, and a far more prevalent one than is suspected.

Hereditary Predisposition.— Facts have established this beyond the possibility of doubt. In France it is notorious, that three-fourths of the horses from Cottentin are roarers; some of them are roarers at six months old: but about La Hague and Le Bocase, not a roarer is known. There is certainly a considerable difference in the soil of the two districts; the first is low and marshy, the latter elevated and dry: but tradition traces it to the introduction of some foreign horses into Cottentin, who bequeathed this infirmity to their progeny, and the curse is not worn out.

In our own country we have as decisive a proof. There was a valuable stallion in Norfolk, belonging to Major Wilson of Didlington. He was a great favourite, and seemed to be getting some excellent stock; but he was a roarer, and some of the breeders took alarm at this. They had occasionally too painful experience of the communication of the defects of the parent to his progeny; and they feared that roaring might possibly be among these hereditary evils. Sir Charles Banbury was requested to obtain Mr. Cline's opinion on the subject. Mr. Cline was a deservedly eminent human surgeon; he had exerted himself in the establishment of the veterinary college; he was an examiner of veterinary pupils, and therefore it was supposed that he must be competent to give an opinion. He gave one, and at considerable length. "The disorder in the horse," said he, "which constitutes a roarer, is caused by a membranous projection in a part of the windpipe, and is the consequence of that part having been inflamed from a cold, and injudiciously treated. A roarer, therefore, is not a diseased horse, for his lungs and every other part may be perfectly sound. The existence of roaring in a stallion cannot be of any consequenee. It cannot be propagated any more than a broken bone or any other accident." A fair specimen of the *horse-knowledge* of one of the best of those nondescript incomprehensible beings, *medical* examiners of *veterinary* pupils!

Sir Charles returned full of glee; the good people of Norfolk and Suffolk were satisfied; Major Wilson's horse was in high request, and in a few years a great part of the two counties was overrun with roarers, and many a breeder half ruined.

Treatment.— Well, then, Gentlemen, what are we to do with these roarers? Abandon them to their fate? No, not so; but we must take care of our own reputation, and not rashly undertake a hopeless affair. In the first place we must obtain all the knowledge we can of the origin of the disease. Did it follow strangles, catarrh, bronchitis, or any affection of the respiratory passages? Is it of long standing? Is it now accompanied by cough or any symptom of general or local irritation? Can any disorganisation of these parts be detected? Any distortion of the larynx? Did it follow breaking-in to harness? The answer to these questions will materially guide our course. If there is plain distortion of the larynx or trachea, or the disease can be associated in point of time, with breaking-in to harness, or the coachman or proprietor has been accustomed to rein him in too tightly or too cruelly, or the sire was a roarer, have nothing to do with the case. Do not be too eager to undertake the affair, if it is a case of long standing, although clearly referable to some previous disease. But if it is of rather recent date, and following closely on some disease with which it can be clearly connected, commence another careful examination of the patient. Is there cough? Can any heat or tenderness be detected about the larynx or trachea? Follow with your ear the course of trachea. Is there every-where the same uniform rushing noise, or on some particular spot can you detect a more forcible rushing, a whizzing or whistling, or a rattling and guggling? Is that whizzing or rattling either confined to one spot, or less sonorous as you recede from that spot above or below, or is it diffused over a considerable portion of the trachea?

You would, I think, bleed, purge, and most certainly blister. Your ear will guide you to the part to which the blister should be applied. The physic having set, you would commence a course of fever medicines; you would consider it as a case of chronic inflammation, and to be subdued, not by excessive depletion, but by the continuance of moderate depletory measures. You would probably bleed again in less quantity; give a second dose of physic, and most certainly blister again, or keep the blister discharging by some stimulating unguent. The degree of success which attends these measures would guide you in pursuing them further. If no relief is obtained after a fortnight or three weeks, you would ponder on another mode of treatment. You would again carefully explore the whole extent of the trachea, and if you could yet refer the rattle or whizzing to the same point, you would boldly propose *tracheotomy* in despite of the sneer of the talented author of "The Elementary Lectures," for you would certainly cut upon the seat of disease

If you found one of these organised bands, the removal of it would afford immediate relief; and if you found merely a thickened membrane, no harm would be done; or the loss of blood might abate the local inflammation. You will recollect what was said in a former lecture of the manner in which the operation of tracheotomy should be performed. You should not perfectly cut through a single ring, much less two or three; if you do, the arch of the trachea will be destroyed—it will there assume a sharper, roof-like shape; the caliber of the tube will be materially lessened there, and if you should perchance remove one cause of roaring, you will assuredly produce another, and that a remediless one. Once more look at these tracheæ on which the operation has been performed, and see the altered shape of the tube, and the certain production of roaring. If a portion only of two of the rings is excised, the remaining part of each will be sufficiently strong to retain the perfect arch-form of the trachea, and without impairment of the caliber.

Of the manner of introducing a tube into the trachea, in case of much obstruction, I have already spoken. On the whole you will not eagerly undertake a case of roaring, but having undertaken it, you will give the measures that you may adopt a fair trial, remembering that, in every chronic case like this, the only hope of success depends on your perseverance.

ON THE
PATHOLOGY AND TREATMENT
OF THE
MALIGNANT CHOLERA.

By Joseph Ayre, *M.D. &c. &c.*

The importance of the subject upon which I am about to write must be my apology for the length to which my remarks will extend. The subject I am about to consider is the pathology and treatment of the English cholera, as known and met with in this country, intending to show that such a resemblance subsists between the English and Asiatic form of the disease, as is sufficient, for all practical purposes, to prove an identity in their pathological conditions, and in the treatment required for them.

The notion entertained by many concerning the nature of the cholera morbus of European countries is, that it essentially consists in a morbidly-increased secretion of bile, which is more or less vitiated, and which serves as the direct cause of all the vomiting and purging present in the complaint; that the sudden and sometimes formidable failure of the vital powers attendant on the severer forms of the disorder, is solely the result of an exhaustion induced by the vomiting and purging, and by the abduction from the body of copious secretions, united with the irritations which those secretions produce on the first passages. That this, however, is not a correct notion of the nature of the cholera of this country, may be affirmed from the fact that the vomiting and purging belonging to it, may exist for many hours before the appearance of bile in the discharges, and of course before there is a copious secretion of it to cause them; for it would inevitably be discharged if secreted. In the same way the sunken and exhausted state is met with simultaneously with the first occurrence of the vomiting, and often before any purging is commenced. It is also well known that the most copious vomitings occur from seasickness, and the most profuse dejections of bile and of watery secretions from the bowels, under the use of calomel and of drastic purgatives, and which often exceed very greatly what are met with in cholera, and yet without any appreciable diminution of the strength, or signs of exhaustion. The prostration, in fact, of cholera comes on in many cases very suddenly, and consentaneously with the vomiting, and often before the purging; and in the same manner it passes away or greatly lessens upon the discharges becoming coloured with the bile. It may be also stated that there is no correspondence in degree in the intensity of the vomiting between the cases of a mild kind and those of severer forms of it in which there is the most alarming failure of the vital powers. We must therefore look to some other cause for the sudden diminution of vital power met with in this complaint, and this cause I now proceed to show, is to be sought for in the peculiar condition of the liver, and not in an increased secretion of its bile, but in a partial and sometimes in a total suppression of it.

Now the circumstances under which the liver is placed are, it is well known, of a peculiar kind, for the venous blood returning from the abdominal viscera to the heart, is appointed in its passage thither to circulate first through the secretory vessels of the liver, as the *pabulum* of the biliary secretion. A portion of the venous blood, and probably some noxious principles of it, are designed to be separated from the mass in the form of bile by vessels destined for that purpose. Any interruption to the due performance of this function will necessarily occasion an accumulation of blood in the *vena portarum* and its branches, and an impeded circulation of the venous blood through the liver will follow, producing a congestive state of its venous system; and that congestion will follow, in cases where secretion is checked or interrupted, we have the amplest reason from analogy to conclude. The female breast, for instance, falls readily into a state of congestion upon any interruption occurring suddenly in its secretory functions, though the congestion in this case is primarily arterial, and the effect inflammation, whilst in the instance of the liver, the congestion consequent upon its interrupted secretion is necessarily venous, and the immediate effect an increased disorder in its secretory system; and where the suppression of the secretion is complete and sudden, a similar congestive state will follow of those abdominal organs whose venous system is associated with the venous circulation of the liver.

Now the congestion thus produced in the portal circle of the liver, constitutes the first stage of the cholera morbus of this country, and forms the essence of it in all; and though with us the congestion is usually slight, because the interruption given to the action of the liver is partial and temporary yet it forms in every case that state of the disorder in which there is the most danger, giving rise in its severest form, as met with here, to a considerable disturbance of the nervous system, the pulse becoming small and compressible, and often intermitting; vomiting, and sometimes purging; urine scanty and high-coloured; oppression about the chest; the extremities cold and sometimes livid, and occasionally affected with cramps; the countenance sunk, with sometimes a blueness about the mouth, particularly in children, with a general depression of vital power, which may be appropriately termed a state of collapse. In its most intense form, and when occurring under circumstances of the system favourable for its influence, the vital powers may become so overwhelmed as to perish in the struggle.

In the ordinary cases of an interruption in the secretory action of an organ there is a provision made by nature for its relief. The congestive and inflamed state of the female breast, to which a sudden interruption in its secretory action gives rise, if not removed artificially by the local abstraction of blood, or by other means, or naturally by a renewal of its proper secretion, terminates in suppuration, by which the congestive state of its arterial circle of vessels is removed, a termination is put for a time to its secretory function, which is from its nature only temporary, and a healthy state of the organ is at length induced. But suppuration is the result alone of arterial action, and cannot be produced by the vessels of the *vena portarum*, whose character and structure are venous, and which, as far as our observation extends, are incapable of taking on an inflammatory action. But a permanent interruption to the secretion of the bile, and the consequent congested state of its secretory vessels, are conditions incompatible with life. Relief to this state is therefore necessary to preserve it, and there are two principal modes, by one or the other of which more or less of relief is afforded. The first in importance and value is a sudden reaction taking place in the secretory vessels of the liver, to which the spontaneous vomiting of the complaint frequently contributes, and by which a copious secretion of bile is produced. This is the natural remedy of the complaint, although often erroneously regarded as the complaint itself, because the overflowing bile by accidentally rising into the stomach, prolongs for a time that irritable condition of it, which the previous congestion of the liver had induced. Where this reaction of the secretory vessels of the liver does not come on, the second mode may ensue, which is of less efficacy, and which consists in a reaction in the arterial circle of the liver, to which the congestion of the veins affords an indirect stimulus, and an inflammatory or excited state becomes established, forming in this, as in other cases of venous congestion, a partial but morbid remedy. This inflammatory state set up in the vessels of the liver, and sometimes in those of the other abdominal viscera, gives rise to an excited state of the heart and arteries, and which is known by the name of bilious fever, being distinguished by a continued interruption in the secretory action of the liver, with an abatement of those symptoms which pertain to the stage of congestion or collapse.

We have thus far confined our views to the pathology of cholera as limited to the complaint known by this name, but the term of cholera so restricted is too limited in its application. We have just noticed that the disorder is not ended upon the termination of the vomiting and purging, and the other symptoms of collapse, but that when a reaction does not take place in the secretory vessels of the liver, and which is its most usual termination, that an inflammatory action comes on in the arterial circle of that organ, giving rise to the fever denominated bilious. As the cholera morbus, therefore, may thus exist after the supposed pathognomonic signs have disappeared, so it will be found to be present in numerous cases before the more obvious and recognised signs of it have come on. There is in fact a chronic form of the complaint which may precede for several days and even weeks the more active stage of it or in that of cholera proper, and either terminate suddenly in a reaction of the secretory vessels, or pass into the inflammatory stage, without the supervention of the active state of it. This, the chronic, varies in its degrees of force, as the degrees of venous congestion vary. It is often accompanied by a diarrhœa, and with nausea and rejection of the food, oppression or faintness about the stomach, at first a craving, and afterwards an utter extinction of the appetite; drowsiness succeeded by great watchfulness; startling dreams; habitual coldness of the feet, and sometimes cramp of them; restlessness of mind; paucity of urine, with sometimes hepatic, uterine, or hemorrhoidal hæmorrhage; the alvine discharges being essentially morbid, and of the colour of grey paper, verging towards that of yeast, and occasionally of the colour and consistence of tar. It is a complaint of common occurrence, and affecting persons of every age, though most prevalent with children. In essence it is the same as the cholera so named, differing from it only in the more gradual approach of the congestive state of the liver, and in the more frequent supervention of the inflammatory stage, when it assumes the name of bilious fever, as it is met with in adults, and that of infantile remittent or marasmus in children.

Before proceeding from the consideration of this part of our subject to that of the treatment, it will be proper, for the elucidation of both, to make a remark or two upon the modes by which it may be assumed as probable that the disturbance in the secretions on the liver is produced. That the disorder originates from some disturbance in the digestive function, is fully and amply established. The process of digestion is carried on by a series of actions of organs, which act in accordance together for that object; and by a union of sympathy established amongst them, there is a befitting adjustment of the quantity of the secretions required from each, as well as of the periods when they should be poured forth. Thus the food, when received into the mouth, excites there, by its own peculiar stimulus, the action of the salivary glands, and when it reaches the stomach there is a corresponding effect excited there to procure from it its necessary secretions; but as the liver, whose secretion is required for digestion, lies out of the reach of any direct stimulus from the food, it can only be affected by an indirect impulse communicated to it from the stomach. In the healthy condition of the stomach, and under the use of proper food, the stimulus imparted to it by the digesting or digested aliment will be of a healthy quality, and a healthy stimulus will be thence communicated to the liver, and a secretion of bile afforded suitable alike in its quantity and kind. If, however, on the other hand, a contrary condition of the stomach exist, the stimulus communicated to the liver will be insufficient in degree, or of a morbid nature, and an insufficient secretion of bile will be supplied. It would be foreign to my design to enter upon the consideration of the nature of the particular disordered states of the stomach which give rise to a disorder in the functions of the liver; or in what they differ from mere *dyspepsia*, or some other affections of it, in which no such effect is produced, nor whether the stomach be constantly, or only occasionally, the first in the series of organs to be affected. For, upon whatever hypothesis we may found our explanation of the origin of the cholera morbus, or of the nature of those morbid states of the stomach and bowels which lead to and accompany it, or of the precise modes of action of those remote causes which are found to act successively in increasing or continuing it, the fact remains, to my judgment, indubitable, that the seat of the disorder is in the liver, and that the means suited to the relief of the disordered actions of this organ, are adequate to restore the other disturbed functions to their healthy condition.

Having now given very summarily the views I entertain of the pathology of cholera, and of the varieties into which it is divisible, I shall next proceed to detail very briefly the circumstances which are to be considered in the treatment.

(*To be concluded.*)

ESSAYS
ON
ANIMAL ACTION.

By William Dobson, *Esq.*, *Surgeon.*

No. II.

Ordinary muscular contraction results from the stimulus of "volition;" but this is a vague term. According to the common acceptation, it signifies the instrument which mind uses to produce certain acts. Thus, if I move my hands, the muscles are the mechanical agents, the stimulus of volition on the muscles precedes this action, and mind, the dictation, is antecedent to volition. The mind directs then, that a muscle shall move, but if the mind be confined to the skull, what is the nature of its messenger? Is it according to the electro-chemical physiologists' galvanic fluid? or is it the mind which leaves the cerebri tabernaculum and carries its own mandate; this being termed volition? If so, then volition may be denominated a messenger of the mind, and that agent which induces *voluntary* movements. Of what is its essence we know not; we merely recognise its existence by its effects; it is a something which excites the muscles to *contraction*, and thus locomotion is performed. How is it then that the acephalic infant can move its muscles? It does so, and must there not be a cause? and is this *volition*? For *volition* is said to reside in the brain, yet here no brain exists; then the inference we are warranted in deducing is, that in ordinary cases, volition being a mandate of the mind, steers the movements of the voluntary muscles; but that when there is no brain, volition can *not* exist; hence those movements which occur in acephalic infants are caused by some other and unknown principle, which must have its seat in the spinal marrow and the nerves, this force being capable of enduring muscular movements, but in no definite manner (only mechanically), in consequence of the want of brain. What are we to designate this motor force? May we term it *nervous* volition in contradistinction to *cerebral* volition? That there is such a difference as to admit of a distinction is clear, but we really perceive them so analogous as to almost induce the belief that they are only grades of one existence. Does the cause of locomotion exist in the brain, in the spinal marrow and their motor nerves? Is it a diffluent principle occupying the whole of these structures? And is it so ordained that each nervous fibre can cause an action in the muscular fibril it supplies? And does the spinal marrow contain an augmented quantity of this motive force, so as to be enabled to induce, through the medium of the intervening nerves, a contraction in one or any certain muscles more perfectly than when only nerves exist, these actions being independent of *cerebral* influence? Numerous recorded facts and every-day observations support a reply in the affirmative. And the quantity of this principle is even in proportion to the complexity of the apparatus in which it exists in a healthy state. This we shall endeavour to prove hereafter.

The 1st proposition which I have to adduce is, that the nervous system is not a unit, but that it is composed of a multiplicity of parts. 2nd. That each individual part possesses a peculiarity of organisation; and, 3rd. That each variety of structure developes a differency of function.

Similarity of structure implies similarity of function. Dissimilarity of structure equally implies diversity of function. Complexity of structure indicates numerous phenomena. Simplicity of structure is ever conjoined with simplicity of phenomena. The brain is a complex organ, and its phenomena are numerous. The spinal marrow is *less* complex, and its phenomena are in a ratio. The nerves are *still less* complex, and *their* phenomena correspond. These are axioms which every observation corroborates, and which sophistry can neither overturn nor invalidate.

To delineate adequately the nature of animal action, would require an extended exposition of all the metaphysical reasoning which is promulgated. But my limits will not allow, nor does the subject demand, so diffuse an investigation. We have to ascertain, as far as reasoning can extend, What are the essential changes in an animal which precede muscular motion? Is that cause which induces muscular motion, necessarily an attribute of *mind*? Or is it a principle *sui generis*? A principle developed by the spinal marrow and the nerves only, and not resident in the brain, but capable of being influenced by the dictates of that organ?

Let us answer these queries *seriatim*. Were muscular contraction a mechanical operation, we should look for a mechanical cause; but as it is not,—as it arises not from a mechanical force, but from an unknown agent, we are referred to a presumed cause in the connective nerves. And it is known, that where there are *no* nerves, there the phenomenon of contraction does not occur. Hence we are justified in supposing that it is in the nerves where the cause of the muscular force resides; that some change must occur in the nerves before any muscular change takes place.

But it has been denied that the nerves can of themselves produce muscular contraction—can develop the force. The class zoophytes completely destroys this assertion. In this class there is a nervous system diffused throughout the whole apparently homogeneous structure; no brain, no ganglia, nor nodule of nervous substance, can be detected: this simple arrangement appears adequate for effecting the limited actions of the animal. Is reason the dictator? We have not to answer. Without a brain muscular motion *can* be produced. Look at the worm how it crawls on the surface of the earth; though there is found a slight enlargement at the upper part of the nervous chord near the œsophagus, bearing some analogy to a brain, yet if the animal be divided into a number of pieces, each part continues to move for a considerable period afterwards. If we observe the polypus when divided into a number of pieces, each part lives and moves, and each individual portion in time forms a perfect and independent creature. All these actions are performed, either in the individual pieces or combinedly, without a brain.

The spinal marrow exercises it is clear some influence over the animal movements, but how far it is relatively productive of those actions must ever remain a matter of conjecture. Experiment has been had recourse to, and many curious results have been obtained, but they are far from satisfactory, far from determining the motor influence of the spinal marrow over the animal movements. However, we are compelled to notice a few facts which have been ascertained by Sir C. Bell. Previously to those great discoveries, it was imagined that one nerve conveyed to the spinal marrow the perceptions of external impressions, and reconveyed the will's mandates also, but Sir C. Bell has demonstrated that each nerve consists of two parts or chords, viz. one which takes perception of things, and carries them to the central masses, and the other which excites motion. Now it is found that the motor nerve communicates with the anterior column of the spinal marrow, and the sensitive with the posterior; and that if the posterior column be destroyed, or the nerves which arise from it be severed, mental perception is abolished; and if the anterior column be destroyed or separated from its nerves, voluntary motion cannot be effected. But these are uncertain evidences, they only prove that a connexion subsists between the spinal marrow and the animal movements, yet we know that the size of the spinal marrow, when compared with the size of other portions of the nervous system, is ever in a ratio with the power of the muscular system; and that where large and numerous nerves originate, the spinal marrow is obviously enlarged, ex. gr. at the origin of the axillary plexus. Annex to the spinal system an enlarged mass at the superior part, and a brain is formed, an organ no less wonderful in its office than complex in its mechanism. Proceeding from the lower orders of animals up to man, a progressive enlargement of the brain is found. Though the size of the brain of man is excelled by that of a few animals, yet when compared with the relative size of the nerves and spinal marrow, man's is the largest. Let us examine the brain in regard to volition and muscular movements. Man, acccording to some, possesses mind exclusively. But if *volition* or *will* be the mandate of mind *only*, how is it that the inferior animals exhibit in a perfect degree the products of mind? If animals have no mind, and can excite the muscles to action, the cause must be independent of mind; for where there is no mind, there can be no cerebral "*volition*." The metaphysicians resort to "*instinct*" for an explanation; and they define instinct, "a principle inherent in an animal which causes it to effect certain actions without any preconceived or definite objects." I am surprised how it can be asserted that quadrupeds act from "blind instinct," when we have facts so palpable and so numerous as to convince even the most sceptical if he be not "hood-winked," that a result is intended when an animal acts.

I conceive then that there is a principle resident in the spinal marrow and its nerves, removed from all connexion with the brain, by which the contraction of the voluntary muscles can be induced; but that these motions thus effected (as ex. gr. in the acephalic infant) without the concomitant dictates of the brain, under the form of volition, are merely fortuitous, there being no definite object in view to obtain which the muscles are called into play, so that the phenomena which result from their contraction may be compared to the condition of a shattered vessel, whose rudder has been dashed away by the foaming billows of a tempestuous sea, and whose pilot, by the direction of whom the vessel should be steered, has sunk into the abyss profound, leaving his former charge at the mercy of the ruthless elements, against whose fury it can offer but little resistance. That the brain, when it exists, is the regulator of the animal movements, is placed beyond a doubt, but it is equally as tenable that animal action can be performed without such an organ: in proportion to the relative size of the brain is the animal capable of obtaining his every want. He excels not by physical, but by mental power; and so, in an ascending ratio up to man, and up to the highest state of intellectual greatness, the demigod of creation. The brain is the seat of the intellect and the feelings, the grand emporium of the mind, the storehouse of knowledge, and the congress of surrounding impressions. It is from this organ that the mandates of mind issue, and being sent to the spinal marrow and the nerves, these act as the messengers of the brain, and the muscles are their agents. Then, after these few observations, cursory as they are, I conceive we are warranted in drawing the following conclusions:—

1. That, as the brain, the spinal marrow, and the connective nerves, are composed of analogous materials, we have a right to infer some analogy in their functions; that is, that mind, or the governing principle of the machine, presides throughout.

2. That, as the spinal marrow and their nerves in the more perfect of organised beings are relatively less complex than the brain, fewer and more simple phenomena are expected to result.

3. That, as certain phenomena are exhibited independent of brain, and which phenomena are such as entitle them to rank as mental attributes—viz. sensation and motion, it is a legitimate conclusion that such principles are resident in the spinal marrow and the nerves, even *when a brain exists.*

ON THE USE AND ABUSE OF

ARTIFICIAL DILATATION

IN THE

PRACTICE OF MIDWIFERY.

> "It is now well ascertained, that much of the success of the latter part of a severe labour depends on the right management of its first stages."—DR. CLARKE.

To the Editor of THE LANCET.

SIR,—In the month of November last, you did me the favour to insert a case of vaginal hernia. I then intended to add some further observations, but deferred their publication from the fear that I might be trespassing on the interests of your Journal by occupying too much space, and thereby displacing more valuable matter. If therefore it be agreeable, I will in this paper conclude my observations, by stating my opinions on the probable cause and origin of the malady in question, and, at the same time, offering some strictures on a malpractice, which, I imagine, prevails in the lower walks of obstetrics to a far greater extent than has hitherto been supposed. It would appear from the patient's own account, that she had been engaged with weak though distinct labour pains forty-eight hours, in which time she had been examined once, and pronounced to be going on slowly. On the pains becoming more urgent, her accoucheur was sent for; he remained with her about twelve hours till the termination of labour; during the whole of this period, at longer or shorter intervals, he maintained a revolving motion of the thumb and fingers gathered into the form of a cone at the orifice of the vagina, with the intention, I should suppose, of preparing the external parts for the last stage of labour; the manœuvre was repeated with more or less frequency, until the uterus and muscular system aiding parturient action, became excessively fatigued. These gyrations, as might be expected, induced a rapid increase of uterine contractions, and a rigid and painful condition of the vagina, provoking, by its sympathy with the uterus, an irritable state of that organ, which prevented its lips yielding by consent with the propelling power as in natural pains. In this dilemma, when the parturient action was suspended, in place of waiting for the restoration of the expended powers of nature, by allowing the patient rest, and, as a probable consequence, refreshing sleep, while the muscular exhaustion was producing the due degree of relaxation, not only in the external parts, but also in the neck and mouth of the uterus, which must now (conformably with the power inherent in the os uteri to contract itself when the longitudinal fibres are no longer able by their superior force to keep it in a state of expansion) have formed partial contractions around some part of the child, instead of waiting I say until the propelling powers had regained their energy, a dose of the ergot of rye was administered, which, from the moment of its operation, kept the wearied sufferer in continued and unremitting pain, totally different from the alternate contraction and repose of natural pains, until the birth of the child. The administration of the secale cornutum at this juncture was, I apprehend, a misapplication of that valuable remedy, as from its effects it is apparent that no stimulus was required, but that partial contractions of some portion of the uterus, probably of its neck around the neck of the fœtus, combined with universal rigidity (the waters having been drained off ten or eleven hours previously), were retarding the further progress of the head. The testimony of the patient's friends, that the vertex was seen occupying the superior commissure of the labia an hour before delivery, would seem to confirm this supposition. It will not therefore be a matter of surprise, that a patient reduced to the last extremity of feebleness by a long-continued excitement on the irritable fibre of the vagina, provoking untimely uterine contraction, and inducing a painfully rigid and almost excoriated state of the soft parts, should, in consequence of the debility induced, and the powerful efforts still required to effect the delivery, be afflicted with such a result as hernia. But besides the untoward malady in question, another and rather novel affection was superinduced by the officious handling just described. At every alvine evacuation, and also at intervals through the day, especially after much exertion, the patient felt the same disagreeable sensation as though the hand were still occupied in stretching the vaginal parietes. This deceptive feeling was attributed by the patient to an excoriation produced by the operator's nails, but I presume that was not the case, for when the altered action of the genital nerves had existed two years, on the birth of the next child it ceased altogether. It certainly is remarkable, that the nerves should be endowed with a delicate, or, I should rather say, indelicate, false perception, for so long a period, and be then suddenly restored; for an explanation of this phenomenon, I must refer the reader to his own physiological ingenuity, since I am at a loss to account for it.' The error of a practice that could induce so much misery, must be obvious to the youngest student; yet, for the sake of enforcing truth by argument, I will state the grounds of my objections to it deduced from physiological principles, and at the same time endeavour to restore the true meaning of a rule to be found in the writings of an ancient and highly respectable author, who has recommended in unqualified terms, that lingering labours be assisted by the revolution of the hand in the way before mentioned, altogether regardless of some important modifications with which the precept should have been guarded—such as the condition of the uterus and other soft parts, the constitution and temperament of the patient, the state of the mucous secretion, and the precise time for such interference. No doubt this assertion of one of the fathers of English midwifery may have given a specious plea to unreflecting or prejudiced minds for the admission of a barbarous innovation on the duties of an accoucheur, yet how any one at all acquainted with the animal economy can imagine that the mechanical expansion that would succeed in dilating inorganised substances of resilient properties, would be admissible in producing the same effect in highly-organised structures, it is astonishing to conceive—the laws of vitality so materially affect the result. Every practical accoucheur will confess, that while pains are frequent and strong, the os uteri thick, tumid, and indisposed to relax, and though pains may have been endured for an unusually long period, the practitioner would not be justified in endeavouring to produce a favourable state of dilatation by manual interference. No! the undoubted remedy in this condition of the soft parts would be venesection, nor would such a practice be permitted in constitutions of irritable fibre, more especially in a first accouchement, where the labour is progressing slowly, nothing less would be expected than a feverish reaction demanding immediate depletion, or terminating, as such cases frequently do, from the effects of repeated unavailing efforts of the muscular system, either in relative or universal exhaustion, the former of which, in some measure, acts as an equivalent to venesection, though at the expense of much fatigue. It is admitted by every practical accoucheur, that the viscid mucus secreted at the os tincæ preparatory to and during labour, is of the utmost consequence, serving the double purpose of depletion and relaxation to the neck of the uterus, vagina, and perineum; and that if this secretion be suspended by too frequent touching, by allowing a patient possessed of a highly inflammatory diathesis to be kept too warm, or to have stimulating drinks, such a disturbance will be excited throughout the system as will put a stop to the discharge, and in consequence to the further progress of the labour, notwithstanding the pains may continue with their previous or even greater energy. This point being so well known, it would seem superfluous to offer any further observations on the usefulness of the mucous discharge in the routine of delivery, or on the danger of suppressing the secretion by untimely expansive efforts. I shall be happy if the few hints already suggested be successful in convincing the few who may be enamoured of this injurious practice, that however well-intentioned such assistance may be, it is decidedly opposed to the principles of obstetrics, and must frequently expose females to irremediable misery, of which the case before us is sufficiently illustrative. But lest I should be thought to consider the duties of an accoucheur those of a passive observer, I will proceed to define my views on the proper period for using the artificial dilatation when admissible, and endeavour to illustrate an important point in practice—the discrimination between cases fitted for such assistance, and those in which it would be obnoxious. I believe it will be found that robust and healthy young females are not the proper subjects for this manœuvre, the glandular structure in them possessing the secretory function in an eminent degree, which, with the sympathy existing between the uterus and external parts, is sufficient to produce the desired relaxation, without any adventitious aid. In constitutions of this kind, the slightest possible interference by way of dilating will produce local inflammation, by exciting the action of the arteries beyond the secerning point—the secretion once suppressed, heat, pain, and dryness, the invariable concomitants of inflammation, succeed, and the contiguous muscular structure is thrown into a state of spasmodic contraction, upon which all the mischief attendant on rigidity supervenes.

If ever the expansive force be applicable, it is in a minor degree, and when the os uteri is well expanded in women of spare habit, pale fibre, and general languid frame, with those arrived at middle life before their first accouchement, and to patients who have suffered a laceration of the perineum in a previous birth, or otherwise possessed of partial constriction of the vagina. Fortunately for females who suffer officious handling at the commencement of labour, the repeated unavailing efforts to overcome the acquired rigidity will, if the inflammatory diathesis be not very considerable, produce universal exhaustion, by which the opposing fibres are almost as effectually and completely relieved, as though bloodletting had been adopted; yet this favourable issue will not always be obtained. In the highly inflammatory, fever would be excited, headach, thirst, hot skin, &c.; in short, a new condition of the system, which almost supersedes the business of labour: here venesection will alone relieve. It cannot, however, be denied, that a painful and tumified condition of the labia, perineum, and sphincter vaginæ, in no way connected with improper manual interference, will sometimes occur. I have myself witnessed some aggravated cases of this unyielding property of the external parts, and also the unavailing powers of all kinds of emollients, until the tonic spasm had been overcome by general or local bleeding, as the case would permit; afterwards warm water, which had previously appeared to increase the swelling and encourage a serous deposition in the loose cellular texture of the labia, become of use in completing the relaxation.

It has been remarked above, that badly-managed cases will often terminate safely where no remedial measures have been used, though at the risk of much danger. Consulting surgeons, I am convinced, must frequently meet with such cases. In illustration I will just give a brief outline of one:—The patient having endured many hours wearisome pains, made fruitless by artificial excitement, at length becomes excessively depressed; relative or universal exhaustion succeeds; uterine efforts are suspended, probably so far as to allow considerable intervals of sleep. The wearied sufferer strongly solicits this indulgence, and the practitioner, anxious to give some respite to his arduous exertions, very willingly consents; thus the whole matter seems to stand still, though in reality the woman is profiting by the repose, and the protrusive powers are recruiting. The head now lying at the orifice of the vagina, effects by its presence there the expansion that had unavailingly been sought for by unnatural means; the shoulders of the child, too, reposing within that cavity, lie moulded to the adaptations of its parietes, which have now become an easy and yielding envelope; and nothing remains to be overcome but the external set of fibres, which, from rest and contiguous sympathy, at length become relaxed. In the space of an hour or two, the patient is aroused by renewed uterine action, the tonic contraction of the uterus causes its fundus to embrace the breech closely, and with a little assistance propels the vertex through external parts. Thus an otherwise natural delivery is made preternatural, and all the contingencies attendant on lingering parturition endangered—especially flooding from an artery of the uterus. Having now considered the various omissions of a rule set forth for instruction, and endeavoured to illustrate the true principles of the edict, I will conclude this paper with a few concise remarks on the abuse of it. In the present advanced stage of surgical knowledge these copious observations will appear unnecessary, as deprecating a practice that could only have existed in times of ignorance, and now justly deserving of consignment to oblivion with all the tricks of infant science; indeed I had long hesitated as to the propriety of revealing the cause of the hernia in question, and should never have done so, were I not assured that the practice is more frequent than suspected; hence duty compels me to unfold and expose to general censure, a covert malpractice and relic of barbarity that even still exists, and which the delicacy of the lying-in chamber conceals from common observation, until revealed to the profession in the shape of a remote cause of hernia, lacerated perineum, sloughing of the urethra, &c.; besides, I should have considered myself wanting in common courtesy to the rising classes, if I had not added my mite of information, in pointing out one of the rocks on which a reputation may be split, and lamenting that there are sceptical students to be found who disregard the reiterated injunctions of their lecturer in some apparently trifling point (such, probably, as the one I now deprecate), and conceive for themselves, or imbibe at a friend's suggestion, some novel plan for expediting the action of the uterus, and abridging the usual time of labour. I remember having once been assailed with some such obliging counsel, but rejoice to say, that the impression, if any was ever made, was soon erased by the more wholesome precepts of the lecturer I was then attending (Dr. Montgomery, professor of midwifery, King and Queen's College, Ireland), to whose superior merits as a teacher, I shall, in conclusion, give my sincere tribute of praise.

Respectfully yours,
John Taylor, Surgeon.
Liverpool, Siddon Street,
April 6th, 1832.

WESTMINSTER MEDICAL SOCIETY.

Saturday, April 7th, 1832.

Mr. Jewel in the Chair.

NOTE TAKING.

If it be true that "hope deferred maketh the heart sick," there must be not a few "sick hearts" amongst the members and visitors at the museum in Windmill Street. Every Saturday evening brings with it an expectation that the cholera discussion will lose its claims to eternity; yet every Saturday it again stalks forward, perhaps in a new dress,—another, but the same. This evening the gaunt visitor was present as usual; he had, however, a very thin audience, his "ravages" having, it seems, terribly decimated the members, who have either been overthrown in the contest, or have forsaken the field of battle to avoid the flying arguments. Every now and then, to enliven the scene, comes a skirmish or two before the general engagement, and on the present occasion the reporter was involved in the *emeute.*

The introductory step to the proceedings of this, as in every other society, is the reading of the "minutes," and the secretary, Mr. Murray, being out of town, this part of the business devolved on his colleague, who, upon the starting of some objections to the words of the record, thought it right to say that the blame did not rest upon *his* shoulders.

Secretary.—It may be proper for me to state that Mr. Murray, being obliged to leave for Norfolk, was unable to deliver the minutes into my hands, having sent his notes to the Editor of The Lancet, from the report in which work the present abstract has been made.

Hereupon Dr. Johnson and some others expressed surprise at, and disapprobation of, the conduct of the absent secretary in sending for publication notes which had not been confirmed.

Two gentlemen, at the same time that they found fault with the "minutes," also took occasion to advert to the incorrectness of The Lancet's reports at that Society, whilst a third rose to speak in favour of those reports, when *he* was called to "order," as introducing extraneous matter!

Mr. Chinnock eulogised Mr. Murray for his general industry and attention; and he (Mr. Chinnock) felt doubtful of the accuracy of the statement that the notes had been sent to the Journal in question.

The Secretary said that he believed he had been led into a mistake,* and that Mr. Murray told him in his haste that the minutes might be taken from The Lancet's report.

Certain alterations were then made, and the minutes read and confirmed. What those alterations were we could not exactly understand, or they would have been introduced here. One of them, however, was, that Dr. Johnson had received, not the "proof sheets," but an *early copy* of the Edinburgh Medical and Surgical Journal.

The subject at length taken up for discussion, was

THE CHOLERA.

The Chairman observed that there was such a tendency to strike off to all points of a large circumference, that he would suggest the propriety of confining observations to some particular division of the question, or to the elimination of facts derived from personal investigations. Perhaps those gentlemen who had been in attendance upon cholera cases would favour the Society with their ideas of the most effective mode of treatment. There was another point also worthy of attention, and that was the period of interment after death.

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* The report in the last number of this Journal was from our notes, but at the time of the discussion we were ignorant whether or not a communication had been made to the Editor; upon inquiry we find that such was not the case.—Reporter.

A Gentleman, adopting Mr. Jewel's suggestion, related a case which he had very lately had under his care. He was sent for, and found the patient lodging in a public-house; he had had previous vomiting and purging, and was then cold and without any pulse. The lips, nose, and fingers were of a purple hue. Great anxiety was depicted on his countenance, and the man complained of a terrible sense of pressure about the region or of the heart. The speaker had seen thirty forty cases of cholera,—and none more decided than this. The treatment here adopted was bleeding to twelve ounces, the administration of the hot-air bath, and ammonia, after the use of which a copious perspiration supervened, and perfect recovery succeeded. The blood abstracted was slightly buffed.

A Member, whose name we could not obtain, adverting to some former observations of Mr. Greenwood, said that Mr. G. had contradicted himself. On the question of contagion, he wished to know how the contagionists would account for the appearance of the cholera in Paris, and what they would say in answer to the declaration of the distinguished surgeons and physicians of the Hôtel-Dieu.

Mr. Greenwood in reply observed, that his statements were taken from the work of Mr. Ainsworth, — a gentleman who had read of and practically studied the disease, and come to very scientific conclusions; he had heaped his facts together, thought for himself, and become, from conviction, a contagionist. The instance related by him was this:—Mr. Hampildon, a surgeon in the cholera districts, had a mother who lived with him, and was taken ill with all the symptoms of cholera, vomiting, purging, &c. The old lady being more susceptible of disease from the wear of her constitution, had thus become the victim of the contagious miasmata. Well, there were other links in the chain; for Mr. Hampildon, not thinking of any danger, sent his clothes to his washerwoman who put them under her bed, when shortly the woman was attacked, and died of cholera, and her husband and child followed. This was a specimen of the particular kind of law which the cholera seemed sometimes to follow, where the germs of the disease were distributed by means of "fomites." As to the declaration of the distinguished men of Paris respecting contagion, although from personal acquaintance with their talents he had the highest respect for them, yet he could but regret that those celebrated men should have come to such a hasty conclusion. A little further experience of the disease might induce them to take a different view from the one they now adopted.

Mr. King thought that this was not an answer but an evasion. Mr. Greenwood had avoided noticing the arrival of the epidemic in Paris, and its showing itself there, as in London, in different parts of the town, without any traces of conveyance by intercourse.

Mr. Chinnock said, that the result of his experience was the conviction that many cases of genuine cholera have occurred in and near London *sporadically*. He had seen in his district about twenty-eight cases altogether; and they had occurred where every circumstance was likely to favour the communication of the disease by contact, in Augusta-court, Jew's-row, &c. Notwithstanding this, however, he had not seen a single instance of the recurrence of the disease there. Perhaps some other member might be able to bring forward twenty-eight additional instances of the occurrence of sporadic cases. At all events he was supported in his statement respecting his own cases, by the inspectors appointed by the government, who had also visited them.

Mr. Greenwood said, it might be very well to go on by induction to examine the question, and in that mode two of the Indian Presidencies had decided in favour of contagion, but he considersd that no person could come to a proper conclusion from his individual experience. He would relate an instance which had lately occurred to him in the court near Wigan-street. The woman who was ill at No. 13 had a man lodging in the same house, who, fearing contagion left, but was afterwards attacked, and the next day was placed on a litter to be carried to a cholera hospital. A mob, however, assembled, bestowed upon the medical attendants all kinds of abuse, and caused much alarm, till at length the poor patient was absolutely taken off the litter by his brother, who carried him on his shoulders back to his lodgings. And, mark! this very man, the patient's brother, soon afterwards fell a victim to the disease, and was buried on Monday. (Some little conversation here ensued respecting the identity of the cholera in different places; after which the thread of the discussion was taken up again.)

Dr. Webster observed, that it would be desirable to know whether the individual last alluded to by Mr. Greenwood was not living, if not in the same house, at least in the same row of houses, and under similar local circumstances. That was a fact the ascertaining of which was very requisite before a conclusion could be come to, as it was important to have all the bearings of the case. Dr. W., pursuing the subject, remarked, that out of sixty-eight cases, which had been reported from Southwark, six had occurred at the workhouse, forty-nine in as many different houses, and one in the King's Bench. Now the Bench, as was too well known, had this year been very full, and yet no other case of cholera had happened there, except indeed one in the latter end of March. This last patient, however, he could state, upon Mr. Hooper's authority, had no communication whatever with the former. Here, then, in Southwark there were forty-nine cases out of sixty-eight, all in different houses, and yet no second person had suffered in either house. He (Dr. W.) had made strenuous endeavours to get out the real history of the circumstances connected with these cases, but after all could arrive at no valuable authentic information. What, then, was to be said of the particulars of cases occurring in the back settlements of Russia?

Dr. Stewart said, that in the parish of St. Giles, which had contributed its full share to the cholera lists, a great number of the patients were included within a space of a few hundred yards square, whilst in the Bloomsbury Dispensary not a single instance of cholera had happened. This was worthy of remark, as a corroboration of the theory of local influences. The facts which had come to the knowledge of the profession since the arrival of the cholera in England had certainly thrown considerable doubts on the question of contagion. There was not a single instance of the disease having occurred ten miles from London—the villages around the metropolis had not been attacked. A gentleman, Mr. Greenwood, had applied the epithet "hasty" to the declaration of the French medical men; but he would ask, when, if they waited, ought their opinions to have been promulgated?

Mr. Chinnock gave the following fact, bearing upon the same question. On St. Patrick's day, the two children to whom he had alluded on a previous occasion, died of cholera, in a crowded court, and in a room only six feet square. Yet in this apartment, where the children were lying dead, there were no less than four persons, and they in a state of intoxication, besides a woman who was reclining on the bed of the children. Not one, however, of these individuals was seized by the disorder; and the same remark applied to the jurymen, who afterwards ordered the bodies to be exhumed, and had the temerity to examine them.

A Foreigner rose to defend the French practitioners from the charge of "hasty" decisions. We understood him to say, that most of those whose names had been attached to the declaration before spoken of had paid attention to the disease in various parts of the world, and had consequently their minds prepared to give a verdict with the slightest additional evidence in their own country.

Mr. Greenwood was desirous of not having his opinions misconstrued. No one could look with greater admiration than himself on the talents of such men as Dupuytren, Recamier, and others, but he was not aware that they had left their hospital duties to investigate this disease in a foreign land, and his opinion of their declaration referred to the evidence they had met with in their own.

Dr. Granville asserted that contagion had lost its adherents in that Society (*No, no*), save Mr. Greenwood—the Sir Charles Wetherell of contagion. (A laugh). When contagion was to be proved, how did the arguers set about it? Why they made one thousand cases the exception, and one the rule general. How did facts stand at Berlin, as stated in the *Berlin Cholera Gazette*? Out of two thousand medical attendants, only eight were the subjects of cholera; and out of a very numerous class—the prostitutes, exposed to every hazard—only thirty-eight. It was true that one case fully established was as good as a thousand, but then, in individual cases, every attendant circumstance should be noted, or we could never be certain that the only clue left was contagion. The scientific men in Paris had come to their conclusions, after having all the facts of Berlin and other places submitted to their minds, and after seeing the cases—a large number—which had occurred in their own capital, and were they therefore to be termed hasty!

Dr. Johnson, since the last meeting, had continued his investigation into the cases of cholera at Guy's and St. Thomas's Hospitals, and the facts thereby acquired would tend to show the fallacies urged by the gentleman whose reasoning bore so much the character of "*post* hoc, ergo *propter* hoc." He (Dr. Johnson) had spoken last Saturday of seven cases which occurred at the two Borough hospitals, each in separate wards; he then stated that there was no communication between them, and what he had since seen, only confirmed the previous conviction that the disease had attacked these individuals quite independently of any contagion. Well, and what was the result of this outbreak of the cholera in the hospitals? Not a single additional instance had as yet occurred there. The first seized by the disorder received the attack in the ward just after undergoing a surgical operation. Of course he was taken to the *cholera* ward, quite distinct from the others; and there he died. Now the woman who attended this man was not a regular nurse, but took the office for a friend who had obtained leave of absence. The attendant escaped, whilst the regular nurse, upon her return to duty—thirty-six hours after the patient's death, immediately experienced a severe attack and died, and had she but returned thirty-six hours sooner, doubtless this would have been held up as a regular case of contagion. Thus was it evident by how apparently trifling a circumstance our minds might be blinded to the real state of the question. What was the remark made by Dr. Bright? That the particulars of this case had shown him how easily we may be deceived into an opinion, and that he could not therefore adopt the doctrine of contagion. For his own part, he (Dr. Johnson) declared that in the whole track of the disease from Limehouse to Kensington, he could not discover any instance of its communication from one individual to another.

Mr. MARSDEN (we believe) said that he was living on board the *Dover*, and that the surgeon of that vessel, who was a disciple of the contagionists, had assured him that every person on board had suffered from diarrhœa, and indeed it had become an axiom that if this diarrhœa were taken in check, an attack of cholera was prevented. He merely threw out this as a hint for watchfulness over other patients.

Mr. KING doubted whether the diarrhœa might not be the fruit of the locality. It appeared that diarrhœa may be brought on by peculiar circumstances,—that it may arise from vicissitudes. He himself had been accustomed to be perpetually in a dissecting room for eight or nine years, and yet whenever he staid too long at one time in the room he almost constantly suffered from diarrhœa. Might not the *cholera* diarrhœa arise from a similar cause, or from depression of spirits and exposure to cold? As to the question of contagion, there were some curious points which he should like to see cleared up. His friend D'Almas, who had assiduously and attentively examined the disease in Prussia, said that in no one instance had he seen the slightest proof of the communication of the disease from the dead body to the living one. In Poland, as in Russia, a sudden mortality would seize the troops, which upon change of position entirely ceased. So convinced was D'Almas of the non-contagiousness of cholera that he said to the Board of Health, "Take me, inoculate me, and show by direct experiment the truth of the position I advocate." He (Mr. King) could not forbear observing that there seemed to be about the Board of Health an unwillingness to state facts, which did not look well. Dr. Barry on his return to this country had been pleased to visit the Society, by whom he was most cordially received, and he had promised to communicate with them on the disease. Since that time, however, nothing had been seen of him in that room.

Dr. GILKREST said that he had visited the *Dover* for the purpose of inquiry. There were in her, from February 19th to April 4th, seventy-three persons, of whom thirty suffered from diarrhœa, and amongst them there were three nurses and eleven sailors. One of the nurses he found badly off in her berth. The diarrhœa was well known to have been prevalent for the last two months along the whole line of the river.

Mr. WALKER represented the people of the *Dover* as so afraid of contagion that all the ports were continually kept open, whilst at one end was a large fire. So that there was a perpetual rush of the cold damp air from the river. He was there the morning the nurse was taken ill. She told him she was sure she should be ill, her cabin was in so exceedingly damp a condition. She felt very chilly, and had a little diarrhœa and vomiting. He thought it a case of cholera, and went next day on purpose to see her, when he found her looking quite well and jolly. He thought the circumstances just stated, added to the want of exercise, were quite sufficient of themselves to account for the prevalence of the diarrhœa.

Mr. MARSDEN spoke of the excessive fatigue of the medical attendants and nurses as another predisposing cause. He himself being doubtful on the subject of contagion, he might mention that the *Dover* was standing off Limehouse with vessels close alongside her, and not one case had yet occurred in them.

Dr. FERGUSSON said that when he was on board the *Dover*, he certainly did not experience the unpleasant feeling of cold. The only time he was at all uncomfortable was when standing immediately opposite the "companion." He could not resist taking the opportunity of expressing a eulogium on the way in which the duties of the vessel seemed to be performed, and the courtesy manifested towards strangers.

ERRATUM.

To the Editor of THE LANCET.

SIR,—In the report of the discussion at the Westminster Medical Society on the 31st March, given in THE LANCET of to-day, I am made to say, that I was one of the deputation from the Cholera Association to the Central Board of Health. I stated that Dr. Granville was, but could not possibly have spoken so of myself, as I was not of the number of deputies on that occasion. The correction of this slight error will much oblige

Your most obedient servant,
D. BALLANTINE FERGUSSON.

7th April, 1832.

THE LANCET.

London, Saturday, April 14, 1832.

The hospital surgeons of London, like other corruptionists, are determined to hold fast by the numerous abuses on which they have so long fattened. True to their craft, and dreading the tendency of example, they will hear of no innovation in their system—no new movement in that "descensive circle" so graphically and elegantly described by Mr. J. H. Green. A letter signed "Q," in another part of our Journal, shows that a *mouvement* in the system is about to be effected at the London Hospital, and if executed in conformity with the opinions of Mr. John Scott, one of the surgeons to that institution, we are strongly inclined to suspect that it will prove an *eccentric* one. The governors of that establishment complain that the students, whose "turn" it has been to "take in" (they themselves having been submitted to a similar process at the commencement of each medical session, every day in fact being a "*taking in*" day, both for students and patients, at all our hospitals), have neglected the patients. The surgeons themselves offer no denial to this complaint, hence the managers have determined on the appointment of a house-surgeon. The students who have *not* transgressed, justly object to this arrangement, as annihilating an engagement entered into when they paid their heavy fees—in other words, when they were "taken in." That the governors have a right to appoint a house-surgeon, they do not dispute, contending, however, that, having entered their names and paid the sum demanded under an express stipulation that each in turn should be entitled to fill the office of house-pupil (the "dresser" of the other hospitals), the misconduct of the few cannot destroy the rights of the many, nor operate against certain bought and paid-for privileges.

We are far from regretting that this dispute has occurred, for it will have the effect of pointing out to the students their true position, and it will also prove to the gentlemen studying in our great hospitals generally, that the medical officers are not the masters—are not the *rulers* of those institutions, but must themselves submit to the regulations prescribed by the acting governors. The students will now find that the physicians and surgeons are enabled only to make such enormous demands for admission fees through the acquiescence, or what is still worse, the often ready connivance of the governors. The reception of students is a business wholly conducted for the benefit of the medical officers; not a shilling of the fees goes into the coffers of the institutions for the benefit of the patients, but the entire amount is swallowed up by those very individuals who profess such a *disinterested* desire to promote the welfare of the pupils. Let it not be inferred that we are opposed to rewards for medical services. Let it not be supposed that we are anxious that persons of talent and industry should perform arduous and laborious duties with nothing but empty "thanks" for pains. No; we have ever contended for the reverse; we have always asserted, and frequently proved, that there are no men in this country who are entitled to higher rewards for their exertions than those individuals who conscientiously and scientifically execute the duties connected with the offices of physician and surgeon to our great medical charities. But what we complain of is this; first, that the pay is not direct, that the source whence it is derived is not obvious to every beholder; and, secondly, that the students are made to pay for the performance of duties which ought to be rewarded by the governors. Under this arrangement patients and students are neglected, the hospitals are destitute of efficient medical officers, and not a *resident* surgeon or physician is to be found in any one of these establishments. We promised in the last LANCET to furnish a condensed view of the system of hospital discipline, as far as students are concerned, in some of the hospital schools on the continent. We redeem that pledge by extracting portions of the evidence given before the "*Anatomy Committee*" of the House of Commons. Limited as these extracts are, in comparison with the length to which they might be carried, they extend to such a space as to leave us no room for comment; but, in taking leave of the subject for the present, we would implore the governors of our hospitals at once to take into consideration the propriety of abolishing all the fees which are paid by students for admission to see the practice of their institutions, and we would implore the students themselves to act in a body, in their endeavours to get rid of imposts which are as degrading in character as insulting to those individuals from whom they are extorted. When Mr. SCOTT next proposes to *lower* the admission fee to *twenty pounds*, we would recommend him to explain to his class why such a toll should be exacted at the doors of our scientific and charitable institutions in England, when no such demand is made at any one of the similar institutions on the continent. We earnestly commend the following "Evidence" to general attention, especially demanding that of the students, officers, and governors of our hospitals. Originating, as it does, from such diverse sources,—from Radicals, Whigs, and Tories,—it must force its way, and produce a salutary impression upon the opinions of the most influential of those members of the profession who belong to the three contending parties in medical politics.

JAMES SOMERVILLE, M.D., called in and examined.

How many dissecting schools are there in Paris?—Two great public schools; L'Ecole de Médecine and La Pitié.

In what manner are the bodies transported from the hospitals?—I have seen carts employed, even in the day-time, to collect bodies.

When they arrive at the schools, under what limitation is the dissecting conducted?—There are superintendents appointed, who are answerable for an equal distribution of the bodies; and these superintendents are under the control of the *Chef des Travaux Anatomiques*.

When the bodies arrive at the school, who are the persons that distribute the bodies?—There is a head, the *Chef des Travaux*.

Are there any assistants?—He has several aids under him.

Of whom do those aids principally consist; are they all natives of France, or are some of them natives of other countries?—I believe there is no distinction made; that it is by *merit*,—that they are the most talented of the students,—that the office is open to *all*.

Are the bodies delivered to the use of the dissecting students at a certain price in the dissecting schools?—They are, I believe, nearly at the same price at all times. *Eight francs* was the common price at the *Ecole de Médecine.*

* * * *

What is the highest price in the dissecting schools?—I never heard of more than sixteen francs: I have known sixteen francs given for a particular body.

* * * *

There is scarcely any limitation to the number of bodies a pupil is allowed to dissect, if he be industrious.

A pupil, who enters himself there for dissection, dissects, if he pleases, without any immediate interference on the part of the aids or of the general superintendents of the establishment.

* * * *

Does not dissection, by the English pupils in Paris, generally take place under the superintendence of English surgeons?—It formerly did so, but a gentleman who is attending can best give information why that has been discontinued.

JAMES RICHARD BENNETT, Esq., called in and examined.

State, as far as you are able, the sources from which the dead bodies are obtained in Paris for dissection.—It may not be unnecessary to premise that prior to the revolution in France, the different hospitals in Paris were supported, as in London, by voluntary contributions, and private and distinct funds, each having its separate government. At the period of the revolution all were connected together, and their several funds being consolidated, and further revenues being provided by the government, the management of all the hospitals was entrusted to a body entitled the "Administration des Hopitaux," which is now composed of the leading noblemen and other distinguished persons in Paris. The Administration des Hopitaux have always felt it their duty, for humanity's sake, to promote the cultivation of medical science, and with that view give up for anatomical purposes the bodies of those who die in hospitals. They thus carry into effect the law passed by the legislative assembly, whereby it was enacted that the bodies of all those persons who died in hospitals, which should be unclaimed within twenty-four hours after death, should be delivered up for the purposes of science. Exhumation was thereby rendered unnecessary, and severe laws were enacted against the practice, which at present is never resorted to in Paris. I believe it is calculated that about one-fourth of the number who die in hospitals are not claimed by their friends; at least in 1822, when I went to Paris, that was the case.

Can you enumerate the different hospitals in Paris from which the supply is derived? The Hotel Dieu, La Charité, St. Louis, Necker, Enfans Malades, La Pitié, and Beaujon, are the principal hospitals, besides the two great houses of refuge—viz. the Hospice Salpetriere and Bicêtre.

What is the mode in which the bodies of those who die are transferred to the dissecting rooms?—They are sewed up in a clean cloth, and being placed in a covered cart, are in that manner brought to one or the other of the great dissecting establisments. There are but two dissecting establishments in Paris tolerated or permitted by the police, the Ecole de Médecine and the Amphitheatre adjoining the Hopital de la Pitié.

Is there no dissecting-room attached to any of those hospitals in particular, at which dissections take place?—It is forbidden; the physicians or surgeons have a right by law to open, for the purpose of enquiring into the nature of the disease, the bodies of those persons who die under their care in the hospitals, whether those bodies be claimed or not; and the examination is made in the dead-room of the hospital.

* * * * * * *

To which of the two schools were you attached?—The Amphitheatre, adjoining the Hopital de la Pitié.

In what capacity were you in that school?—I had no office nor authority from the French government; I merely taught there under sufferance, *every person having a right to dissect there;* but there were certain parts of the establishment which at that period were hired out for private dissection, and those rooms were held by the English students to whom I lectured on anatomy.

Is an ample supply of bodies obtained from those sources for the use of the pupils dissecting?—Very ample indeed; more than sufficient.

At what price are the bodies charged to the pupils?—If a body was opened and examined in the hospital, the charge was only three francs; if the body was not opened, it was five francs; and if the body was injected it was twelve francs. * *

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Did you yourself latterly meet with some difficulties in superintending the English students, of whom you had the care?—I experienced much difficulty from those individuals who had the right of teaching at the Amphitheatre de la Pitié, they conceiving, I presume, that I interfered with their interests. I consequently applied to Sir Charles Stuart, then British ambassador at the French court, and prayed his interference with the French government, for their permission of an authorized English school in Paris. Sir Charles Stuart did not feel himself at liberty to make the request without the sanction of the English government; I, therefore, came home and addressed Mr. Canning, then Secretary for Foreign Affairs, and was led to expect his concurrence; but on the subject being made known to the College of Surgeons in London, they waited on Mr. Canning, and *dissuaded him from granting my request.* Sometime after my return to Paris, the French authorities obliged me to desist from teaching * * *

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Will you have the goodness to state why you have been induced to give up your own private school?—The expenses of it are so very considerable, in consequence of the charge for dead bodies, and it has occurred to me, as a beginner, that the resurrection men have exacted more from me than has been usually paid by others. I have paid fourteen guineas for a subject which I had afterwards to give to the pupils for eight guineas, that being the usual sum paid by students for a dead body in London.

Is not this applicable to every private dissecting establishment in London?—I believe so.

The owners of *private* establishments not being supported, like the teachers at the hospital-schools by the fees derivable from large classes of pupils, and the advantage connected with the use of the public theatre and dissecting room, are they capable to pay the price necessary for obtaining a supply of subjects?—I believe so.

[A letter from Dr. Carswell was read, from which the following is an extract.]

"I believe I have nothing further to add to what I had the honour of stating to you when in London, as to the mode of supplying bodies for the above purposes, adopted by the French Administration of Hospitals. In one word, all unclaimed bodies may be employed for the purposes of dissection, and are regularly brought from the different hospitals of the metropolis early in the morning in covered waggons, and deposited in the two great schools of practical anatomy—La Pitié and l'Ecole de Médecine. The former is destined for public dissections, and *students of every country are admitted to it* GRATIS, having only to pay a trifling sum for each body they dissect. This sum is fixed by the administration at the rate of from two shillings to about seven shillings, according as the body has been opened, is unopened, or injected. The latter is set apart for a particular class of students."

Should you concur in the statements of Dr. Carswell, contained in that letter?—I would.

In the studies of the English students, which you superintended in Paris, did it form a part of the course that they should perform on the dead body the principal surgical operations which they may be required to perform on the living? They all availed themselves of the extreme cheapness of subjects to perform the operations.

Did you, as superintending their studies, aid in teaching them to perform those operations?—Yes, I did.

You consider that an essential part of surgical instruction?—Yes, I do.

DAVID BARRY, M.D., called in and examined.

Is it not considered indispensable, in a course of medical and surgical education in France, that the student should perform upon the dead body the principal operations required to be performed upon the living?—Most certainly.

* * * *

Is there not attached to La Pitié, a gentleman of the name of M. Lisfranc, who is celebrated for teaching the mode of performing upon a dead body the principal surgical operations?—Yes, there is.

Are not his demonstrations frequented by a very large number of English students who resort to Paris?—Particularly so, almost by every one.

Do you know of any similar course given in this country?—I know of none; I have studied in Dublin and in this country; I know of none.

Do you not consider that course of surgical instruction of the highest importance?—I certainly do.

* * * *

The bodies that are distributed to the dissecting schools being sold at a certain price to the pupils who dissect in Paris, what is done with the proceeds arising from that sale?—The proceeds are employed by the administration of the hospitals to defray the expense of carriage and the burial of the remains.

Will you describe more particularly the manner of obtaining, and afterwards distributing, the bodies to the dissecting schools at Paris.—The only connexion that the student at the hospital has with bodies, is while the man is a patient in the ward, and is in his last moments, and again when he sees him *on the table of the clinical lecturer*. What becomes of the body in the interval, between the dying in the ward and the *lecture* upon his viscera or appearances after death, I am not personally acquainted with. If the friends claim the body after it is opened, it is given up, decently sewed up in canvas.

Claimed within what time?—Twenty-four hours, I believe.

* * * *

What is the result of your experience as to the practice in the *Portuguese* schools?—Medical science, as far as regards anatomy, is at a low ebb in Portugal; and for whatever dissection is practised there, the subjects are had from the hospitals called Misericordia, equivalent to the Hopital de la Pitiè, or Charity, in France.

Are there any professed schools of surgery and anatomy at Lisbon?—I refer to Lisbon, Coimbra, and Oporto. There is an university at Coimbra where anatomy is taught.

Was an ample supply of bodies obtained for the wants of the pupils?—Most ample: there is not the slightest objection to the disposition of bodies. The supply was particularly ample of children, from the number of children put into the *roda* or foundling cradles: they die in great numbers, and may be had for asking for.

JAMES R. BENNETT, Esq., recalled.

Is any person allowed to open a dissecting school at Paris, at any other place than at one of the two leading dissecting establishments?—No, they are not.

The facilities for dissection given at those two leading establishments are so great that private dissecting rooms are quite unnecessary?—Quite unnecessary.

Are there not certain pupils, at the Hôpital de la Pitié, who have certain privileges?—Yes.

Will you state who are the pupils that are allowed those privileges.—At Paris there are attached to each hospital a certain number of pupils, who are denominated Eléves Intérnes, or house pupils: those persons are annually *elected* to the office *at a public examination,* and each holds the office for a certain number of years. Those individuals have the first choice of the subjects at La Pitié, and at a less cost than the ordinary students.

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What is the establishment for dissection adjoining the Hôpital de la Pitié?—It consists of two or three large dissecting rooms, and some smaller ones placed above them. The great ones are open to *all* medical students, who are only required to pay for the subjects they dissect, without *any* further charge.

Gustavus Himly called in and examined:

What is the mode of obtaining bodies in Göttingen?—Every person dying in prison or penitentiary is given up to anatomy; that is, not only in Göttingen, but they are sent from the great penitentiaries in Hameln, and from another at Moringen, near Göttingen. Persons who cannot pay for their burial in town are given up to anatomy: the poor people who are supported at the public expense are obliged to be given up; and it is often the case that poor people give up their body, and get money for it. All persons executed are given up to anatomy: public women are also given up. Those who die in Hospitals are *not* given; they are only opened in the hospitals.

By opened, do you mean that they are completely dissected, or only examined?—Only examined, and the disease they die of ascertained.

James Moncrief Arnott, Esq., called in and examined.

You have some experience as to the practice in the hospitals and the dissecting schools at Vienna?—I have.

Will you describe to the committee the mode adopted for supplying the dissecting schools?—The dissecting rooms are supplied with bodies from the Allgemeine Krankenhaus, or general hospital—a large hospital situated in one of the suburbs. The bodies are sent to the University in the city, and dissection is carried on there.

Are all the patients who die in hospitals sent in for disseection, or only those unreclaimed by their friends and relatives?—The bodies of all who die in the hospital must, without exception, be opened, if such is the will of the attendant physician or surgeon: and this is performed by a Prosecutor of pathological anatomy, appointed by government for this purpose. The bodies for dissection are those of the unclaimed.

Are great facilities given at Vienna to foreign students frequenting that University?—Great facilities are given; the government not only throws no obstacles in their way, but grants them certain advantages. Besides being allowed to *attend the hospital* and all the public lectures *gratuitously,* like the Austrian students, they are also allowed to take advantage of private courses of lectures with the professors.

* * * *

It is merely necessary that you should introduce yourself to the professor of anatomy; you are then admitted to the dissecting room; and bodies are furnished to you at from four to five shillings a piece from the general hospital.

Gaetano Negri, M.D., called in and examined.

Is it a rule at the hospitals (in Parma and Bologna), that the bodies of all who die there shall be given up to the dissecting schools?—Yes, if they are required.

Do you mean without exception, whether they are claimed or not by their relatives?—I think if a relation claims a body, and will pay the expenses of the funeral, they will give up that body; but in general the bodies of all poor people are examined by the physicians and surgeons of the hospital, or they are sent to the school of anatomy.

* * * * * * *

What price would a student have to pay for a body, suppose he was in want of one?—I cannot say anything of that, because we have always plenty without the expense.

Are foreign students admitted to dissect in the different schools in Italy?—Yes; but for dissecting it is necessary to have particular permission, because it is necessary to have a dead body by order of the professor of anatomy.

Does dissection take place only at the University, or does it take place also in private dissecting-rooms?—At the University and at the hospitals; but at the hospitals only for those pupils in practice at the hospitals.

John Webster, M.D., called in and examined.

Is there any information you possess and could add to the information already given?—With regard to the mode of examination at Berlin, I think I can state all the particulars, since Paris, Pavia, and Berlin are the universities I have resided most at, having passed a winter at each. * * I know of nothing particular in addition to what Dr. Negri has stated regarding examinations and dissections at *Italian* universities. The facilities for studying anatomy are very considerable at Pavia; at no school I have visited are they so great.

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What is the practice at Berlin?—The

facilities are great at Berlin, indeed all over Germany I never heard of any difficulty.

What were the sources from which bodies were obtained?—I understood they were from the prisons and hospitals; but I cannot speak as to legislative enactments.

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What is the price of subjects?—I never heard of any person paying for a subject.

Is there more than one dissecting school at Berlin, where dissections can take place?—In the university only, as far as I know, where the supply is abundant.

Do students of all nations resort to the university?—Yes; I was the only Englishman there, and I found every facility for study.

NEW REGULATIONS AT THE LONDON HOSPITAL.

To the Editor of THE LANCET.

SIR,—Feeling a lively interest in the welfare of a school of which I am a pupil, I may be excused intruding upon your valuable columns a few observations relative to an occurrence which took place at the London Hospital last evening. You must know, Sir, that at this hospital it has been the practice for two of the surgical pupils in rotation to perform, weekly, the duties of dressers, taking the charge and responsibility of the numerous accident and other patients admitted during that period. Several complaints have been made to the members of the House Committee, of negligence towards the in and out-door patients, consequently they have occupied themselves in devising a remedy; and the adoption of a most radical remedy is contemplated by them,—no other in fact than the subversion of the privileges and advantages of the pupils, by the appointment of a house-surgeon. During the consideration of so important a subject, the surgeons of the hospital could not remain inactive, knowing as they do how totally incompatible the appointment of a house-surgeon would be with the interests of the pupils; and if either of the surgeons of the hospital deserved well for his endeavours to avert so great a calamity, that one was Mr. John Scott, but how has he been repaid? By the vilest ingratitude. Upon the mere report that he wished for the establishment of a house-surgeon, or that he recommended another arrangement, was he condemned and exposed to the most disgusting insults last evening. A party assembled before the hour of the surgical lecture, and agreed to express their disapprobation of his reported conduct, by hissing and hooting him, so as to prevent or delay the delivery of his lecture, and afterwards to quit the theatre.

Mr. Scott, however, anticipated them, and entered immediately into an explanation of his views on the subject. "He had himself proposed that the fee for the surgeon's practice should be reduced to 20*l.*, and that nothing extra should be exacted for a dressership, for he maintained that no person ought to pay a single farthing for that for which he gave his services in return. It had been asked, he said, if the present system of rotation were not continued, by what means could the dressers be impartially appointed from among the mass of pupils? He had proposed that the pupils should no longer indiscriminately take in under any one of the surgeons, but that they should, during one portion of their surgical practice, be considered the pupils of one surgeon, and during another portion the pupils of another, and that the dressers for the week should be those two of the candidates who should acquit themselves most satisfactorily at a public examination on the anatomical and surgical subjects which had been treated of during the preceding week; that merit and merit only should be the passport to office, and that a testimonial of their qualifications should be awarded to them.

After Mr. Scott had concluded his harangue, the party (consisting chiefly of pupils whose practice had wholly or nearly expired) uttered a few hisses, &c., and quitted the theatre. Mr. Scott then proceeded in the delivery of his lecture.

Was such treatment merited by one whose greatest ambition is to advance the pupil's interest?—one who three years ago offered the same resistance to the identical proposition, one who has even recently refused his third of a dresser's fee? Ought not his conduct to be hailed as the dawn of that general reform of the hospital surgeons themselves which THE LANCET has so often and so ably advocated, than which nothing could more conduce to the advancement of science, by calling forth the latent and stifled energies of many, who but for the existing impediments, would become ornaments to the profession and to society?

Since writing the above, I have heard, though not with surprise, that certain interested individuals are the movers of this machinery directed against Mr. Scott. I am, Sir, your humble servant,

Q.

Tuesday, April 10, 1832.

Since the publication of the last number of our journal, the capital of France has been the scene of such melancholy events as will form one of the most gloomy pages in the history of pestilential diseases. Within ten days from its announcement in Paris, 7,000 cases of cholera have occurred, of which number 3,600 have terminated fatally. The mails received as we write, give the last *daily* report at nearly *two thousand* cases and *eight hundred* deaths within the twenty-four hours.

When it is remembered that this disease and mortality, enormous as they are, are but the amount of what has occurred in the city of Paris alone; and when it is also borne in mind that the scourge is raging with equal violence in all the *immediate suburbs* of the town, from which the cases are not reported; and, lastly, when it is taken into consideration, that at least one-third of the cases *in* Paris are not reported, as is the fact, a just idea may be formed of the overwhelming power of this almost unprecedented calamity.

If we ask ourselves,—What is the nature and what the laws which govern this anomalous pestilence?—how strange, how inexplicable are the facts we have to contemplate! In Paris, not five days before the onset of the disorder, M. Villerme, the well-known and gifted statist, in a memoir on epidemics, read before the *Academie de Médecine*, congratulated his fellow citizens on the probable, nay almost certain immunity, they were to enjoy from the cholera, as well as from all other similar visitations. "Paris," he said, "exceeded all other cities in the extent of its civilization; the inhabitants of that capital were the strongest in moral courage of any nation in the world,—were unrivalled in physical energy;" it was superintended and controlled by an excellent and indefatigable medical police; no nuisances there existed; knackeries, lay-stalls, dunghills, and all other similar nuisances, were carefully removed from it beyond the barriers; in short, the provisions of the "code sanitaire," taken along with the circumstances of the citizens, were either to defend them altogether from the cholera, or so to divest the disease of its sting, that the monster would be harmless in the precincts of the privileged town, and might be visited and stared at as one of the lions of the day. But what has proved to be the fact? From the prime minister, the peer, the deputy, and the general, to the pauper "*chiffonier*" and "*debardeur*," all classes of inhabitants have been smitten, and in equal ratio to the proportion which they bear in numbers to each other. The timid and the brave, the weakly and the robust, from the infant to the decrepid man, all suffer from the scourge; and more victims fall daily within the walls of Paris than sufficed for the overthrow of the moral pest exterminated in face of the barricades of July.

Our readers must be aware how useless it would be to crowd into our pages the multiferious details which are supplied by the public journals on this subject. It is rather our province briefly to select from and arrange them, and to draw inferences from their indications—a task which, in due season, shall be carefully performed.

Meanwhile we wish to devote a passing glance at a question of *little practical import*, but of considerable controversial interest, namely, the mode by which the disease made its appearance in Paris, and has since extended itself therein.

On the first point, although there are many facts to show that the malady *may* have been introduced from without by human intercourse, there are many more which are contradictory of that opinion. It is true, for example, that the very first cases in France, *occurred in Calais *three weeks before the irruptions in Paris*, and that cases of a more than suspicious kind took place in the interval in some of the towns on the high road to that city. Nevertheless we unhesitatingly avow our conviction that it would be worse than frivolous to discuss the proposition that some other influence besides contagion was concerned, and mainly concerned, in the excitement of the disease in the French capital, and has since contributed powerfully and fatally to its propagation. What this cause is we know not, and we know that no one else comprehends it. We cannot speculate upon it. We believe it, in short, to be at present beyond the limits of human comprehension. But we know full well *what it is not*. It is not the nature of Parisian food; for the food of Paris has been of the same kind for centuries, and has generated no such disorder. It is not the filth of Paris, for Paris is less filthy by an infinite number of degrees than it has been for many years. It is not, in short, any one of those things of which human senses can take cognizance; for all these things have never given birth to the prevailing disease. Away, then, with the specious humbug that pretends to teach us the "philosophical causation" of the disease, by reminding us of these local circumstances. Let us honestly acknowledge our ignorance on the subject; which, after all, is far more scientific than the pretence to science.

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* We state this fact from our certain knowledge.

While we thus, however, readily admit the irresistible evidence of facts, we cannot reject the equally forcible testimony which teaches us that the pestilence *can* spread from one individual to another. With proofs of this description our pages are already crowded; while the Paris irruption stands solitary und unparalleled in the history of the disease. Another group of these facts we now subjoin, communicated by Mr. CHARLES PEEBLE, of East Hendred. Let any man peruse them, and reject the inferences they so palpably afford, and we are not destitute of courtesy when we say, either that his brains can be only fit for the skull of a bedlamite, or his honesty is inferior to his boldness.

At some other period we propose to afford our readers a view, drawn up in parallel passages and columns, of every argument yet advanced in proof and disproof of the contagious nature of this disease. The facts will show that truth lies in the mean of both opinions, that there are positive, well-authenticated facts, on both sides. The result will perhaps abundantly teach us the fatal error of becoming like the boisterous, though, we believe, philanthropic Maclean, the unalterable partisan of a given opinion, and of so closing our senses against conviction ,that facts pass by unheeded, and truth, instead of being followed and obeyed, is taken as a guide only as she suits the views of the party whose doctrine is to be supported at all hazards.

CASE 1.—William Woodley, ætat. 23, spare habit, went from East Hagbourn on Thursday, 22d March, to London. He slept at 31, Tooley-street (a street in which cholera prevailed at the time) the four following nights. On Saturday, 24th, diarrhæa came on. On the fifth night he slept in the neighbourhood of St. Albans, and returned to East Hagbourn on Tuesday 27th. Vomiting, with copious rice-coloured fluid evacuations, attended with violent pain of the bowels and cramps of the lower extremities, supervened on Thursday. The cramps left him the following day, but the diarrhœa continued until Monday. On Tuesday, the 3rd April, the stools were natural, and he was convalescent. The secretion of urine was natural.

CASE 2.—Mary Woodley (mother of the above, and nursing him), ætat. 51, corpulent, and generally unhealthy, was, on Sunday, April 1st, seized with violent cramps of trunk and extremities, universal coldness, vomiting, and purging with rice-coloured evacuations, blueness of the skin, &c. She died in 23 hours.

CASE 3.—Mary Winders, ætat. 70, nurse to the first case, had symptoms precisely similar to Mary Woodley, commencing on Thursday, the fifth of April. She sank in 15½ hours.

WESTMINSTER HOSPITAL.

AMBIGUOUS CASE OF LUXATION OF THE HIP.

Mary Williams, an unprotected female, about thirty years old, and of small figure, was brought into the hospital on Sunday the 12th February, under the following circumstances. The patient was conducted by two policemen, whom she charged with having inflicted upon her the injury under which she suffered, by dragging her forcibly through the streets. She was stripped and laid on a bed. The right leg was found to be *three inches* shorter than the left, inverted; the ball of the right toe resting upon the instep of the left foot. The limb could not be rotated or extended. Under these circumstances, Mr. Edwards considered the case to be a dislocation of the femur upon the dorsum ilii; and proceeded to the reduction forthwith. Sufficient extension was effected by means of two or three jack towels, and in about fifteen minutes the limb was apparently restored to its proper position, and the woman regained the use of the joint, and moved it in various directions. From this moment she had cessation from all pain, and fell almost immediately into a sound sleep. Great care was taken in regulating the force, the head of the bone was raised with much caution over the supercilium of the articular cavity, and the tension was lessened so gradually, that no sound was produced by the replacement of the bone.

For a week after this period, we are given to understand, the poor woman continued apparently convalescent, and was enabled to move about in bed without inconvenience. At this stage she was taken out of bed with great carelessness by the nurse, and, by *falling, luxated her hip a second time.* The limb was again shortened as much as ever: the inversion as well marked as before, but a slight rotatory motion might be produced. In this state she was seen by Mr. W. B. Lynn, the assistant-surgeon, who pronounced the case to be one of *fractured cervix femoris.* We believe the grounds of this last gentleman's opinion were the remarkable mobility of the femur, and a fancied crepitus produced by rotation.

In consequence of this discrepancy of opinion it was determined that a consultation should take place. Accordingly Sir A. Carlisle, Mr. Lynn, and Mr. Guthrie saw the patient; but there was no uniformity of opinion. Mr. Guthrie adopted the first view entertained of the case, and declared it was an ordinary dislocation, stating that in the course of his very extensive civil and military practice, he had not met with more than two cases of fractured cervix in which inversion of the limb existed, and these were owing to a diagonal fracture of the bone between the two trochanters, so as to leave the attachments of the psoas magnus and iliacus internus within, and those of the glutæi without the fracture. In these instances, however, the crepitus was so manifest as to admit of no ambiguity.

On Saturday, the 25th February, the patient was brought into the operation theatre, and publicly examined. When the impression, that a dislocation existed, became prevalent, the pulleys were applied, and the limb extended as far down as to make it even with its fellow, but no reduction was effected, and when the pulleys were removed, the limb was retracted. After this unsatisfactory proceeding, the patient was replaced in bed, and kept quiet for a few days. After this rest the affected parts presented the following appearances:—right leg shortened; the upper edge of the right patella *three inches* higher than that of the left. Inversion of the limb; ball of right great-toe rests on the left instep. The injured thigh forms a more acute angle with the pelvis than the left. The trochanter major higher than its natural position, and about three inches distant from the anterior superior spine of the ilium. The foot may be rotated outwards, until the great-toe comes in a line with the anterior spine of the ilium, and forms a right angle with the plane of the horizon. On each movement of the affected thigh, the caput femoris is perceived through the flaccid muscles on the dorsum acetabuli to move simultaneously with the trochanter. On placing the patient on her belly, and viewing the parts in their posterior aspect, a corresponding shortening of the different points of the limb is perceptible. The right nates much more tumefied than the left, forming a segment of a sphere. The round head of the femur is distinctly felt also on this aspect, moving at the same time with the trochanter at a distance of six inches from the tuber ischii (measured on the convexity of the integuments) six inches from the anterior superior spinous process of the ilium, four inches from the trochanter major, four from the middle line of the sacrum, and about five from the posterior inferior spinous process of the ilium; the concavity which is generally seen behind the trochanter-major was of course obliterated.

Saturday, 3d March, Mr. W. B. Lynn, the assistant-surgeon, made an ineffectual attempt at reduction, and was consequently more than ever confirmed in his original opinion of existence of fracture. The patient was put to bed, and in the course of the week a second consultation was held, at which it was determined to make another attempt on the ensuing Sunday.

Sunday, 10th March. Extension was applied, under the superintendence of Mr. Guthrie, with two pulleys, and after a stress of forty-five minutes, the bone returned (with a click) into its socket, as was proved by restoration of all the powers of the limb. The patient has continued well ever since, and is now (1st April) able to move the limb with perfect facility, and presents no deformity whatever.

EXOMPHALOS.—OPERATION.—DEATH.—POST-MORTEM EXAMINATION.

Sarah Bearby, a washer-woman, sixty years old was admitted on the 19th January, under Mr. White, with strangulated umbilical hernia. The poor woman had been subject to this species of hernia for thirty years; but on each descent of the gut she had been enabled to reduce it without difficulty. Four days previous to admission, however, the gut protruded, and she could not succeed in returning it. The bowels became constipated, nausea and vomiting succeeded, the countenance became anxious, and the pulse was rapid, but small and intermitting. On admission the tumour was as large as a moderate-sized breakfast cup, and more than the upper half of it was phlegmonous. Twelve ounces of blood were drawn from the arm by the house-surgeon; the patient was placed in a warm-bath, and nauseating doses of antimony were given, but without effect. A bladder filled with powdered ice was afterwards applied, but the tumour continued unaffected; and in the evening it was determined that the *dernier ressort* of an operation should be had recourse to.

On the upper part of the tumour, the integument forming the margin of the navel presented a peculiar twist, which Mr. White considered an index to the seat of stricture. Mr. W. B. Lynn, who performed the operation under the guidance of Mr. White, made an incision in the line of the linea alba, through the integument, covering the upper part of the tumour and the adjacent integument. Over the tumour the covering was about the thickness of a shilling, but over every other part of the belly there was at least an inch and a half depth of adipose substance. This last circumstance added a little to the difficulty of the operation. The operator now introduced a blunt-pointed bistoury, and made a slight incision in the upper edge of the umbilical ring; the sac was then opened, and found to contain a large mass of omentum, with some inches of strangulated intestine. This last was of a deep purple colour, which, however, verged towards crimson after a little exposure to the atmosphere. Some difficulty was now experienced in detecting the exact seat of stricture; this, however, was ascertained to be at the cœcal instead of at the duodenal end of the intestine, as was at first conjectured. At the point of stricture the ring of intestine was so much contracted as to render the tube impervious, and appeared almost in a state of sphacelus; no feculent matter could be obtruded through it, and the gut was returned with considerable misgiving as to the result: the mass of omentum having been long incarcerated in the hernial tumour was considered as a foreign body, and not returned. The wound was closed in the usual manner.

The patient was now placed in bed, and every means consistent with prudence was taken to promote the healthy action of the bowel; mild aperients and injections were administered, but no fæces were evacuated. The patient's features, however, betrayed no anxiety, and there was but little stomachic irritation discernible. Two days after the operation, Mr. Lynn finding the intestinal canal still torpid, and some irritation arising, prescribed calomel and opium: this quieted the irritability and induced sleep. No alvine evacuation, however, could be produced, and on the fourth day from the operation the patient expired.

Autopsy, thirty-six hours after Death.

The integuments generally were loaded with fat, and the omentum and other cellular processes in the abdomen were also charged with the same substance. The stomach and small intestines down to the point of stricture were distended with flatus, but contained very little of the ordinary pulp or fluid. From the point of stricture to the ilio-cœcal valve, a distance of about eighteen inches, the gut remained nearly impervious, and contained no flatus whatever. The large intestines contained a great deal of gas and but little fæcal matter. All the coats of the intestines were apparently healthy, except at the point of constriction, where there was great discolouration and thickening, and where the mucous membrane, both above and below the ring, was injected. The liver was rather indurated, but none of the other viscera afforded any particular morbid indications.

TO CORRESPONDENTS.

The letter of *Investigator* has been in type this fortnight.

Dr. Henry's letter, and many others, are also in type.

Erratum.—In the notice respecting the cholera in Paris, in page 27 of our last No., the word "patient" was omitted at the close of the article.

THE LANCET.

Vol. II.] LONDON, SATURDAY, APRIL 21. [1831-32.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE

IN THE UNIVERSITY OF LONDON,

Delivered March 5th, 1832.

CASE OF FEVER.

Trial of the Saline Treatment.

It is right, Gentlemen, that I should mention to you that the bad case in which I had recourse to the saline treatment, as it is called, has proved fatal. The first patient got completely well, but this patient died, not however through the fever, but the consequences left by it.

This woman, whose case I alluded to on a former occasion, was doing at that time perfectly well, and after that time she did still better, till from an accidental circumstance, as we may call it, she died. The woman was named Ann Cox. She was admitted on the 26th of January, and was forty-two years of age. She was brought here from St. George's workhouse, and it was reported that she had then been ill for a fortnight. She was found to have extreme debility, the whole abdomen was tender on pressure, and she could give no further account of herself than that she had been ill a fortnight. Her eye was heavy, and her countenance of a dark hue. She was a swarthy woman. Her tongue was extremely red, dry, and cracked; the pulse was exceedingly weak, nearly as weak as it could be, to be felt, and she had passed her fæces in bed. This was, in fact, an extreme case of typhus fever.

Treatment.—I found that, on account of the tenderness of the abdomen, twenty leeches had been applied immediately on her admission, followed by a poultice, and she had been ordered a dose of castor oil. The castor oil did not open her bowels, and it was therefore repeated. On the 28th it was judged necessary to attempt the further relief of the inflammatory state of the abdomen. Leeches, however, were not admissible, and a large blister was applied. It was also thought necessary to order her a pint of strong beef-tea every day. On the 29th her debility was so great that I ordered her half an ounce of wine every six hours. On the 30th the half ounce of wine was given every four hours. Mustard poultices were applied to the feet, and a glyster was given in the evening: not however by my directions, but it was judged necessary, and I have no doubt it was so. On the 31st, seeing how the saline treatment had agreed with the former patient, I ordered the woman a scruple of carbonate of soda together with ten grains of nitrate of potash every four hours, the wine being still continued. Arrow-root and milk were also allowed her.

She improved from this time steadily, but she was a very intractable sort of a woman, a bad woman, obstinate, and not at all accessible to reason. A sore was discovered on one of her hips, she being exceedingly thin, and having reclined more on that side than the other as she grew stronger. On the 7th of February the report is (this treatment having been continued), pulse only 112, having before been very quick. On the 10th two eggs were allowed in addition. She was very much emaciated, and the sore on the hip was sloughing. On the 16th she was allowed twelves ounces of wine a day. On the 27th she appeared really free from all fever, was perfectly convalescent, only in a state of great debility. On the 28th the sore looked exceedingly bad, and she appeared to be sinking from its effects. The same saline treatment was continued, and the wine was increased, the sore on the hip was getting well, the granulations looking very red, and she appeared to be doing quite well, with the exception of a sore that now took place on the other hip. She now became more obstinate than ever. During the fever we managed her pretty well, but when she came to herself, she showed her natural character decidedly. She would not take her wine, neither would she take much of her medicine; and after she had taken a part, she would endeavour to spit it out, but she was entirely cured of the fever. From the great debility, however, the great emaciation, the sore first on one hip and then on the other, and having no strength of constitution, she sank. It is probable that if the means had been employed that we wished, she would have recovered; but in practice we continually meet with obstinate people who, in fact, die from their own absurdity.

On opening the body, of course there was nothing found. I said I was sure that nothing would be discovered. I could not expect that there was ulceration or inflammation of the intestines. These might have been present during the fever, but she was so free from every symptom except debility, and a sore hip, that nothing of course could be expected. The report is (I was not present at the autopsy), "Bowels distended with flatus, stomach, intestines, and indeed all the viscera, appear in a healthy state. Some slight effusion under the membrane of the brain." That is a circumstance which is continually found in many persons who die from all other complaints; and whether there was or was not slight effusion was a mere matter of opinion after all. Perhaps the brain was a little softer and paler than usual. When however people die emaciated, the brain is usually in that condition. Nothing however was found worthy of the slightest notice. All the viscera of the trunk were healthy, and as to the effusion on the brain, when it is slight it is a matter of mere opinion. Some persons think, on making a post-mortem examination, that there is a little more serum than there should be, and some think there is a little less, so that we may fairly say that in this patient nothing was found, that she died from exhaustion left by fever, and the sore which occurred on the hip, from refusing to employ the proper means which we had administered when she was not herself, when she was labouring under fever and we could manage her. This case affords no argument whatever against the efficacy of the saline treatment. In severe cases of typhus I shall have recourse to it, until I thoroughly satisfy myself whether there is any advantage in it or not.

CASES OF PERICARDITIS.

Case 1.—The next case I will mention is one of a little boy who was in Jacob's ward, and who went out sometime ago with symptoms of pericarditis. I shall mention this case, in order to compare it with another which was further advanced. This boy was named Thomas Jones; he was sixteen years of age, and was admitted on the 3rd of January, on account of pain in the cardiac region, a great impulse at the wrist, and also of the left ventricle of the heart. On placing the stethoscope over the left ventricle, it was found to beat with considerable violence, but on striking around the heart there was no dull sound beyond the natural limits. The heart did not appear to be dilated, and there was no bellows sound to be heard, which was another reason for presuming that the heart was not dilated. The great impulse, however, at the ventricle showed, that, at any rate, the heart was labouring under extreme excitement, and the great pain, increased on pressure, also showed that, in all probability, there was an inflammatory state, and this opinion was still more favoured by there being a degree of pyrexia present, and from the patient having just before suffered an attack of rheumatism.

Treatment.—The treatment was simple, that of any common inflammation. Leeches were applied to the region of the heart again and again, and vinum colchici was given. This treatment was carried on with more or less perseverance, according to the exigency of the symptoms. Low diet was ordered, and the patient was kept in bed. He became so well that he went out, but no doubt, if he catches cold, he will be liable to a return of all the symptoms, and if he be neglectful, organic disease may arise. If there were any hypertrophy, it was of the slightest description. I could not satisfy myself that there was more than pericarditis. There was nothing more than may be got over by the patient living, if I may so speak, an antiphlogistic life.

Case 2.—But in the same ward you will see a boy in precisely similar circumstances, that is to say, he is about the same age, and has laboured under rheumatism, but who, besides the violent action of the heart and pain increased on pressure, has a strong bellows sound, and the action of the heart is far more violent. This boy is fifteen years of age, and reports himself as having been ill only five weeks, but this I do not believe. There was a great impulse in the region of the left ventricle, and a very loud bellows sound, and he also, as I have just stated, previously had rheumatism. The age of these individuals, the sex, the previous rheumatism, and the irritation of the heart, are similar, but in this case there is, besides the very great impulse, a loud bellows sound at the moment the blood leaves the heart. In striking over the region of the heart, there also is found a dull sound to a greater extent than usual.

Treatment.—He was first of all bled I see to ten ounces; three grains of calomel were given him, and a quantity of rhubarb. The next day I saw him, and ordered him to be bled over the region of the heart to ten ounces. I fancy that cupping over the heart in inflammatory affections of it is more serviceable than bleeding in the arm. In a few cases it is right to bleed in the arm in order to produce a great impression; but, as a general rule, I think bleeding over the region of the heart answers better. He was also ordered three grains of calomel twice a day, and of course put on slops, and from that time leeches were applied to the region of the heart, ten or a dozen every day, or every other day, till at last he was very much reduced. The calomel was continued till his mouth became sore. It was then kept in this state, and he was ordered to remain in bed. Under this treatment he improved so much, that he would get up, and he did so, without having many clothes upon him, indeed without any coat. The consequence of this was, that he had another inflammatory attack, and was obliged to lie in bed again, but this has been subdued under the use of leeches and antiphlogistic treatment.

Remarks.—Now these are cases which you will see every day in practice. It is impossible to practise medicine without being incessantly consulted on cases of this description. That is to say, violent palpitation of the heart after rheumatism, or during rheumatism, in young persons, attended with pain, and frequently with a bellows sound. They are all cases of an inflammatory state of the pericardium, producing, after it has continued a certain time, an inflammatory state of the substance of the heart. The left ventricle particularly suffers. It is more affected than any other part. The constant excitement there causes more substance to be deposited than natural, so that it becomes thicker, and generally also is harder, and very frequently it becomes dilated. It is not only hypertrophied, but its cavity is increased, so that you have dilatation. Under all these circumstances, you have a bellows sound. In this case the bellows sound was heard with the pulse; that is to say, it took place at the moment of the impulse of the heart, before the second sound which you hear in the heart. In by far the greatest number of cases the bellows sound takes place at the moment of the impulse,—before the second sound,—so as to be almost immediately before or quite immediately at the pulse at the wrist; but as the wrist is distant from the heart, of course there is a minute interval between the pulsation of the heart and the pulse at the wrist. If you feel the pulsation of the aorta, which you can often do in a case of aneurism of its arch, you will find the pulsation before that at the wrist, for, on account of the distance, the impulse must take a certain time to be communicated. The time is often so very minute as to be insensible, but occasionally you may perceive it.

Now the bellows sound, I have no doubt, always arises from an impediment to the flow of the blood. The opening by nature have a certain proportion to the cavities, and if disease diminishes an opening, then of course the blood cannot flow out with its usual freedom, and from the difficulty with which it passes the sides are agitated, so that there is a noise, just as a river flowing through a large arch of a bridge goes through with little noise, but if you diminish the arch in any way, then you hear a loud noise from the water. If again on the other hand the river be increased very much by a great flow of water in the country, then you hear the same noise. So it is with regard to the heart. If the cavity be increased, and the openings remain the same, you have a bellows sound. In the greater number of instances in which you have heard a bellows sound in boys and girls about the age of puberty, on opening them after death, you will find no disease of the openings at all, but the left ventricle is hypertrophied and dilated, so that the blood cannot flow with its usual freedom. It is a common thing in persons who are older, to have a bellows sound arising from a change in the aortic valves or the aorta, so that the opening has become narrow. Then you have a bellows sound from absolute obstruction, but in young persons you frequently have a bellows sound without any alteration of the openings, without any diminution of them, but from the left ventricle becoming so dilated, that the body of blood which rushes through is too great for the opening. On this account you will find in many cases that you may diminish the bellows sound, and even take it away entirely for a time by bleeding. If you lessen the volume of blood to a certain point, then there is less blood running through the diminished openings, or running from the enlarged cavity, and the blood consequently escapes freely, and passes without noise. But if you bleed to a very great quantity—if a person be reduced by bleeding—then the pulse will become very rapid. Now in this case, the heart having so little blood to impel, acts very violently; it drives the blood out with great force. The power of the heart is out of proportion to the quantity it has to move, and the blood is thinner than usual; it is therefore driven with such an impulse that you have a bellows sound from this cause, and as the person improves, the sound diminishes. I have no doubt that a bellows sound always arises from the blood finding more difficulty than usual in escaping from the cavity. A mere alteration in the position of the heart will produce the same effect. The heart is placed by nature in such a position, that the blood escapes with the greatest freedom from the left ventricle into the aorta. But in many cases of disease of the heart, the organ will lie quite in a different direction; it will lie across the chest, and in cases of this description you will have a bellows sound occur, because the blood does not go direct into the aorta. You will have the bellows sound alter according to situation. In an erect posture you will have the bellows sound diminish which takes place at the moment of the pulse. I presume it arises from the blood rushing more easily to the aorta in that posture. If a person lie down, and the abdominal viscera press upon the apex of the heart, and a bellows sound is then heard, in the greater number of cases it is diminished when the person stands up. I mentioned formerly an instance of this, where a woman had a bellows sound from ascites. The moment we tapped her it went away. The water reaccumulated, and as the abdomen became distended, the bellows sound was heard again. On again tapping her, away again went the bellows sound. Thus the bellows sound itself does not show disease of the openings of the heart by any means. It must arise merely from an impediment. There may be some organic disease in the openings, or there may be dilatation of the walls of the cavity, and I have no doubt it may arise from spasm; not, as Laennec says, when fibres are thrown into spasm, that you hear a bellows sound from a vibration of the fibres; but no doubt the spasm acts, by contracting the size of the opening, just as takes place in the urethra. I mentioned all these things in the book which I published some time ago on diseases of the heart, and as it is in the library, you can consult it there.

With regard to the second bellows sound, that which you hear after the impulse, that, I believe, as often arises, perhaps oftener than not, from the valves of the aorta not doing their duty. As soon as the left ventricle contracts, its effort is over, and it dilates. It is dilated and the blood rushes into it, and therefore no doubt, then, the blood from the auricle rushes into it, and if the blood find an impediment in rushing in from the auricle, if the mitral valve is diseased, then a bellows sound is heard; or suppose the valves of the aorta do not do their duty, and exclude the blood from the left ventriole when it is dilated, a certain portion will run back, and thus you may have a bellows sound; but if it all rushed in, then you would not.

There has been a difference of opinion as to whether the second sound which is naturally heard arises from the action of the auricles or not. I was not satisfied with any arguments that were brought forward against the opinion of Laennec, that it arose from a contraction of the auricles. It seemed to me that the auricles parted with their blood immediately after the ventricles, and that I believe still, but whether the auricle contracts at the time is of no consequence. Whether the second sound in the healthy state of heart arises from the contraction of the auricle is not important. As regards the second sound you hear in health, no doubt it arises from the blood rushing from the auricles into the ventricles. It is allowed, that whether the auricle contracts before or after the ventricle, still that the blood rushes into the ventricle the moment it has done contracting, and therefore if there be any impediment to that rush of the blood, of course you then have a bellows sound. I have never seen disease of the mitral or tricuspid valves, without the bellows sound taking place immediately after the pulse; but that is not the only source of that bellows sound; for if the blood rush back partially into the ventricles from the artery, then you will have a bellows sound. But with regard to the second natural sound, I should think from some observatiens that have been made, that it cannot be ascribed to the *contraction* of the auricles. I was not satisfied with any argument before, but I am satisfied with the argument now adduced, because I happened to see some of these experiments. A jackass was made insensible by prussic acid. A stethoscope was put down on the left ventricle, and I heard the first sound; and then the second sound was instantly heard afterwards clearly in the ventricle itself, and the auricle which of course I could not see, I was told contracted sometimes at the time of the second sound, and sometimes not—sometimes before, and sometimes after. In fact the contraction of the auricle is more like the contraction of a snake's tongue. The greater part of the auricle is a large sinus, and acts no more upon the blood than the sinuses of the brain. Every now and then the proper auricle acts before the ventricle, and the action is very insignificant, and I cannot be persuaded that it throws the blood into the ventricle. The ventricle seems to draw the blood from the auricle without any action of it worth notice. You know that a vacuum must be formed in the ventricles when they dilate. The blood attempts to run into them at the same time from the pulmonary artery and the aorta, but it cannot: it rushes into them however then spontaneously from the auricles. It is quite clear that the blood must run into the ventricles when they are empty. It is said that the auricle contracts before the ventricle, and therefore I am quite satisfied that the auricle has little to do with the passage of the blood into the ventricle. The blood rushes, as Laennec has said, and as I have maintained, into the auricle at the moment the ventricle has contracted, and whether it goes into it from the contraction of the auricle or not, is a matter of no consequence. It must at that moment leave the auricle. You have a bellows sound after the impulse of the heart, when there is any disease of the mitral or tricuspid valves impeding its progress from the auricles, or disease of the aortic valves, or the valves of the pulmonary artery occasioning the return of the blood into the ventricles not to be perfectly prevented. The blood leaves the auricle at the time that Laennec has said it does. You will see all the arguments in my work to which I have referred, but I will not read them, because you can consult them at your leisure. At pages 15 and 16 you will find my view of the subject, and though I have no idea of the second healthy sound being owing to the muscular contraction of the auricle, yet I have no doubt the second sound heard in health, arises from the blood quitting the auricle for the ventricle, and that the second sound heard in disease of the heart will arise from impediments to the blood leaving the auricle, and therefore Laennec, although he is wrong in saying the auricles contract at the moment they do, yet is right in saying that the blood *leaves* the auricle at the time he says it does. The blood does not leave the auricles by any action of their own, but a vacuum is found in the left ventricle, and it draws the blood into it from the auricle. The contraction of the auricle is slight, necessarily partial and most irregular as to its occurrence, being now before, now after the contraction of the ventricle, and often occurring several times to one action of the ventricle.

CASE OF PALSY FROM LEAD,

Treated with Strychnine.

The next case of which I will speak is one of palsy of the wrists from lead. The case is one of a very common description, but it is interesting on account of the exhibition of strychnine.

This patient was a plumber, twenty-seven years of age, and admitted on the 8th of December. His hands dropped. There was a loss of motion in his hands, costiveness, and occasional tormina. He was a painter four years ago, when he had the first attack of colic, and from that time he has had a dozen attacks. The present symptoms are palsy, with chronic pustules on the skin, which have nothing to do with the loss of power in his hands, and he is unable to extend either his hands or his fingers.

Treatment.—The treatment that I have found most successful in cases of this description, I mean the palsy, has been the application of stimulants, frequent blistering and electricity in sparks. Shocks do not appear so powerful as sparks. I have also found strychnine very serviceable. He was improving moderately, when on the 31st January I had several other patients in the ward taking strychnine for palsy, and I thought I would ascertain whether he would mend still faster with the addition of that medicine. The former treatment was continued—stimulating liniment and electricity, and his bowels were attended to by the exhibition of a quarter of a drop of croton oil every day. He also took the sixth of a grain of strychnine three times a day. On the 3rd of February he took the sixth of a grain, and on the 7th the fourth of a grain; and he improved, perhaps, more rapidly than before. On the 10th he took the third of a grain three times a day. On the 16th he complained of giddiness from the medicine, and in consequence of that it was diminished to the fourth of a grain. On the 21st he complained of a tingling in his limbs, and on the 28th the tingling was felt all over the system, and catchings of the arms and legs, with much giddiness, and a diminution of vision. Now it has been said, that when strychnine is given, it produces twitches and tingling only in the paralysed parts. We have exhibited it lately to many patients in the hospital on account of having had several cases of paralysis that arose from cold, and appeared therefore to be merely functional affection, without any organic change. Strychnine will not do good where there is organic change; but as these cases arose from cold, the patients took strychnine; and in all or nearly all, it has been observed that other parts were affected with twitches and tingling as well as the affected parts, as well as those parts which laboured under paralysis. In this man there has only been paralysis of the upper extremity, but he had twitches and tingling of the legs, and indeed all over the system. It has likewise been said, that this medicine affects only the spinal marrow and the nerves proceeding from it, but this is not the case certainly, for this person had giddiness, and he had also dimness of sight, and when the medicine was left off, those symptoms went off. There have been other cases where persons complained of giddiness, and I have seen persons intoxicated from it—rolling about; but they were not patients of my own, because I never give it to such an extent. The circumstance, however, might occur to any one where patients are particularly susceptible to its influence, or where more was given than was intended; but in general these affections may be avoided. I have seen intoxication and giddiness to such an amount as to be followed by stupor, and, in fact, temporary apoplexy from this remedy; and on one occasion I saw a person become apoplectic through it. At least a man did become apoplectic who was taking it; but I will not say through the medicine. But if it produce intoxication, giddiness, and convulsions, there is no doubt that if the medicine be immediately omitted, those symptoms will generally all cease, In nearly all the patients in the hospital there has been giddiness, and more or less affection of vision; but in all there have been a tingling of various parts of the body, and twitches not only in the affected parts, but in others also.

I may mention with respect to this man, that he got perfectly well; at least so well that he was able to go to work, and he took a quantity of the medicine with him in order to perfect the cure. The other cases appear to be doing well which had arisen from cold. I never saw it do good except in torpidity of parts, which has arisen from cold or lead, or some circumstance that produced torpor. When there is organic disease, or pressure, I cannot say that I have seen it do good, nor can I believe that it will. There is great room for deception in the treatment of paralysis. The disease often depends on temporary causes,—depends on some little effusion that will become absorbed, or congestion,—that will cease,—and you continually get credit for the cure of paralysis in cases where you deserve no credit at all. A large number of cases of paralysis may be merely temporary, and would do perfectly well if you took no means whatever,—at least if you adopted no means that were injurious. Some arise only from temporary causes, and will cease almost in spite of any harm that you can do; and when a patient continues to take the medicine for a length of time, it is of course asserted that the medicine has cured him, and I think often unjustifiably so. If a number of medicines be employed in succession, the last has the credit of effecting the cure. If you use strychnine in such cases as the present, I have no doubt but you will frequently find great benefit from it. It is a medicine that may be given with perfect safety. Some give it in one form, and some in another. I have never gone beyond the third of a grain three times a day. Of all the patients in the hospital, none are able to take this quantity long together; but when the twitches have become so great as nearly to throw them out of the bed, then of course it has been omitted, and then given again in a small quantity, and if that has not produced a decided effect, it has again been increased. It is a perfectly safe medicine, although one of the most powerful that is now employed in the practice of physic.

LONDON HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

DR. BILLING.

CASE OF SCURVY.

January 7, 1831.—GENTLEMEN, we have had a good case of scurvy in the clinical journal, which is rather a rarity of late years, since the improved mode of dieting seamen; and I shall occupy some time with the description, as the disease might possibly be sometimes mistaken by those who see it for the first time; but I do not think the disease likely to be mistaken in future by any of you who have seen this case, as it came to us in that state in which it perhaps most resembles syphilis, when the patches had scaly depressions, like the scaly spots of that disease, with other circumstances of which I shall mention the diagnostic peculiarities by-and-by.

Dec. 10. J. N. æt. 23, sailor, complains of pains in his chest and limbs, preventing him from walking. He says that about three months ago, during a voyage, he slept exposed to the night dews, and caught cold, having pains in the loins, arms, and across the chest, which are now much less severe than they have been. He has been above a week ashore; the skin and subcutaneous cellular membrane and muscles of the legs, feel hard, not yielding to pressure; and around the right ancle there is a dark-brown colour; the upper parts of both legs present the appearance of yellowish patches, like ecchymoses of bruises when becoming absorbed; and there are small scaly depressions like syphilitic spots, surrounded by lividity; pulse 86, weak; appetite good; tongue clean. Had seven weeks ago much bleeding from the gums, which are still spongy; bowels confined, and are subject to constipation; had taken a dose of opening medicine this morning.

Succi limonis recentis ℥j *ter die.*
Baln. tepid. 98°. *Full diet.*

You seldom hear now of scorbutic pains,

or sweeteners of the blood to remove scorbutic blotches proceeding from scorbutic humours—terms so frequently in use whilst there was any remnant of the humoral pathology passing current; and yet though our knowledge is increased, and the state of the solids is more generally taken into account in pathological explanations, there can be no doubt that indigestion and imperfect assimilation must produce a deterioration in the quantity of the blood, favourable to the generation of disease; and the mere circumstance, which must be familiar to you, that the serum of the blood, when drawn by venesection, is sometimes turbid, muddy, or milky, shows that it must vary in its qualities, though the opinions formerly held respecting putrescency of the blood were erroneous, whether in scurvy, fever, or other diseases. Pains are now chiefly denominated rheumatic, syphilitic, or neuralgic, and we scarcely hear of scorbutic eruptions, since the publications of Bateman and Willan, though many cases that are now denominated purpura, would in the olden time have been called land scurvy. Scurvy, whether sea or land, comes on with languor, sense of weariness, and pain in the limbs and loins, like rheumatism; but there is more alteration of tissue than in rheumatism, as mentioned in the case of T. N., and the uniform sense of oppression at the præcordia, must depend on the participation of the muscular structure of the heart. The external marks at first are, discoloration of the skin, spots in various parts like bruises, in various stages, some yellow and livid, others black and red; evident ecchymosis of blood escaped from the debilitated vessels. There is commonly a dirty yellowish tinge about the mouth and lower eyelid, like what is seen in persons who have indigestion, and call themselves bilious; and it is remarkable that both scurvy and rheumatism are connected with, and induced by, the same causes, damp and cold, or vicissitudes and indigestion, whether that indigestion be because the stomach gets food into it which it cannot digest, from the food being indigestible, or because the stomach is weak; or both together; the first inducing the second, whether on sea or land, as we see scurvy and purpura arise, either when the constitution is broken down by wretched diet, or when the digestion fails from some cause, sometimes suddenly in stout healthy-looking patients, at any age, in others as the gradual progress of disease. After the lassitude, pains, and blotches, have lasted for some time, the blotches, either by a slight scratch or rub, or spontaneously, are deprived of cuticle, and either ulcerate or a thin scale or scab forms, especially on the legs, looking like syphilitic spots, only instead of being copper-coloured, they are more livid, and have a blackish areola, the skin of which is weak, in the words of Hippocrates, as well as I remember them. Not that I quote him for the purpose of recommending you to wade through the dry desert sands of the ancient authors, for the sake of occasionally finding an oasis of information, for when I look back, I consider that, for my sins, I was doomed to waste many a precious hour in perusing them.

As the disease advances spongy granulations sprout up on the spots, and bleed; the internal rete mucosum partakes of the same state, and bleeding takes place from the nose, bronchia, intestinal canal or urinary organs; the gums becoming spongy, shrink away from the teeth, and if the disease be not checked, the teeth will fall out. Now you may find this scorbutic state of gums frequently in patients in the lower classes, if ill-fed or dissipated, or both together; and in the higher classes, if not ill-fed, yet from indigestion produced by dissipation, or other causes, the food though good not turning to good account; and though the scurvy has been termed, par excellence, sea scurvy, and said to arise from salted provisions, this is not the sole cause, for it may arise from any kind of bad food; in the first place, meat or fish which has been smoke-dried, or otherwise, without salt, will produce it: vegetable food if indigestible will do it, as bad heavy bread, such as bad rye bread, especially if made unleavened; dried peas and beans, and even good biscuit from being unleavened, help to produce scurvy. It appears that the substances which supply nourishment best are easily decomposed, supplying the chyle and blood with carbonaceous matter, loosely combined with hydrogen, oxygen, and nitrogen, which is especially the case with good meat, most esculent green vegetables, and bread which is baked in the progress of fermentation; acid fruits and especially lemons do this, but it is not the acidity which is useful, as mineral acids are not efficacious in scurvy, nor vinegar even, which approaches nearer in composition to lemon juice; nor is crystalized citric acid equal to lemon juice, which abounds in mucilaginous vegetable matter containing carbon, &c., a fresh supply of which is indispensable to nutrition and animal heat.

I have mentioned the distinction between scorbutic and syphilitic blotches. There are also other points of distinction. The pains in the legs might induce you to examine the shins for nodes, which will not be found in simple scurvy. I say simple, because it is not impossible that you might find a syphilitic taint co-existing; you may have in scurvy so solid a mass, from the muscles being hardened and stiff, and their cellular sheaths consolidated with the periosteum, as in the case of J. N., that you might be induced at first to suppose there was syphilitic thickening of the periosteum, but you must recollect that when there are nodes, the thickening takes place anteriorly, as well as laterally, causing irregularity of the shaft of the tibia, whereas in scurvy, unless when the ulcers or blotches obscure the feeling, the face of the tibia is smooth and regular, unlike nodes. Again, the pains may be distinguished from those which are syphilitic previous to nodes appearing, by their not being peculiarly worse in bed, but rather increased on motion, like rheumatic pains, from which the blotches, &c. will distinguish them, but especially the spongy gums, which would also distinguish the complaint from syphilis. This might be thought a sufficient guide, and so it is when you find that the patient has not been taking mercury, but every ship almost is so well supplied with blue pill and calomel, that it is a chance if you do not find the mouth partly under the influence of mercury, administered by the captain if the ship be too small to have a surgeon on board. J. N. had had pills given on his voyage home, but his mouth was not under the influence of mercury, for you all know well enough, that the moment the mouth becomes even slightly affected by mercury, the tongue becomes furred, and this will help you to distinguish scorbutic from mercurialized gums. I hope I have now shown you sufficiently well how to distinguish scurvy, as mercury, which is used so indiscriminately now a days, is more likely to do harm than good in it. I expect some day to see a parody of the *Currus triumphalis Antimonii* of Paracelsus. Indeed, Abernethy's recommendation of blue pill is a near approach to it. Mercury is undoubtedly one of the most universally useful medicines we possess; and yet, except some venereal cases, (and even this point is sub judice,) there is no disease, which is curable, that may not be cured without it; and the same may be predicated of antimony, notwithstanding three quacks have had great success with it at different periods—first, Paracelsus, second James with his powder, and lastly a gentleman of the name of Wilson, above thirty years back. A dissenting clergyman, who had a smattering of physic, ingeniously formed a soluble powder which had all the effect of James's powder, and was even more certain in its action, being a combination of a minute dose of tartar emetic, with a few grains of some neutral salt to disguise it. Of this he sold immense numbers of packets, to habitual physic takers, who used them dissolved in tea, soup, &c. two or three times a day, by which he made a considerable appendix to the profits of a small congregation. He wrote also a very tolerably ingenious octavo, on the constitutional treatment of diseases, recommending the same remedy for each. I mention these circumstances respecting two of the most useful medicines we possess (and which you see me make such constant use of), merely to warn you not to depend too much on any one medicine, lest it make you indolent in prescribing, for you must never hope to find such a short cut to practice, as to realize the dreams of the alchemists, by a medicine which can cure all cases.

J. N. had had a dose of opening medicine given him in the morning before I saw him, and I did not think it necessary to order him anything more than the lemon juice, to the amount of three lemons per day, with full diet of the hospital, and a warm-bath at 98° Fahr., which produces a sensation of relief where there are general pains, though perhaps contributing little to the actual cure; and I must even warn you, that in some of the very badly aggravated cases of scurvy, the patient could scarcely bear the fatigue which a warm-bath would produce, as in the advanced stage a slight exertion will sometimes produce fatal syncope.

The report on the 12th was—Has less pain in his limbs, and can walk better; pain still in loins; bowels relieved. *Ordered linim. saponis cruribus.* The friction with a mild liniment has the effect of dispersing the swelling, as after a bruise, when the tenderness is gone off. From this time he improved gradually, without any additional prescription, except some opening medicine on the 15th, at which time the discoloration of the legs was reported to be disappearing.

On the 23d the legs were becoming softer, and I pointed out to the gentlemen in the ward, that as the legs became softer, the outline of the muscles, when put in action, was distinctly marked by sharp lines, which were not perceptible at his admission, the whole being an unyielding mass. On the 3rd instant the report was — Hardness and discoloration nearly gone; no pains; he can walk well, and feels quite well. To-day there is no perceptible stiffness of the muscles, nor discoloration of the skin, and he will leave the hospital next week.

CHRONIC HEPATITIS.

Jan. 7. I have next to explain to you the nature and treatment of a case of chronic hepatitis, in which you will have an opportunity of seeing how useful calomel and other preparations of mercury are, and I shall take the opportunity to show you how far it is desirable to carry their action, and how to secure the advantages you gain thereby.

J. J., ætat. 41, labourer. Nov. 18. Has been ill for about nine months, with severe dull pains in the epigastric and hypochondriac regions, eructations, cardialgia, and nausea after taking food; appetite bad; thirst; bowels constipated; fæces dark-coloured and scybalous; urine high-coloured; remarkably so about seven weeks ago; sleeps pretty well; is generally chilly; skin drier than natural; feels constantly languid and low-spirited; was formerly addicted to intemperance; has been exposed to being over heated by steam, and suddenly chilled by currents of air, in his business of dyer's labourer. He applied for relief as an out-patient a week ago, at which time his countenance was anxious, jaundiced, and sallow; he was at that time ordered,

> *Pil. hydrargyri*, gr. v, *nocte maneque; Magnesiæ sulph.*, ʒj; *Ex. inf. gent. co.*, ℥iss *ter die; Empl. ammoniaci c. hydrarg. hypochond. dextro.*

He now feels lighter, and has less pain; countenance improved, but he has no appetite, and is thirsty; tongue moist and slightly furred; pulse 76; bowels have been relaxed, and are to-day much purged, though he has taken no medicine yesterday or to-day; motions yellowish, with griping; urine pale and copious; headach for the last two or three days; skin dry; chilliness.

> *Baln. calid. statim. Capt. sulph. quininæ*, gr. j, *ex. inf. rosæ co.* ℥iss *ter die. Omitt. cætera. Diet; milk, bread, and rice.*

This man had been exposed to two very common causes of disease of the liver—intemperance, and heat; the heat and vicissitude of temperature to which he was subjected, having an effect analogous to a hot climate, and equivalent to the receipt given by a French cook to make a fowl's liver large, viz., to tie it by the leg close to a large fire. In these cases the liver loses its natural purplish brown colour (the true liver colour), and becomes of a mottled yellowish pale brown, like this specimen from the museum, called dram-drinker's liver. Commonly it is swollen, sometimes little altered in its consistence, but generally rather firmer than natural, except when the patient dies after mercurial medicine has caused a partial reduction of its size, in which case it is sometimes flaccid. When this kind of liver is cut into, you sometimes find bile diffused through, and tinging, its substance; not confined to the ducts as in the healthy organ. This accounts for jaundice arising, though the larger ducts leading to the duodenum be quite pervious; for, the chronic inflammation obstructing the minute ducts and pores, the bile becomes diffused in the substance of the liver, and is carried thence by the absorbents into the blood. At the same time sufficient bile may pass into the duodenum to prevent the fæces from being clay-coloured; and yet, the liver being unsound, this bile is not of a quality sufficiently good to play its part in digestion, as was in this case evinced by the scybalous motions, the eructations, nausea, &c., which might have been attributed to disease of the stomach, were it not that the jaundice established the diagnosis, and that there was nausea only, not vomiting, which occurs ordinarily when the ventriculus itself is diseased. There were in this case pains and sense of fulness in the region of the liver and stomach;—of the liver, on account of its being the seat of chronic inflammation; and in the stomach, duodenum, and transverse arch of the colon, on account of the indigestion and flatus, owing to defect in the biliary secretion. You will find very frequently in persons who are jaundiced, that there is itching of the skin similar to that of prurigo, which is uniformly connected with indigestion. The urine was high-coloured, both on account of the tinge of bile, and of the feverishness, which feverishness was evinced by thirst, chilliness, and dry skin. There is a remarkable difference in the effect on the spirits, between chronic inflammation of the liver and of the lungs, as you find here reported that the patient was languid and low-spirited, whilst in phthisical affections the spirits are generally buoyant. This man was reported to sleep well, and jaundiced patients are frequently drowsy when not kept awake by pain.

When he applied first for relief as an out-patient, the indication was to give him the mercurial pill both to improve the secretion of bile, and to diminish the chronic inflammation. And at the same time to relieve the indigestion (without waiting for the slower action of the mercury), by administering a bitter unstimulating tonic, the inf. gent. co., combined with enough magnesiæ sulph. to free the bowels. The plaster has the effect of supporting and relieving pain, and the plaster here prescribed is the one usually employed on account of the mercury in its composition, though but little of it can be absorbed. Under this treatment he had improved considerably, but at admission he was too much purged, and as his tongue showed the slight fur which precedes salivation, I ordered him to take only a grain of sulphate of quinine in solution, ter die, and a warm-bath to promote the circulation on the surface, and to relieve his bowels at the moment, which were over purged. With this medicine his appetite improved, and he felt better, though still dyspeptic, and requiring opening medicine occasionally; his complexion improved, and the motions were of a natural colour; the epigastrium and hypochondrium were still however tumid, and percussion proved this to be from visceral enlargement, not flatus, requiring the renewal of mercurial action; but as his mouth appeared susceptible, I ordered him gr. iii calomel, every second day, combined with rhubarb gr. xv, as a tonic laxative, and at the same time six leeches to the epigastrium every second day. On the 6th of December this had relieved his symptoms, but he was pulled down, and I allowed him ℥iv wine, and beef-tea, and the bitter infusion again, which improved his appetite. On the 23rd, as he had been for some days free from pain, the leeches were omitted. On the 27th his mouth became sore, and he had renewal of pain in the right side, with diarrhœa, but no fulness or tenderness on pressure; the pain seemed rather griping in the arch of the colon; I then left off the mercurial, and ordered sulph. quinine gr. v ter die, and no other medicine. I find in these cases that the quinine secures the good effects produced by mercury, without which it has been found that the patient will frequently require a renewal of the mercury almost by the time the mouth gets well, and would be thus worn out between the effects of the remedy and disease; we have an analogy also greatly in favour of the employment of quinine in affections of the liver and spleen, inasmuch as they are so frequently connected with ague, and in those cases relieved by that medicine. Within the last ten days he has got the appearance of returning health, and has recovered spirits and confidence, and as he appears well I shall let him leave the hospital in another week, provided I find he can digest animal food, which he is now beginning to make use of, and that he acquires strength to work.

FORMER CASES CONCLUDED.

Fever.—At the last lecture, I mentioned the circumstances under which I was induced to give J. H. a small dose of tincture of opium (page 722), of which I am so cautious in all cases of fever. The report taken next day was:—

Jan. 1st. Feels better in every respect; still uneasy about his family; no pain in his head, but has a humming noise in his left ear. Says he did not sleep in the night; but it is ascertained that he did sleep, and was not delirious. Skin soft and temperate; pulse 108, strong and full; tongue white and moist; thirst; no nausea; bowels open twice; craves much to be allowed meat. *Perstet.*

This strong desire for animal food always pleases me, as an evidence of fever being gone; but I am very cautious in gratifying it, as I have known serious relapses take place in consequence of mistaken indulgence by friends or attendants. I directed, however, that he should have about an ounce of meat, as we ought in general to comply with the dictates of nature to a certain extent; and such a restless subject as he is, is more likely to get sleep if not thwarted unnecessarily. You will find besides, that, usually, convalescents, though feeling strong appetite, are satisfied with a mouthful or two, on account of the weak state of the stomach, and will not ask for the same thing a second time, which if not granted would have kept them longing for days. His appetite is now moderate; he had a dose of ol. ricini on the 3d, and continues taking the decoction. Yesterday, the 6th, he was marked convalescent.

Fever.—L. S. (page 722), whose case afforded such a perfect example of fallacious symptoms of debility, has made satisfactory progress.

Jan. 1. Still drowsy. Says he should feel very well but for the cough; no crepitation; he slept in the night, but muttered constantly; he is slightly deaf but perfectly sensible, and his eyes are lively; pulse 94, full and soft; tongue dry, not furred; bowels open once; skin soft. *Perstet.*

You may observe, that the leeches to the temples removed what remained of the comatose state, and as that subsided he became sensible to, and complained of, the annoyance of the cough; but that ceased to be of consequence, as there was now no crepitation, no heat or dryness of skin; in fact both fever and inflammation were subdued.

2. Feels better; had a good night. *Perstet.*

On the 3d, as he was better in every respect, except that the catarrhal affection of the bronchia was very troublesome, and as his head was quite well, I did not hesitate to give him a small quantity of opium, in the form of *Dover's powder*, of which I ordered him gr. v, with gr. v *pil scillæ co.*, two pills three times in the 24 hours. This had nearly removed the cough, which today is very slight and loose, with some rhonchus mucosus; he is, in fact, convalescent, complaining only of weakness, on account of which I have left off the expectorant, which is now not only unnecessary but tends to diminish the appetite, and I have ordered him *sulphate of quinine* instead.

ST. GEORGE'S HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

DR. SEYMOUR,

April 7th, 1832.

PHTHISIS.

GENTLEMEN, I have now two cases of this disease under my care in the hospital; and I will therefore take the opportunity of making a few remarks to you upon the disease. Physicians of the present day have thrown aside the hope, which those of former days supported, of the possibility of curing it; yet this fact ought not to deter the young practitioner from doing all in his power to alleviate many of the painful symptoms which this disease presents, for it is one to which perhaps he may be called more frequently than to any other.

Case 1.—One of the cases which I have in the house is phthisis of the florid kind. In these you may have more or less of fever present, running through the hot and cold stages, with hectic flushings of the cheeks towards the evening, and profuse expectoration from the lungs and bronchiæ in the morning. The pus expectorated in the last stages of consumption much resembles that secreted from an inflamed membrane. Where you find in a patient the slightest symptom of hæmoptysis (except it be during the menstrual period), it should immediately excite your utmost vigilance and attention. If you should see a young fair man, with clear complexion, fine teeth, long neck, and narrow chest, who has had hæmoptysis, you will examine the throat; but do not let the appearances which it may present draw off your attention from the mischief which may be going on within the chest. In a case of the kind, the hæmorrhage may go off, but expectoration will surely succeed it; and again, the hæmorrhage will come on, and every symptom of phthisis will be set up, and this may occur in the course of a few months.

Case 2.—The other case which I have under my care is a female; and among females you will frequently find phthisis of a *languid* kind. In these cases the menstrual secretion is totally wanting, or not in sufficient quantity; and females in general put down this as a primary symptom, although in this disease it is to be considered but as secondary. These cases of phthisis are rarer than the former ones. Where you find the cough and the expectoration cease, you will generally find the diarrhœa come on, and vice versa. Of all the other symptoms you will in general find the diarrhœa to be the most violent, as it is usually attended with great atony, or want of action, and ulceration of the small intestines and colon. If the ejections from the bowels are bloody, you had better use bleedings in small quantities with castor oil. If, however, these symptoms show themselves more particularly in the latter stages of the disease, you must give such medicines as will stimulate the intestinal coats, of which I think Ward's paste (the confectio piperis nigri of the London pharmacopœia) to be one of the very best, or you may give the black pepper in a draught, with a small quantity of opium. These medicines will stop the diarrhœa better than any others I am acquainted with. You must look upon the diarrhœa only as symptomatic of vomicæ in the lungs and ulceration of the intestines. The above cases, Gentlemen, are cases of tuberculous phthisis.

Remarks.—There are other forms of the disease, however, besides these common ones of phthisis tuberculosa. You will sometimes find the mucous membrane of the bronchiæ inflamed, with obstruction to respiration, and to the proper circulation of blood through the pulmonary vessels. The patient's lips become dark. He has turgescence of the countenance, with headach, heaviness over the eyes, &c. This is phthisis cutanhalis or chronic bronchitis. Sometimes you will find a patient lose his flesh, and become weak, thin, and emaciated; pulse never above 100; hectic flushings of the cheeks; frothy saliva; voice suppressed and scarcely to be heard. With these he will have pain in the upper part of the windpipe, till at last, exhausted by his sufferings, he sinks and dies; and on examining the body after death, you will find ulceration of the cartilages of the larynx. This form of disease is denominated phthisis trachealis; it may be unconnected with any other disease, or it may be complicated with an affection lower down in the lungs. You will sometimes find a patient affected with a variety of this disease termed phthisis calculosa, wherein he will spit up portions of calcareous matter. You will in general find these to consist of phosphate of lime or urate of soda. This form of disease may exist without the patient having any symptom of phthisis pulmonalis. If, however, the two diseases should be joined together, which is sometimes the case, the patient will always linger longer, or be more susceptible of relief from art, than where they attack him singly. Eight years ago I attended a case of this nature with Dr. Warren, wherein the patient received much benefit from being put upon mineral acids, and a full and more generous diet.

There was a gentleman, who was a pupil of this hospital five or six years ago, who had phthisis calculosa; he was treated with mineral acids, and sent into the country for free air, from which he received great benefit. I have heard it reported, that a professional gentleman, who was formerly physician to St. Thomas's Hospital, was affected with this form of phthisis. He went to sea for some time, returned home, became physician to St. Thomas's Hospital, and lived for many years.

There is another variety of the disease termed phthisis cancerosa, which you will in general find connected with some malignant tumours of the lungs of the nature of fungus hæmatodes, and with the formation of cancerous tumours in other parts of the body, or it may appear after an operation for cancer. Cases of this form of phthisis are very rare except where they succeed to some malignant disease.

I now come to that form which is most commonly met with in practice, and which you will be called upon most frequently to treat, viz. phthisis tuberculosa. In this species there are warty unorganised formations found in the lungs. Sometimes these warts are found diffused in small tumours throughout the substance of the lungs, when the term *miliary* has been given to them. Refrigerants, sedatives, milk diet, and pure air, are the usual means on which we place reliance in the ordinary forms of this disease. Where the skin is cool, and there are no moral depressing causes, steel is generally given. Are tubercles in the lungs the result of previous inflammation? *I do not believe that they are*, for tubercles are found in the lungs of a still-born fœtus, an animal which never breathed. It is a frequent remark, that persons who are employed in needle manufactories are often the subjects of phthisis tuberculosa. I believe this statement to be in a great measure correct, for they are frequently affected with inflammation of the lungs; lymph is poured out, and phthisis is thus set up.

Hemlock, mercury, and iodine, have been sometimes given. Hemlock, ʒj infused in ℥j of ether, is sometimes used with benefit as an inhalent; but the great difficulty to be got over in all these diseases, is to prove whether the patient actually ever had tubercles in the lungs, and whether such remedies have ever cured them. The experiment is not an experimentum crucis. Such cases, however, open a great field of operation to charlatans, quacks, &c., who are eager and anxious to cast a slur upon the character of every upright and honourable professional man. Emetics of zinci sulphas and cupri sulphas used formerly to be given. I have no faith in the use of iodine in this disease. If the vomicæ are full formed, you will have afternoon fever, evening hectic, pulse at 120, and profuse sweating. There will be formed an excavated cavity in the upper part of the lungs, and the patient will very soon sink under the disease. The inhaling of tar and pitch has been recommended, and the balsams of Peru and myrrh have been given. In catarrhal phthisis these latter remedies are decidedly useful, but in excavation of the lungs I never knew them do any good. Bark was given by the old physicians. Resinous substances are of more service in phthisis cutanhalis than in any other form of the disease. But the remedy which you will find most useful in the early stages of phthisis pulmonalis is counter-irritation; it is one on which you may place the greatest reliance, and it is one which you may use to a very great extent without injury to your patient; but you must be careful not to make your external sore greater than your internal one,—instances of such malpractice I need not bring to your recollection. Counter-irritation may stop ulceration, and accurate observers of the disease have placed setons in the chest over that part of the lung which was ulcerated. In inflammation of the lungs, the posterior part is generally affected. In phthisis pulmonalis the upper part of the lung is in general the seat of tuberculous deposits. In the languid form, where there is much pain in the chest, steel medicines with blisters will be beneficial. Many persons have, however, a very great aversion to being blistered, and when you name it to them will say, "Oh, if I am to be blistered I will go to some one else." Mild nutritious food of all kinds may be given, but you will find, very commonly, that patients will imagine that everything you give them in the way of diet or food is to cure. Iceland moss has sometimes had this virtue ascribed to it; I need not tell you how falsely. If this is steeped in water to free it from its bitterness, and then ℥j of it is boiled in milk, it forms a very nutritious article of diet. Every remedy should be used here to alleviate the symptoms. The tussilago farfara, the decoctum malvæ, linseed tea, nutritious vegetables, quiet, and change of air, all these may be employed.

A gentleman, who was formerly an officer of this hospital, and whose family were of a phthisical disposition, in ten days lost four children out of five. Great anxiety of mind and depression of spirits were the result; and in six months this gentleman died of phthisis pulmonalis.* In this disease the medical practitioner should endeavour, as far as he can, to inspirit the moral feelings and to strengthen the habit of body.

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* We believe Dr. Seymour alluded to the case of the late Mr. Rose.—*Rep.*

The colliquative sweating, the diarrhœa, and the profuse expectoration, are the principal symptoms to which attention should be directed. The sweating may sometimes be checked by sponging the patient well with warm vinegar and water before it comes on, or the mineral acids with water may be given; if this symptom, however, should continue in spite of these remedies, you will always find the acetate of lead stop it. You may give it in doses of gr. iij with opium, in a pill, or in a draught according to the following formula:—

℞ *Liq. plumb. subacetat.;*
Tincturæ opii aa ♏ xxv;
Aquæ distillatæ ℥iss. *Misce, fiat haustus.*

You must be careful to use *distilled water*, or you will have a precipitate. You may give lead in very large quantities in this way, and for a very considerable time, without its producing any ill effects upon the patient. It is of most service when given in cases of florid phthisis. The diarrhœa must be treated according to the circumstances of the case. Equal parts of milk and the infus. cortex granat. is a very useful remedy in cases where the diarrhœa is very severe. Where you have reason to suspect ulceration of the intestines, a draught containing black pepper gr. xxv, and tincture of opium ♏ xxv, will tend to check this symptom. If the patient expectorates with difficulty, the pil. scillæ comp. is a very useful remedy. Where on the other hand the expectoration is profuse, it may be checked by giving the patient equal parts of lemon juice and syrup of poppies. You must in all these cases adopt your remedies according to the form and stage of the disease.

IMPUTED NEGLECT OF DUTY BY DR. SIGMOND.

To the Editor of The Lancet.

Sir,—As you profess to be the "Pupil's Friend," I will thank you to take notice, in your Journal, of the extreme remissness of Dr. Sigmond.—I believe we have now not had a lecture on Materia Medica from him for some weeks. At any rate I have been to the School repeatedly of late, and the answer has always been, "Dr. Sigmond don't lecture to-day."

I remain, yours truly,
A Pupil of the Little Windmill-street School.
April 17, 1832.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXVI.

EPIDEMIC CATARRH IN HORSES.—THE MALIGNANT EPIDEMIC.

Although, Gentlemen, too many veterinary practitioners have been accustomed to include nearly every affection of the chest under the convenient title of inflammation of the lungs, and to adopt one unvarying mode of treatment for all, so summary and unscientific a mode of proceeding will not satisfy you or me. I will not inquire into the cause, although I lament the prevalence, of that unfounded and injurious opinion, that the diseases of domesticated animals are few, easily understood, and easily treated; but will proceed to describe, so far as I am able, some of these morbid affections of the respiratory organs, unacknowledged in our schools—unacknowledged in some of our standard books,—acknowledged and understood, however, by our best practitioners, and which are clearly referable to different portions of the respiratory apparatus, characterised by different symptoms, attended by different results, and demanding different treatment.

EPIDEMIC CATARRH.

I will begin with that disease so prevalent a few weeks ago, and recognised under the name of influenza, distemper, catarrhal fever, and epidemic catarrh. The latter term I am inclined to adopt, although he, who, in my mind, stands highest of all among our veterinary writers, sneeringly tells me that he has "heard veterinary surgeons talk of *influenza* and (though rather unlearnedly) of *epidemic* catarrh among horses." I confess myself one of the unlearned here. It is a term which best of all explains the disease *epidemic*—and approaching nearer than any other to the character of severe *catarrh*.

The Subjects of the Disease.—In the spring of the year,—a cold wet spring,—and that succeeding a mild winter, and especially among young horses, and those in high condition, or made up for sale, or that have been kept in hot stables, or exposed to the usual causes of inflammation, this disease principally, and sometimes almost exclusively, prevails. Those that are in moderate work, and that are correspondingly fed, generally escape; or even when it appears in most of the stables which are in a narrower or wider district, horses in barracks, regularly worked and moderately fed, although not entirely exempt, are, comparatively, seldom diseased.

Symptoms.—If it has been observed from the beginning, the attack will usually be sudden — ushered in by rigor, and that quickly succeeded by acceleration of pulse, —heat of mouth—staring coat—tucked-up belly—diminution of appetite—painful but not loud cough—heaving at the flanks—injected nose — weeping eye — dejected countenance: these are the symptoms of catarrh, but under a somewhat aggravated form.

How distinguished from Pneumonia and common Catarrh.—It clearly is not inflammation of the lungs, for there is no coldness of the extremities—no looking at the flanks—no stiff immoveable position—no obstinate standing up. It is not simple catarrh, for, as early as the second day, there is evident debility. The horse staggers as he walks.

Nature of the Disease.—It is inflammation of the respiratory passages, generally. It commences in the Schneiderian membrane, but it gradually involves the whole of the respiratory apparatus. Before the disease has been established four-and-twenty hours, there is sore throat. The horse quids his hay, and gulps his water. There is no great enlargement of the glands; the parotids are a little tumefied, the submaxillary somewhat more, but not at all equivalent to the degree of soreness. It is inflammation of the lining membrane of the pharynx, or spreading from the larynx to the posterior part of the mouth. The soreness is excessive; day after day the horse will obstinately refuse to eat. Discharge from the nose soon follows in considerable quantity, thick, very early purulent, and sometimes fœtid. The breathing is accelerated and laborious at the beginning, but does not always increase with the progress of the disease,—nay, sometimes a deceitful calm succeeds, and the pulse, quickened and full at first, soon loses its firmness, and although it usually maintains its unnatural quickness, yet sometimes it even deviates from this, and subsides to little more than its natural standard. The extremities continue to be comfortably warm, or at least the temperature is variable; and there is not in the manner of the animal, or in any one symptom, a decided reference to any particular part or spot, as the chief seat of disease.

Progress of the Disease.—Thus the disease proceeds for an uncertain period; occasionally for several days; in not a few instances through the whole of its course, and the animal dies exhausted by extensive or general irritation: but in other cases the inflammation assumes a local determination; we have bronchitis or pneumonia, but of no very acute character, and difficult to treat from the general debility with which it is connected. Sometimes there are considerable swellings in various parts, as the chest, the belly, the extremities, and particularly the head. The brain is occasionally affected: the horse becomes stupid; the conjunctiva grows alarmingly red; the animal is first unconscious, and delirium follows. A curious thickening is sometimes observed about the tendons—it would be mistaken for severe sprain. It is seen under the knee, or about the fetlock. It is hot and tender, and the lameness is considerable. The feet have occasionally suffered. Here there has been determination far more violent than the original disease; there has been separation of the laminæ and descent of the sole. You may easily imagine how roaring should be connected with epidemic catarrh; but I never knew this disease to be followed by glanders, although some writers have affirmed that this is the case. These metastases are not critical; but we are rather glad to see them, except indeed when the feet are attacked, for the disease seems inclined to shift its situation or character, and is more easily subdued.

Debility the chief Character.—The most decided character of this disease is debility. Not the stiff, unwilling motion of the horse with pneumonia, and which has been mistaken for debility—every muscle being needed for the purposes of respiration, and therefore imperfectly used in locomotion—but actual loss of power in the muscular system generally. The horse staggers from the second day. He threatens to fall if you move him; he is down, permanently down on the third or fourth day. The emaciation is rapid, extreme—the animal becomes a mere bag of bones.

Final Stage.—At length your medical treatment succeeds, or nature begins to rally. The cough a little subsides, the pulse assumes its natural standard; the countenance acquires a little more animation; the horse will eat a little of some choice thing; and health and strength slowly, very slowly indeed, return; but at other times when there had been no decided change in the character of the disease, no manageable metastasis of inflammation while there was sufficient power left in the constitution to struggle with it, a strange exacerbation of symptoms accompanies the closing scene. The extremities become deathy cold, the flanks heave, the countenance betrays greater distress, the membrane of the nose is of an intenser red, and inflammation of the substance of the lungs and congestion and death speedily follow.

At other times the redness of the nostril suddenly disappears; it becomes purple, livid, dirty brown, and the discharge is bloody, fœtid; the breath and all the excretions become fœtid too; the mild character of the disease gives way to malignant typhus; swellings, and stinking ulcers spread over different parts of the frame; and the animal is soon destroyed.

Epidemic.—This disease is clearly attributable to atmospheric influence, but of the precise nature of this influence we are altogether ignorant. It is some foreign injurious principle which mingles with and contaminates the air, but whence this poison is derived, or how it is diffused, we know not. It is engendered, or it is most prevalent, in cold ungenial weather; or this weather may dispose the patient for catarrh, or prepare the tissues to be affected by causes which would otherwise be harmless, or which may at all times exist.

It is most frequent in the spring of the year, but it occasionally rages in autumn and in winter. It is *epidemic*, it spreads over large districts. It sometimes pervades the whole country. Scarcely a stable escapes. Its appearance is sudden, its progress rapid. Mr. Wilkinson had 36 new cases in one day. I have been told of a celebrated practitioner in London, who had nearly double that number in less than twenty-four hours.

Endemic.—At other times it is endemic. It pervades one town; one little track of country. It is confined to spots exceedingly circumscribed. It is dependent on atmospheric agency, but these require some injurious adjuvant; or the principle of contagion must be called into play. I have known it rife enough in the lower parts of London, while in the upper and north-western districts scarcely a case occurred. Not many weeks afterwards there was not a mews in Marybone which did not contain some patients, while Westminster was exempt from disease; and, in Marybone, I have known it to be confined to a district not a furlong square. In one extraordinary case every fifth horse in a certain mews died, while there was no vestige of disease elsewhere. I recollect that, in one of our barracks, the majority of the horses on one side of the yard were attacked by epidemic catarrh, while there was not a sick horse on the other side.

Most prevalent in Crowded Stables.—These prevalences, and these exceptions, are altogether unaccountable. The stables, and the system of stable management, have been most carefully inquired into in the infected and healthy districts, and no satisfactory difference could be ascertained. One fact, however, has been established, and a very important one it is to the horse proprietor as well as the practitioner. The probability of the disease seemed to be in a tenfold ratio with the number of horses inhabiting the stable. Two or three horses shut up in a comparatively close stable would escape. Out of thirty horses distributed through ten or fifteen little stables, not a horse would be affected; but in a stable containing ten or twelve horses the disease would assuredly appear, although it was proportionally larger and well ventilated. It is on this account that postmasters and horse-dealers dread its appearance. In a sickly season their stables are never free from it; and if perchance it does enter one of these largest stables, almost every horse will be affected. Therefore it is that grooms have so much dread of a distempered stable, and the odds are so seriously affected if distemper has broken out in a racing establishment.

Contagious.—Does this lead to the conclusion that epidemic catarrh is contagious? Not necessarily, but it excites violent suspicion of it, and there are so many facts of the disease following the introduction of a distempered horse into an establishment, that we can hardly refuse to rank this disease among those that are both contagious and epidemic. It is strange what contrariety of opinion there is here between the theoretical and the practical man. I never met with a well-informed groom, or an extensive owner of horses that was much among them, or a country veterinary surgeon of much practice, who entertained a doubt about the matter. The shrewdest and most successful calculators are they who are most alarmed at and influenced by the report of this disease in a favourite stable; yet there are veterinary surgeons, and of deserved eminence too, who stoutly deny the contagiousness of distemper in horses. It is a subject that merits more attention than has hitherto been paid to it.

Precautions.—I would however advise you, Gentlemen, to adopt every necessary precaution. Remove from the stable as quickly as possible the horse that exhibits symptoms of epidemic catarrh. Remove the affected horse and not the sound one. The apparently sound one may have the disease lurking about him, and may more widely propagate the disease.

Treatment. — *Bleeding.* — The medical treatment of epidemic catarrh is far from difficult. It is a disease of the mucous membrane, and of an extensive portion of it, and thus connected with much debility. It is nevertheless a febrile affection, and for a little while, of considerable intensity. This fever must be subdued, and in a way least likely to increase the subsequent debility.

Recourse must be had to the lancet, and blood abstracted at once, and that in sufficient quantity to affect the constitution. There is no fact so evident or so important in veterinary pathology, as that early and copious depletion will abate inflammatory action without permanently debilitating the system; while repeated small bleedings will weaken the constitution, and perhaps remove chronic inflammation, yet will not have the slightest effect on that which is of an acute character. Therefore bleed early, regulated by the condition of the animal, by the fullness of the pulse, and more particularly by the effect of the operation. Here, more than in any other case, we should bleed ourselves, or stand by while the animal is bled; and here, more than in any other case, it is necessary that the orifice should be large, that the blood should flow quickly, that the pulse should falter, and that the depletion should then be immediately suspended. In proportion as you fear the possible typhoid form which the disease may assume, let your measures be prompt and decisive.

Medicine.—In this insidious disease the bowels are usually constipated. The pellets of dung are small and hard. Give that which has been recommended in catarrh,—small doses of aloes, combined with the usual fever medicine,—the digitalis, nitre, and emetic tartar. If by auscultation, joined with a comparison of other symptoms, you are assured that there is little or no affection of the chest, a mild dose of physic may be exhibited, but there is some danger attending this, and the laxative doses of aloes are preferable. As soon as the fæces are pultaceous, (even before), and before purging commences, omit the aperient, and continue the fever medicine, and that until the inflammatory action is evidently subdued.

Diet.—In a disease in which it is so necessary to abate fever, and yet not to weaken the system, strict attention, much stricter than the veterinarian usually enforces, or the groom dreams of, should be paid to diet. No corn must be allowed, but mashes and thin gruel. The horse will not drink gruel if he can get at water; then let his water be entirely taken away, and a bucket of thin gruel suspended in his box. This is an excellent plan with regard to every sick horse that we do not wish to reduce too much; and when he finds that the morning and the evening pass over, and his water is not offered to him, he will readily take to the gruel, and drink as much of it as is good for him. Green meat should be early offered; such as grass, tares (the latter especially), lucerne, and, above all, carrots. If these cannot be procured a little hay may be damped, and offered morsel after morsel by the hand; and should this be refused, it may be damped with water slightly salted, when the patient will generally seize it with avidity.

Should the horse refuse to eat during the two or three first days, do not be in a hurry to drench with gruel; it will make the mouth sore, and the throat sore, and teaze and disgust; but if he should continue obstinately to refuse to feed, nutriment must be forced upon him. Good thick gruel must be horned down, or, what is better, given by means of Read's pump.

Counter-irritants. — You will often and anxiously have recourse to auscultation. You will listen for the mucous rattle, creeping down the windpipe, and entering the bronchial passages. If you cannot detect it below the larynx, you will apply a strong blister reaching from ear to ear, and extending to the second or third ring of the trachea. If you can trace the rattle in the trachea you must follow it,—you must blister as far as the disease has spread. This will often have an excellent effect, not only as a counter-irritant, but as rousing the languid powers of the constitution. A rowel of tolerable size between the fore-legs cannot do harm. It may act as a derivative, or it may take away a disposition to inflammation in the contiguous portion of the chest.

Fomentations.—The inflammation which characterises the early stage of this disease is at first confined to the membrane of the mouth and the fauces. Can we apply fomentations? We shall allay irritation, and open the exhalents, and soften the texture. But how apply them here? Why to the very part by means of a hot mash; and that not thrown into the manger, over which the horse will not hold, and you cannot to any effectual purpose confine his head, or where he may be scalded or disgusted, or where, at all events, the bran will soon get cool; but placed in that too-much-undervalued and discarded article of stable-furniture the nose-bag. The vapour of the water will, at every inspiration, pass over the inflamed surface. In the majority of cases relief will speedily be obtained, and that suppuration from the part so necessary to the permanent removal of the inflammation,—a copious discharge of mucus or purulent matter from the nostrils—will be hastened. If the discharge does not appear so speedily as we could wish, we can in the same way apply a stimulant to the part. The vapour impregnated with turpentine arising from fresh yellow deal saw-dust, used instead of bran, will have very considerable effect in quickening and increasing the suppuration. It may even be resorted to almost from the beginning, if there is not evidently much irritability of membrane.

A hood is a useful article of clothing in these cases. It increases the perspiration from the surface covering the inflamed part, a circumstance always of considerable moment.

General Treatment.—An equable warmth should be preserved, if possible, over the whole body. The hand-brush may be gently used every day, and harder and more effectual rubbing may be applied to the legs. The patient should if possible be placed in a loose box, in which he may toddle about, and take a little exercise, and out of which he should rarely if at all be taken. The exercise of which the groom is so fond in these cases, and which you must in the most peremptory terms forbid, has destroyed thousands of horses. The box should be airy but not cold. Lay this down as a golden rule for the treatment of most chest affections. The air should be fresh and uncontaminated but never chilly. Your object is to increase and not to repress cutaneous perspiration; to produce if possible a determination of blood to the skin, and not to drive it to the part already too much overloaded. The box should be airy, but sheltered and comfortable, and the clothing rather warmer than usual. These are the dictates of common sense, and your experience will confirm their propriety.

Cautious Treatment.—Well, Gentlemen, I will suppose that you are proceeding with your case somewhat slowly, and not quite satisfactorily to yourselves or your employers. There is not much fever; there is little or no local inflammation, but there is great emaciation and debility, and total loss of appetite. You may lessen the quantity of the sedative, but you must not omit it altogether; the fire may not be extinguished, although for a little while concealed. There are no diseases so insidious and treacherous as these. You may lessen the quantity of fever medicine, but you must not venture on a tonic. Many a horse has been lost by the impatience of the practitioner or the owner. The concealed, half-smothered flame has burst out afresh, and consumed all before it. There is one medicine which you may give, an excellent refrigerent, and yet a mild and safe stimulant, the spirit of nitrous ether, which may be added to your half-doses of the former medicine in the quantity of two drachms, or perhaps half an ounce. You must attend more sedulously to the feeding. Try every kind of green meat that you can obtain, particularly give carrots nicely scraped and sliced,—change the food every day as the capricious appetite prompts,—if necessary, force with gruel, as thick as it will run from the horn; but give no corn, no not a morsel. The nurse, in the form of an attentive groom, will be the best doctor in this stage of the disease.

Tonics.—Are you yet making no progress, then you must venture on a tonic, but with a great deal of caution. Still administer the fever medicine, but add to each ball a drachm of gentian. When there has been much previous fever it may be prudent to prepare the way even for this by the administration of a drachm of powdered camomile flowers morning and night during one day. If this compound of sedative and tonic medicine has no effect, I will permit you, though unwillingly, to omit the former. Give one drachm of gentian, half a drachm of ginger, and half an ounce of the spirit of nitrous ether, in a little gruel; and this having produced its effect, the appetite being a little recalled, the horse being evidently somewhat better, do not make more haste than good speed. You may very easily give one dose of tonic medicine too much. You may kindle the fire afresh. When nature is set a going after such a disease let her alone, or if you must continue to give medicine—if the prejudices of the master or the groom seem imperiously to require it, give the best drug your pharmacopœia now affords—a few linseed-meal balls.

I know not which is most to be dreaded, or has been the cause of most murders, playing with the disease at first, or hurrying the process of convalescence afterwards. Gentlemen, when you have lost your first horse by the injudicious use of tonics, remember what I have now told you.

The personal attendance of the Veterinary Surgeon.—You will gather from what I have said, the paramount importance of your personal and most diligent attention to these cases. This involves the whole secret of the treatment of epidemic catarrh. Such cases you should get if possible into your own infirmary, where you may see your patient many times in the day; or if you must visit him at the stable of the owner, let no day pass without your early and late attendance on him. A thoroughly-qualified assistant, a veterinary surgeon cannot always afford to keep, and in some views it would not be prudent so to do. A rival in skill might turn out by-and-by a dangerous rival in practice. If you must content yourselves with the assistant who will do what you desire him, and of whose rivalry you have no fear, let your own personal attendance be exemplary, and especially let it be so here. To the practised eye the treatment of the disease is simple enough; but there is no malady that requires closer watching, or in which a blunder would be so likely to be fatal.

THE MALIGNANT EPIDEMIC.

Continental veterinarians describe a malignant variety or termination of this disease, and *our* imperfect history of veterinary medicine is not without its records of it. So lately as the year 1815 an epidemic of a malignant character reigned among horses. Three out of five who were attacked died. It re-appeared in 1823, but was not so fatal. It was said that the horses that died were ultimately farcied—the truth was that swellings, and ulcerations with fœtid discharge appeared in various parts, or almost all over them—the swellings of the complaint we have been considering, but aggravated and malignant. Our recollection of the classic lore of our early years, will furnish us with instances of the same pest in distant times and countries. We have not forgotten the vivid description of Apollo darting his fiery arrows among the Greeks, and involving in one common destruction the human being, the mule, the horse, the ox, and the dog. Lucretius, when describing the plague at Athens, speaks of a malignant epidemic affecting almost every animal—

Nor longer birds at noon, nor beasts at night
Their native woods deserted; with the pest
Remote they languished, and full frequent died.
But chief the dog his generous strength resigned.

The Cause of the Establishment of Veterinary Schools.—In 1714 a malignant epidemic was imported from the continent, and in the course of a few months destroyed 70,000 head of cattle. It continued to visit other countries with but short intervals for 50 years afterwards. Out of evil, however, came good. The continental agriculturists were alarmed by this destruction of their property. The different governments participated in the terror, and veterinary schools were established, in which the anatomy and diseases of cattle might be studied, and the cause and treatment of these periodical pests discovered; and from the time that this branch of medical science began to receive the attention it deserved, these epidemics, if they have not quite ceased, have changed their character, and have become comparatively mild and manageable. As however they yet occur, and are far too fatal, I must endeavour to collect the symptoms, and point out the treatment of them.

Symptoms of Malignant Epidemic.—In horses the disease was almost uniformly ushered in by the prevailing symptom of the common epidemic, inflammation of the mucous membrane of the respiratory passages, but soon involving other portions, and then ensued diarrhœa, which no art could arrest. The fever, acute at first, rapidly passed over, and was succeeded by great prostration of strength. The inflammation now spread to the cellular texture, and there was a peculiar disposition to the formation of phlegmonous tumours; sometimes there were pustular eruptions, but oftener deep-seated tumours rapidly proceeding to suppuration. Connected with this was a strong tendency to decomposition, and unless the animal was relieved by some critical flux or evacuation, malignant typhus was established, and the horse speedily sunk.

Brugnone's Account of the Epidemic of 1783.—The most satisfactory account of one of these epidemics is given us by Professor Brugnone of Turin. It commenced with loss of appetite, staring coat, a wild and wandering look, a staggering from the very commencement—the horse would continually lie down and get up again, as if tormented by colic, and he gazed alternately at both flanks. In the moments of comparative ease there were universal twitchings of the skin, and spasms of the limbs; the temperature of the ears and feet was variable. If there happened to be about the animal any old wound or scar from setoning or firing, it broke open and discharged a quantity of thick and black blood. Very shortly afterwards the flanks, which were quiet before, began to heave, the nostrils were dilated, the head extended for breath. The horse had by this time become so weak, that if he lay or fell down he could rise no more; or if he was up he would stand trembling, staggering, and threatening to fall every moment; the mouth was dry, the tongue white, and the breath fœtid—a discharge of yellow or bloody fœtid matter from the nose—fœtid blood from the anus. The duration of the disease did not usually exceed twelve or twenty-four hours; or if the animal dwindled on, swellings of the head and throat and sheath and scrotum followed, and he died exhausted or in convulsions.

Post-mortem Appearances.—Black spots of extravasation were found in the cellular membrane, in the tissue of all the membranes, and on the stomach. The mesenteric and lymphatic glands were engorged, black, and gangrenous. The membrane of the nose and the pharynx highly injected, the lungs were filled with black and frothy blood, or with black and livid spots. The brain and its meninges were unaltered.

Further History.—It commenced in March 1783. The barracks then contained 116 horses; all but thirteen were attacked, and seventy-eight of them died. The horses of both the officers and men were subject to attack; and three horses from the town died, two of which had drawn the carts which conveyed the carcases away, and the other stood under a window, from which the dung of an infected stable had been thrown out. The disease would probably have spread, but the most summary measures for arresting its progress were adopted; every horse in the town was killed that had had the slightest communication with those in the barracks. One horse was inoculated with the pus discharged from the ulcer of an infected horse, and he died. A portion of his thymus gland was introduced under the skin of another horse, and he also died.

Cause.—The disease was supposed to be connected with the food of the horses. All the oats had been consumed, and the lolium temulentum, or awned darnel, had been given instead. *It is said* that the darnel is used by some of our brewers to give an intoxicating quality to their malt liquor. I well recollect, some five-and-thirty years ago, when an acre or two of the Battersea-common field used to be annually sown with it, and it used to be jokingly said, that nobody knew what became of the crop. For fifteen days no alteration of health was perceived, and then in less than eighteen hours half of a troop perished. The stables were not crowded, and there was no improper treatment. A man disinterred some of the horses to get at the fat; swellings rapidly appeared in his throat, and he died in two days. A portion of their flesh was given to two pigs and some dogs, and they died.

M. Brugnone found that bleeding only accelerated the death of the patient. He afterwards tried, and ineffectually, acids, cordials, purgatives, vesicatories, and the actual cautery, and he frankly attributes to the power of nature the recovery of the few who survived.

Gilbert's Account of the Epidemic of 1795.—M. Gilbert describes a malignant epidemic which appeared in Paris in 1795, characterised by dulness, cough, loss of appetite, weakness, pulse at first rapid and full, and afterwards continuing rapid, but gradually becoming small, weak, and intermittent. The bowels at first constipated, and then violent purging succeeding. The weakness rapidly increasing, with fœtid breath, and fœtid evacuations. Tumours soon appeared about the limbs, under the chest, and in the head, the neck, and loins. If they suppurated and burst, the animal usually did well; but otherwise he inevitably died. The formation of these tumours was critical; if they rapidly advanced, it was considered as a favourable symptom; but if they continued obscure, a fatal termination was prognosticated.

Treatment.—Bleeding, even in an early stage, seemed here also to be injurious, and increased the debility. Physic was given, mild and nutritious food, gruel, cordials; deep incisions were made into the tumours, and the cautery applied; stimulating frictions were used, but all were of little avail.

Summary of the Nature and Treatment.—I have narrated these cases at considerable length, in order to give you some idea of the nature of this disease, and because, with the exception of a short but very excellent account of the malignant epidemic, in the last edition of Mr. Blaine's Veterinary Outlines, you will not find any satisfactory history of it in the writings of our English veterinarians. It is evidently a disease of the mucous membranes, both the respiratory and digestive. It is accompanied by early and great debility, with loss of all vital power, with vitiation of every secretion; with effusions and tumours every-where, and it runs its course with fearful rapidity. If you saw it at its very onset, you would bleed, but if a few hours only had elapsed, you would find with MM. Brugnone and Gilbert, that venesection would only hasten the catastrophe. You would administer stimulants mingled with opium,—the spirit of nitrous ether in doses of three or four ounces, with an ounce or more of laudanum. The quantity of opium would be regulated by the spasms and the diarrhœa. These medicines would be repeated in a few hours, combined perhaps with ginger and gentian. If these failed, I know not who could do. Deep incisions into the tumours, or blisters over them, would be proper measures; but perhaps your principal attention would be directed to the arresting of the contagion. The infected would be immediately removed from the healthy. All offensive matter would be carefully cleared away, and no small portion of chloride of lime would be used in washing the animal, and particularly his ulcers; it might with great propriety be administered internally, while the stable and everything that belonged to the patient would undergo a careful ablution with the same powerful disinfectant.

OBSERVATIONS
ON SOME POINTS CONNECTED WITH THE
ANATOMICAL PATHOLOGY OF THE MALIGNANT CHOLERA.

By P. H. Green, A.B. M.B., *London.*

During the last few weeks various opinions have been delivered by medical writers on the nature of cholera, and on the deductions which may be drawn from post-mortem investigations. Several ingenious conjectures have been thrown out, but it appears to me that hypothetical reasoning has too often taken the place of fact, and that many have indulged in physiological and pathological speculations, not founded upon actual inspection of the dead body. Before we can form a just estimate of the nature of a disease, which is certainly new to every indigenous English practitioner of the present day, it is necessary to accumulate a large body of facts, to multiply observations, to eliminate all erroneous impressions, and by a careful comparison of symptoms of morbid appearances, and of the effects produced by different therapeutic agents, to afford a foundation on which the future nosologist may erect a safe and lasting superstructure. Such an accumulation of facts can only be made by the combined industry of medical men in the various branches of the profession. Let the practical physician, the physiologist, the chemist, and the morbid anatomist, each in his own department, ascertain facts and collect observations, and we shall then by an accurate survey of the whole, have the means of forming a just estimate of this novel and fatal disease. With this view, I take the liberty of stating one or two facts connected with the pathological anatomy of cholera, which I have observed; they are not perhaps of much importance, but as their accuracy is established by various preparations, now in my possession, they may be accepted as a mite to the fund which the industry of abler investigators will no doubt soon accumulate. As a preliminary, I would remark, that we are not to trust to the colour or apparent vascularity of an intestine, as a proof of existent inflammation. Dr. Venables has exposed the chemical error of believing every greenish fluid contained in the stomach of a cholera patient to be bile; the florid colour of the intestines has frequently led observers into a similar error, which a careful inspection of the prepared intestine would have removed. I have remarked, on many occasions, that exposure to air, to water, and perhaps the action of the various substances contained in the intestines, have communicated a florid colour to the blood in the capillary branches, and thus given the intestine a colour nearly as deep as in inflammation; but by pressing on the trunk of the vein, the dark blood would be driven in to some of the smaller branches, and in part diminish the florid appearance of the injection.

The mucous coat of the intestines of patients who die, both in the primary and secondary stages, sometimes exhibits large florid vascular patches which look very like inflammation; by preparing the intestine I have ascertained that these injected patches are formed by extravasation of serum, holding in solution the colouring matter of the blood, into the submucous tissue; the extravasated fluid is not blood, for it is too light-coloured; it is generally darker in the neighbourhood of the vessels from which it is poured out, and gradually becomes lighter and more transparent.

I have also prepared portions of the great and small intestines in various patients, and in all have found the arteries distributed to the villous coat, remarkably contracted, often reduced to mere lines, so as to require a magnifying power to render them apparent, and in a condition quite incompatible with the existence of inflammation, while the veins were distended in their trunks, and injected in their smallest branches. I do not mean to affirm that this condition of the arteries is constant, but simply declare that I found it where the intestine was very florid and vascular.

I shall conclude these few observations by the statement of another fact, which indeed the great serous discharges per anum would lead us *a priori* to conclude; viz. that the capillary mouths of the veins opening on the intestines are preternaturally relaxed. This conclusion is warranted by the observation of a circumstance which has occurred to me more than once in the course of my dissections.—While removing the intestinal contents from a portion of gut tied at both ends, and opened by a short incision to give exit to the rice-water fluid, I have noticed that the first portions of the fluid were quite milky, but on pressing the gut between the fingers the fluid always became pinkish; on allowing the fluid to stand for some hours it separated into two portions, one white and fibrinous, the other, which floated on the top, reddish, and composed of serum holding the colouring matter in solution; from this fact I conclude that very slight pressure was sufficient to make the serum and colouring matter exude from the capillaries, and also that the additional fluid was discharged by the capillary veins, because on minute inspection I found the veins dilated, and the arteries contracted. These few observations are given as the result of some little practical acquaintance with the pathological anatomy of cholera, and may perhaps lead to more extensive and useful researches.

NOTE FROM DR. O'SHAUGHNESSY.

To the Editor of THE LANCET.

SIR,—The editor of a contemporary journal has paid me the compliment of informing his readers that I am the sub-editor of THE LANCET. Permit me to state that he is mistaken, as I have never occupied that situation.

I am, Sir, your obedient servant,

W. B. O'SHAUGHNESSY, M.D.

WESTMINSTER MEDICAL SOCIETY.

Saturday, April 14th, 1832.

Dr. Stewart in the Chair.

THE CHOLERA.

Dr. **Gregory** commenced the discussion by asking Dr. Johnson whether he still adhered to the opinions he advocated some months ago in a letter or two addressed to the *Courier*. Dr. Johnson had therein stated it as his opinion that the variations of temperature were prejudicial to the violence and spreading of such a disease as the cholera in this country, and he now took the liberty of asking him whether circumstances had caused any alteration in his judgment.

Dr. **Johnson** wished that his catechiser, when quoting, would stick a little more closely to the facts. He was glad at all events to find that an opinion of Dr. Johnson's should, after a lapse of several months, form a topic of deliberation for the mind of Dr. Gregory. But to the point. What had he stated ! Why he had run the risk of prognosticating, as an individual, that the cholera either would not attack this country, or that, if it did, the climate, assisted by the habits of the people, would put an efficient check upon its course. He had travelled somewhat further from his native land than Dr. Gregory; in fact, from the land of the origin of this disease to the West Indies, and having thus been enabled to compare its effects on various countries, he had been enabled to predict that it would be devoid of its usual virulence in this country. Had not the result fully confirmed his views ? Look at it in London and Paris. In England this epidemic had now been six or seven months ; in that time it had visited twenty or thirty individual spots, and carried off two or three thousand persons. Did not this support his argument ? Fever had killed an immense number compared with the "raging pestilence." In particular spots, as at Musselburgh and Ely, where the cholera *had* been very destructive, there were peculiar local circumstances to account for it, and in London eight-tenths of the cases occurred on the right bank of the river.

Dr. **Gregory** thought Dr. Johnson unnecessarily warm. His object in popping the question had been merely to ascertain whether Dr. Johnson's present opinions corresponded with those of yore, and now having been satisfied with the reply, he should have the more pleasure in examining the details of the letters referred to.

Dr. **Granville**. — The little rencontre commenced by Dr. Gregory was very amusing. He, and no doubt the Society generally, thought that the object of the query was, after obtaining Dr. Johnson's admission, either to convict him of error, or to give confirmation to his opinions. But no ; the only consolation after all was, that the learned Doctor would now look back and peruse the documents to his perfect satisfaction. Really Dr. Gregory, from his official opportunities, might have been expected to bear some testimony, either negative or confirmatory, to the statements of Dr. Johnson, which, in the opinion of the speaker, had been fully borne out.

Mr. **Greenwood**, adverting to Dr. Johnson's observations on the cases in the Borough hospitals, said he had, from personal investigation, acquired certain facts, overlooked by Dr. Johnson, and tending to completely overturn his deductions. The account then given by the steward of Guy's was, that the cholera had been introduced by a poor man, who having been found lying at one of the gates on Friday, was taken into Petersham's ward, and there died on the succeeding Sunday with every symptom of cholera. Well, upon going to the matron's room in this ward (so we understood) she answered his questions about the nurses. He found that in that ward out of thirteen cases, eight had had cholera, of whom five died. One of the nurses, Eliz. Powel, a night-nurse, was allowed the liberty of going out during the day. One evening she returned, after being absent some hours, when being taken suddenly with the different symptoms of cholera, she was removed to Petersham's ward and died the next morning. In this ward the nurse who succeeded her ten days after, slept in the same bed. This was followed by an attack of the same disease. Eliz. Crabbe, one of the night-nurses, went to Bermondsey where the cholera was raging, stopped there a week, returned, and was seized with the cholera morbus. What conclusion was now to be drawn from the cases in this hospital ? He thought that Dr. Johnson had leaped to his conclusions without fully examining into facts, and considered that the details he had given were decidedly corroborative of the doctrine of contagion. He had seen in the *Morning Herald* of a late date a letter from the French correspondent, who stated that a change of opinion seemed to have taken place amongst the faculty in Paris, who now thought that the disease was an epidemic spreading by contagion. This was a set-off to what he had designated the "hasty conclusions." He concluded by referring to the excellent effects caused by the institution of distinct cholera hospitals ; no doubt the insulation thus established by the Board of Health had tended most materially to prevent the spreading of the disease by contagion.

Dr. **Granville** wished to inquire of Mr. Greenwood what became of the nurses who attended the patients in the *day* time, for the *night* nurses only had as yet been mentioned. And it was more than probable that they spent the day in carousing. Besides, Mr. Greenwood seemed to have gained all his facts from the matron and steward, overlooking the medical officers.

Mr. **Greenwood** was unable to give the required information, and thought that what should be looked at in these instances was, the grand chain—the general bearing. As to the authenticity of his facts, it was no matter where they were obtained. Would Drs. Addison or Bright have any objections to offer against them?

Mr. **Chinnock** concurred in the propriety of Dr. Granville's question, and in these cases it was at all times desirable to know the sources whence the information was derived.

Mr. **Greenwood** replied that he had waited, and had been unable to see the medical officers. However, the inquiry was not into the pathology of the cases. The mere enunciation of the facts was what he sought, and perhaps, therefore, the simpler the source the better. The nurses could have no particular views to bias their story.

Dr. **Gilkrest** *had* inquired of the medical officers of Guy's respecting the cases alluded to. Mr. Stoker informed him that, after the strictest investigation, no trace of a contagious origin could be discovered. The first case was that of a nurse. As the gentleman who spoke last had quoted a morning paper on a change of opinion respecting contagion in France, he would ask him what he thought of the recent declarations by Alibert, Richerand, and others, well known in medicine. They had given judgment against contagion. Regarding this point, contagion, he had lately witnessed in Limehouse some circumstances which gave him additional confidence in his previous opinions. At a house in Tile Yard, a man called John Coffin died of cholera, and the next day a girl in the same house also fell a victim. Being Irish, of course a "waking" took place. The bodies were put on a table with merely a sheet over them, and for several days were continually surrounded by the friends and relations. While ill, Coffin had been visited by his sister, brother-in-law, and several others; the girl also had been seen repeatedly by her father, mother, and friends, and the medical men had rubbed the body, but of the whole circle of these persons not one had taken the disease, although the circumstances spoken of occurred a week ago. Now surely this could not be called a contagious disorder.

Mr. **Burnett** related two cases which had just occurred in the cholera hospital of St. Giles's. They were two nurses who had been in attendance on the cholera patients, and to that must be ascribed their seizure, for the hospital was warm and well ventilated, and they had not been allowed to leave it, in addition to which they were very well fed. He ascribed much importance to the history of individual cases, because if, in one single instance, the communication of the disease by contagion could be clearly traced that would be sufficient, because the possibility of such a mode of propagation was the thing contended for. That would declare the truth of contagion, the degree might be decided by further investigation. Look at small-pox. He himself did not take it until three years ago, although of course repeatedly exposed to it in its worst forms for years previously. Persons were every day running the risk of suffering from acknowledged contagious diseases without taking them, but was this said to prove their non-tagiousness? No; because it was considered that those persons did not risk the exposure under circumstances favourable to the communicability of the infection. Seeds would not flourish in every soil, and it was much the same with the germs of disease. Alluding to Dr. Johnson's prophecy, he could not allow him the merit of being singular in his prediction.

Dr. **Gregory** had also seen the cases in St. Giles' Cholera Hospital, and corroborated Mr. Burnett's statement, adding, however, that there were *three* nurses attacked, the last in the afternoon of the day preceding.

Mr. **King**, in illustration of his view of the propagation of the disease, said, that supposing the cholera to prevail on the Continent,—England being free from it,—if any quarantine regulations were established, they ought to be such as would rather prevent Englishmen from going to the diseased country, than the people of that country from coming hither; but he deemed any quarantine at all perfectly useless.

In reply to a question from Dr. Blicke,

Mr. **Burnett** stated, that the nurses in St. Giles' Hospital had had a full supply of food, but had been prevented from excess. As to the wages they received enabling them to obtain drink or luxuries, the fact was, that they talked of striking for an increase of wages on account of the danger of the service. In the use of the warm-bath, it had been found that the water-bath was preferable to the action of vapour. The former raised the warmth of the skin in a much better manner and more speedily, the latter tending to cause blisters if carried to the same degree of heat.

Dr. **Blicke** thought that excess in *eating* was the circumstance most predisposing to an attack of cholera. As to *drinking*, he really deemed that it had a contrary effect—the habitual drunkard keeping his body in a continual state of excitement. There were some points illustrated by the marching of troops, of which he had been an eye-witness, and which might contribute to the general view of the character of the disease. When he was with the army in India, he had known a regiment march from a place in perfect health, but after a journey of seven miles, experience an attack of cholera. Several died there; the regiment was then marched back, during which time seven died. Not a new case, however, occurred. They had then gone on again, taking a circuitous route, avoiding the former fatal spot, and thus escaped completely. ["That is an exception."] No, it was not, for the same thing occurred again and again, and those facts spoke volumes. Moreover, if a captain of a vessel were on shore, and found any of his men seized with symptoms of cholera, what did he do? Why he immediately ordered all hands on board and struck far out to sea. This was the general practice, and showed what experience dictated respecting contagion. Small-pox had been instanced, but there was no comparison between this and the disease in question, even supposing the latter to be contagious. Expose one hundred children who had not had small-pox, to the influence of the disease, and ninety-nine of them would be found to take it.

Dr. **Webster** had come in late, but he understood Mr. Burnett's cases as intended to illustrate the doctrine of contagious influence. Mr. Burnett had stated, that the nurses had not left the hospital, but he (Dr. Webster) would in answer merely refer to Dr. Stewart's former remark, that in St. Giles's parish, the majority of the cases were from a compass of some hundred yards. Proof, then, would require that they should have been under different circumstances from the other patients. He regarded those cases as the result of being exposed to the same local influence, to which the nurses might have rendered themselves susceptible by some excesses. When he was travelling in Italy, he had nearly got his death from the ague incurred by half an hour's sleep in a malarious district, whilst two of his companions, who incautiously slept the whole way, did die from the same disorder. Thus he considered that they had the severer attack from having been longer exposed to the peculiar local influences.

Dr. **Gregory** laughed at the divers arguments of the anti-contagionists. In relating a case of apparent contagion, if the patient were poor and ill fed, "Oh!" it was said, "that's entirely from starvation!" Give another instance where the goods of life were at command, and again they have you, "That's the effect of excesses!" They were very hard to please.

Mr. **Hunt** found it very difficult to answer the questions on the subject of contagion, every day put to him by shrewd non-medical friends, and he experienced still more difficulty in reconciling the anomalies continually presenting themselves in the history of this disease. For instance, one day the "reports" would state that this "contagious" epidemic had broken out in some thickly-populated town. Well, of course in the next accounts he expected to find it had spread in all directions. Instead of that, however, the report from that place would present a large round O, and the unpleasant visitor was discovered to have taken his departure to three or four miles distance. Again, as at Gateshead, when the points of contagion, if contagion there were, had become multiplied, from that very moment, instead of spreading, the disease began to decrease. He was happy to witness the manner in which the profession in France had acted and declared their opinions on this occasion.

Dr. **Wight**, although the period allotted for discussion had far advanced, would make a statement corroborative of Dr. Blicke's remarks. Two regiments in India were marching along the same road, but with an interval of a day between them. The first in advance was attacked by the cholera, and lost a great number of men, but the second, by a variation in the march of about two hundred yards, escaped with comparative immunity, losing only four, whilst of the other regiment about two hundred and fifty died. He should mention that both had, previous to the march, been a long time in the same station, and under similar circumstances. He had met with the account of a late discussion at a learned society in Edinburgh, where a veterinary surgeon stated that on those days when the cholera made the most numerous attacks upon human beings in that city, the largest proportion of the lower animals had suffered. The disease prevailed mostly, and to a great extent, among the graminivorous animals. He (Dr. Wight) disagreed with Dr. Blicke as to the predisposing effects of intoxication. No state could be more favourable for the inroad of the cholera than the languid condition succeeding a debauch in liquor.

Dr. **Fergusson** took notice of a remark which had fallen from Mr. Hunt:—that the mortality had not been increased in the places invaded by the cholera. He was astonished at such an assertion, and referred for an illustration of the contrary to an individual parish, St. George's, where it appeared that, comparing the returns of the last few years, there was a majority of 131 in favour of the cholera.

Mr. MARSHALL (the gentleman to whom in our last we gave the name of MARSDEN) said it had been asked where you would find twenty persons, hitherto free from the disease, exposed to small-pox contagion with impunity. A fact was called for, and he was happy to be enabled to offer one. When cruising off the coast of Scotland, one of the officers of his ship was seized with confluent small-pox. Not believing in the contagiousness of small-pox, he (Mr. M.) took no particular precautions, and yet not one of the crew was attacked. They were detained between the Orkney and Shetland Islands five days in a calm, and upon arriving at the former, they found that the same disorder had carried off several persons, who were then being buried. Now had they arrived a few days earlier, was it not probable that the whole onus would have been laid upon their vessel, which would have been said to be the instrument of contagion? There seemed to be some great error in supposing that the cholera attacked merely, or principally, those who suffered privation of food. What was the fact in the *Dover*? Out of fifty or sixty cases, the great majority were men of strong plethoric condition, married, and of temperate habits, in full employment, and consequently able to feed themselves well.

The speaker next went into the doctrine of contagion, and proceeded to harangue the members at great length on the all-importance of a most serious and strict inquiry into the facts, &c. &c., till at length

Dr. WILLIAMS rose and objected, observing, that in entering that Society, many years ago, he had other views than being lectured to upon the proper mode of investigating truth.

This had the desired effect; the oration soon terminated, and as the members were retiring,

Mr. KING went into a description of the morbid anatomy of cholera, concluding with his mode of treating the disease. He gave notice of a motion for the furtherance of the usefulness of the Society, by the establishment of a library, reading-room, &c., and an adjournment took place till that day fortnight, which will be the last day of meeting for the present season.

THE LANCET.

London, Saturday, April 21, 1832.

THERE are not wanting in the best-informed circles of our profession, men of acknowledged attainments, who are at a loss for an explanation of the circumstances which have retarded the elevation of medical rank in this country. It is admitted that medical practitioners, speaking technically, are learned individuals,—that they study zealously in early life, pursue their professional avocations with assiduity and ability to the close of their career,—that they render mighty and heart-endearing services to the community, and yet though deserving of the most transcendent renown and opulence for the advantages which they confer on the public, yet, we say, the medical profession, *as a body*, command but little of the respect and esteem of the people at large. As *individuals* they are admired and beloved within the immediate range of their labours, but what we complain of, in common with other authors whose pens have glanced at this subject, is, that the profession, as a body, when spoken of as such, carries not with it the weight and force of national character which belong to the profession of the law or to that of divinity. Whence arises this anomaly? for that it is one, and of a prominent character, will not be denied. From these causes :—First, the members of the medical profession are not a body of wealthy individuals; secondly, they do not hold high and commanding lucrative offices in the state; thirdly, England is cursed by *quacks*, between whom and the well-educated branches of the profession, the public have hitherto failed to mark out the just line of discrimination.

Though an evil of considerable magnitude, the last has infinitely less effect in depressing the importance of the medical character than either of the others. If the profession generally were a wealthy body, the stain produced by the conduct of the quacks, both regular and irregular, would be wholly transparent. Wealth is the idol which commands the process of genuflexion amongst all classes of the English community. Without it reputation for talent is localized and not diffused. Celebrity is confined to a particular sphere, but blend acknowledged skill with known wealth, and the echo of "consequence" is heard in every quarter. If the ability of the members of the medical profession could have been displayed in offices of high and paramount importance, as connected with the administration of the law and the dispensation of the affairs of the church, then would the science of medicine have stood upon its legitimate pedestals, and occupied incomparably above all others, the first rank in the scale of society. Wise governments would have laboured to this end. Instead, however, of studying to confer wealth and importance upon the chief labourers of the vineyard—instead of bestowing rewards upon individuals, who, with a view to benefit mankind, by seeking for gems of truth, have penetrated the caverns of the east, and plunged into the bosom of the ocean, emoluments beyond estimate, offices without number, and titles high and mighty, have been conferred upon the *concealers*, upon the *perverters* of truth—upon the administrators of the "law," which is nick-named "justice," and upon the preachers of the doctrines of a religion which they know not how to practise. We have Lord Chancellors and Vice Chancellors—we have Chief Justices with their twenty, fifteen, and ten thousand pounds a year; we have Archbishops and Bishops with their twenty and twenty-five thousand pounds a year—but in this enlightened community, in England in the nineteenth century, there is scarcely an office which yields to any medical practitioner—no matter how great his merits—which yields directly the paltry sum of 500*l*. per annum. In our colleges the principle of action for the *in*-direct payment is levelled against the public interests on the one hand, and *against* the attainment of professional talent on the other, for the reward to the "Examiners" is made to depend upon the number of claimants admitted, thereby offering a bonus for the introduction into the ranks of the profession of persons incompetent to an efficient discharge of its duties. Thus is it with our medical colleges and companies, the public, instead of being benefited, suffer from the establishment of such institutions; not entirely from an abuse of the trusts appointed by the law, but by the very execution of the mandates of the law itself. Again, glance at our hospitals, and what do we see? That the medical officers derive, comparatively speaking, no salaries directly from the funds of those institutions, but their real or pretended services to their sick are paid for by that portion of the public which comprises the relatives and friends of medical students. The pupils—in a word—are made to pay most dearly for the scanty and miserable attendance bestowed upon the unfortunate patients, the payers themselves deriving no commensurate advantage for the awards made by them. Such is the system, or, rather, such are parts of that pernicious custom which exercises a despotic rule, not only in our important national hospitals, but in all the minor medical and charitable institutions of the country. Physicians and surgeons are called upon to render services of inestimable value, while they are left like men of disreputable characters to derive payment from some indirect source. This is the treatment to which the whole of the profession in this country is subjected.

In the first place, the colleges are tempted by their charters to admit such a number of practitioners, that sufficient rewards cannot be afforded to them; and in the second place, the public, being incapable of discriminating between the two classes of practitioners,—the competent and incompetent,—resolve upon withholding remuneration from both. The effects of this baneful circumstance are exemplified daily. Merit being altogether disregarded, individuals of inferior attainments are allowed to fill the most responsible offices, whilst men of the most exalted attainments are left to pine in penury and want. Nor are such misfortunes limited to the establishments of this metropolis. The great provincial towns, and numberless of the villages, throw the same *blight* upon fair prospects—the same blur upon well-earned reputation. In the medical concerns of a parish, it frequently happens that he who is cleverest in *intrigue* obtains the office of "*parish surgeon*," to the exclusion of him who is by far the cleverest in his *profession*. At the same time the administrators of parochial affairs entirely overlook the enlarged understanding and enlightened and generous feelings—of some small value to the sick poor—which are created by the continued occupation of scientific research. These are trifles, and weigh not in the scale of importance with those consequential personages who are required to fulfil towards the poor the conditions of the 43d of Elizabeth. After all, the *pay* awarded to the parish surgeon, whether skilful or not, generally amounts, in our rural parishes, to a less sum than is given by a farmer to each of his carters or ploughmen. In several of the counties where parishes, in their long diameters, extend not less than from five to seven miles, the stipend does not exceed as many pounds sterling. In other instances, where some points of the parish are at the distance of at least *ten miles* from the practitioner, he does not obtain *ten pounds* for his services! "Oh! but the *introduction* to the better class of patients!" This is the cry eternally set up by the intriguers. It is really difficult to learn of what materials the minds of these persons are constituted. They delight in everything that is crooked or oblique; their abhorrence being inveterate against straightforward, open, ingenuous conduct. They would reward a *cunning* thief for the ingenuity of his devices, and at the same time visit upon a *courageous* one their deepest imprecations for his boldness; in short, they would hang a highwayman, not because he had robbed, but because he had assailed his victim *in front*.

In considering the lack of wealth amongst the profession as a body, and the consequent loss of that *peculiar* importance which the people of England attribute to large pecuniary possessions—are we, we say, in investigating the causes, to withhold blame from practitioners themselves? It were an error to assert that their conduct has been faultless. Can it be contended that the physicians and surgeons of our hospitals do not lower themselves in the estimation of the pupils and the public, in consequence of deriving their fees from *indirect* sources? Is it just, is it honourable, to make students pay heavy sums of money for the privilege of merely walking through the wards of public institutions? Is it correct, is it just, is it decent, that the *pupils* should be made to pay for attendance upon the sick, when the *chartered* funds are infinitely beyond the possibility of suffering from their appropriation to such legitimate purposes? Or does it partake of the character of that Christian, charitable, feeling, which, it is to be hoped, led to the foundation of such institutions, to make a demand of twenty or thirty guineas from each young gentleman who claims to be admitted to the bedside of the sick? Thus determining by practice, if not in words, that the collateral advantages to be derived from the emanations of charity cannot be obtained without *purchase*, and that they shall not be bestowed without an equivalent in the shape of a pecuniary sacrifice. Such conduct on the part of the governors and officers of our hospitals is utterly indefensible, and the perseverance with which it is continued exhibits a callousness of feeling—with regard not only to the interests of pupils, and the welfare of patients, but to public opinion—which betokens anything rather than elevation of sentiment, nobleness of disposition, or the habit of thinking correctly.

What then is to be said of those persons who, while they know better, and are not unconscious of the miseries thus produced, attempt, either by writing or speaking, to perpetuate a state of things so prejudicial to professional character, and so baneful to the public interests? We ask this question; and the foregoing remarks have been partly elicited by a malicious and infamous calumny, which has been printed in the last issued of the four quarters of the old "*Medical Repository.*" Our readers cannot have forgotten the MAUSOLEUM; how the *sextons* were changed almost weekly, and how the poor old dingy receptacle for what was *not* human was opened monthly! After several "final closures," and suffering all sorts of defacement—after the destruction of its pillars of *Underwood*, after every other species of injury, except the last, and *that* by far the worst, because marked both by dishonesty and incapacity—it solicits patronage (with the bare alteration of the word "Physical" into "Surgical") under the title of the "London Medical and Physical Journal," *also* a "Mausoleum," the gateway of which, it will be recollected, was for some time very imperfectly guarded by Roderick—alas, poor Roderick!—by Roderick Macleod, of "five-pound-damage-and-general-laugh notoriety." Well, but we must not go back to these olden times—those halcyon days of exposed and discomfited opponents, otherwise we shall forget the present, and also the atrocious calumny[*] we are about to extract,—which would be a sin indeed.

The libel stands thus. We copy the passages *verbatim.*

"VALID REASONS FOR THE CONTAGIOUSNESS OF CHOLERA IN GREAT BRITAIN.

"In a Parliamentary document just published, we find the *monthly* amount of the expenses of the Central Board of Health to be as follows:—Lieutenant Marshall has 33*l.* a month; Sir W. Russell, M.D. 60*l.*; Sir D. Barry, 42*l.*; Messrs. Macdonald, 33*l.*; and W. Maclean, 20*l.*; two clerks, a messenger, and a porter to the Board, 28*l.*; four deputy inspectors of hospitals, 89*l.* per month; four staff and regimental surgeons, 87*l.*; two assistant surgeons, 36*l.*; three naval surgeons, 63*l.*; one assistant-surgeon in charge of a district, 18*l.*; three assistant-surgeons, 41*l.*; deputy-inspector of hospitals at Glasgow, 31*l.*; two assistant-surgeons there, 27*l.*; assistant-surgeon at Newcastle, 13*l.*; assistant-surgeon at Hetton, 27*l.*; total monthly, 644*l.*

"This document offers ample evidence of the real motives which have influenced the Central Board of Health. After its perusal, no one can mistake the reason which led to the promulgation of the terrific proclamations on the contagiousness of the cholera."

There! We did not believe that such a traitor to the cause of professional respectability existed in its ranks, as the writer of this black, foul-tongued, cowardly insinuation. The "*real* motives" are to be found in the pay awarded to *thirty-two* individuals—amounting to about six hundred pounds a month—the whole not equalling the emoluments of *one* of Ellenborough's sons, as a clerk in the Court of King's Bench! Of course we know not the parent of this monstrous birth; but, according to his own acknowledgment, the existing chief grave-digger is one Michael Ryan. Now, Michael, we require that you come forward in your own proper person, and acknowledge that this vile slander was vomited forth by some lying, intriguing, deceitful, cozening, envious knave, and that it was published by thy kindred "association of physicians and surgeons"—(an "association," judging from appearances, about upon a par in point of talent with the "Umbrella Company") without thy privity and concurrence, or prepare for a smothering in thy own filthy "repository."

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[*] E— is thanked very cordially for sending us this paper. Had he not done so it would have escaped our notice, as, being already sufficiently lethargic, we have no appetite for a weekly dose of "Medical and Surgical" dulness.

At all events, concerning Michael's future fate, we strongly suspect that, if he cannot in the most unqualified manner disclaim this libel, the whole profession will disclaim *him*, as being a man who would not scruple to blast the reputation of the entire medical character of the country, or impute the most unworthy motives to his brother practitioners, if by so doing he could gratify his own feelings, or serve his own ends. So pray look to it, Michael Ryan; the injunction is offered in no unfriendly voice!

NON-MEDICAL CORONERS.

These gentry have fully sustained their reputation during the prevailing epidemic. In an inquest held on the body of Elizabeth Chalk, of New Street, Cloth Fair, who had died from *cholera*, the jury, under the direction of the Coroner, returned a verdict of *manslaughter*! The testimony of Dr. Waller, who attended the patient, that of Mr. Kiernan, who examined the body, and that of two other medical gentlemen, could make no impression against the evidence of three children, one of whom stated that she was knocked over an iron railing; the second, that she was pushed against it; and the third, that she was "shoved about." The Coroner, in his wisdom, refused to allow the medical gentlemen who examined the body, to give their evidence, alleging, that it was the duty of himself and jury to inquire whether the death of the deceased was caused by *violence*;—which, of course, he was capable of deciding, without making any scrutiny as to the condition of the internal parts of the body! The anomalies and abuses connected with these non-medical coronerships are, we fear, to be swept away only in the current of general Reform.

Mr. Youatt's Lecture, in the present Number of The Lancet, on the Epidemic and Malignant Catarrh of Horses, is pre-eminently entitled to attention. We may probably be excused for alluding in this place to the very valuable information which is contained in these Lectures generally.

DISINFECTION BY HEAT.

LETTER FROM DR. HENRY IN REPLY TO THE STATEMENTS OF MR. WALLACE.

To the Editor of The Lancet.

Sir,—Having long ceased to be a reader of periodical works on medicine, the letter of Mr. Wallace, in the 446th number of your Journal, might have entirely escaped my notice, had it not been sent to me a few days ago by a friend.

Until I read that letter, I was totally ignorant that any person but myself had ever directed his attention to the disinfecting powers of high temperatures. This distinct and unqualified declaration will at once satisfy all who know me. To those who do not I offer the following explanation.

The occasion which first induced me to undertake an inquiry into the disinfecting powers of heat arose in the summer of 1824, out of a pressing emergency. Cargoes of raw Egyptian cotton, of the value of upwards of twenty thousand pounds, had been long detained under quarantine at Milford Haven, to the great injury of the owners; by whom I was earnestly requested to suggest some method of shortening the time of expurgation. A few days only were allowed me for considering the subject, and performing the experiments, the results of which led me to propose the application to the raw material of a *dry heat*, obtained, for security to the goods, by means of steam. This proposal was immediately communicated to the owners; but the early liberation of the cargoes rendered its adoption unnecessary. It was, at a subsequent period, laid before the late Right Hon. Mr. Huskisson, by a most respectable deputation of Liverpool merchants; and every facility for trying it was promised by that gentleman, if any similar exigency should in future arise.

After the lapse of seven years, I was induced, last August, to resume and extend the inquiry, by the threatened approach of Asiatic cholera to our shores. Led by a train of reasoning on the corpuscular arrangement of the elements of animal compounds, I made the experiments on the destructibility of the contagions of cow-pox, typhus, and scarlatina, which are related in the Philosophical Magazine for January last. That I might not, however, incur the risk of trespassing on ground already, without my knowledge, occupied by any one else, I inquired of several professional friends, whether their reading furnished any instance of similar views. Among others, I consulted Dr. Traill (president of the Philosophical Society, and physician to the Infirmary at Liverpool), as one distinguished not only by his extensive acquisitions in science, but in medical literature. After carefully searching the volumes most likely to afford such information, he expressed his belief that my views and experiments were entirely original. This, too, is the uniform tenour of the opinions (now in my possession) of other physicians of acknowledged learning.

My earliest views respecting disinfection (it must be kept in mind) were entertained by, and the proposal arising out of them suggested to, the owners of the cotton, in August 1824. Early in 1825, Mr. Wallace, it appears, published a tract entitled, "An Account of the Apparatuses for the Treatment of Rheumatism and Diseases of the Skin which have been constructed at the Dublin Skin Infirmary." A copy of this, it seems, was sent by the author to the library of the Manchester Infirmary, in February 1825. But having retired many years before that period from the office of physician to that institution, and having long relinquished medical practice and medical studies, I never heard of Mr. Wallace's book, or of its contents, till I read his letter a few days ago. Having now examined the copy alluded to, I find that the whole of what it contains respecting disinfection by heat, is the paragraph of fourteen lines copied into The Lancet (p. 855); but that not a single line is to be found in it respecting the experiments on the destructibility by heat of the poisons of cow-pox, small-pox, and syphilis, to which Mr. Wallace lays claim at p. 856. To any other source of information respecting his views I never had, nor could have had, access. He cannot expect that I should be acquainted with what he may have taught in lectures, or communicated through channels not open to the public at large.

Yet, on these slender grounds has Mr. Wallace, an entire stranger to me, founded an insinuation, too plain to be misunderstood by any one, that I have appropriated to myself information taken without acknowledgment from him. His right to claim whatever he may think belongs to him I do not dispute; but he is not justifiable in imputing to me an offence, degrading (if there were the slightest foundation for the charge) alike to my moral and scientific character; and in doing so, he has himself offended against the rules of candour and of justice.

I am, Sir,
Your obedient servant,
William Henry.

Manchester, April 5, 1832.

ST. BARTHOLOMEW'S HOSPITAL.

CONCUSSION OF THE SPINAL CHORD.

William Rodman, admitted into Henry's ward, under the care of Mr. Lawrence, March 22d.

The patient, who is a man of spare habit, 35 years of age, stated, that the day before his admission, whilst coming down stairs, he suddenly slipped, and fell on his back, descending two or three steps. This accident was, at the moment, succeeded by some little pain in the loins; which, however, being subject to rheumatic affections, he did not altogether attribute to the blow received. The usual avocations were therefore proceeded with during the remainder of that day; but, on the following, violent pains supervened, together with inability to move either his arms or legs; and he then procured admission to the hospital. Upon examination, no fracture, displacement, or external injury about the spine, could be detected; and the patient was unable to refer to any particular spot as the situation of the blow, at the same time complaining of pain generally in the lumbar region. He also felt a great sense of uneasiness about the chest, and said that there was a "tingling" in his fingers. Not the least pain in the head. There was complete paralysis of the lower, and partial paralysis of the upper, extremities. No motion or sensation in the legs or thighs; could lift the arm, but the will had very slight influence over the muscular power of the hand. Involuntary discharge of the urine and fæces. Tongue whitish; pulse small; depression and anxiety alternately depicted on the countenance. There are, on the right leg, two circular excavated sores, of six months' duration, to which a linseed poultice was applied. *Mist. Sen. Comp. p. r. n.*

March 26.—Mr. Lawrence thought i

curious that the nerves supplying the *upper* extremities should be affected, as they had their origin above the part to which the patient referred the principal pain. The pressure which the man is capable of exerting with his fingers is hardly perceptible. He cannot feel a pinch in the thigh. Bowels open; tongue and pulse much the same. Ten ounces of blood to be extracted from the loins by cupping.

28th.—Thinks himself better. Deficiency of sensation continues: power of motion in the arm improved. The counter-irritant plan to be adopted, by the application of two moxæ on each side of the spine, at the immediate seat of pain.

April 2d. Much in the same state, but has regained command over the bladder and rectum.

6. Moxæ repeated. Sensation restored in the upper and lower extremity, except in the feet.

7. Eighteen leeches to the painful part. *Mist. salin. efferves.* every four hours. Nine p. m., Take ten grains of Dover's powder. Repeat twenty-four leeches.

8. Is better. The draught to be repeated, with twenty drops of antimonial wine every four hours. Nine p. m. *Hydrarg. submur.* gr. iv.; *Pulv. Jalapæ* gr. xii, to be taken immediately.

9. In a very depressed state; pulse uncommonly small.

10. Ordered *arrow-root*, and *brandy* ℥j daily; *tincture of opium* ♏ xv immediately.

11. On inquiry this morning, we find that the patient died after a severe struggle during the night, and the body was almost immediately removed, the friends having been violently opposed to a post-mortem inspection. This we must say is deeply to be regretted, as an examination, which doubtless would have exhibited an extravasation sufficient to affect both columns of the spinal chord, would have made this case, already exceedingly instructive, one of very great practical importance. Is it not most extraordinary, that the depletory measures over the spine were not commenced until the 26th, the man having been admitted into the hospital on the 22d? Is it not really astonishing, that the *only* remedy employed up to that period consisted of such an inefficient milk-and-water agent as the compound senna mixture? The application of the moxa induces us to imagine that that portion of disease which excited *any* attention at all was entirely misunderstood.

The mischief, we believe, arose not from inflammation, but extravasation.

SYPHILITIC DISEASE—FUMIGATION.

C. S——, admitted March 25th, a girl of about 20 years of age, contracted lues venerea two years ago, and its effects are now manifested by ulceration, which has made extensive havoc in the posterior part of the pharynx, and likewise by a large ulcerated aperture in the velum palati.

16. Put upon light diet, and ordered *pil. saponis c. opio*, gr. v, *omni nocte sum. Ext. conii*, gr. v, *mane et meridie. Ess. sarsæ*, ℥ss, *ter quotidie.*

On the 21st Mr. Lawrence prescribed mercurial *fumigation* twice daily, by which her mouth became speedily affected, a free salivation being produced. Upon this Mr. Lawrence observed (April 2nd), that here was a case exhibiting what had frequently happened of late in the hospital practice, namely, the occurrence of complete salivation after the fumigation of a sore. This was the sixth or seventh instance he had lately met with, in which the same circumstance was remarked. "And we find that the sores are healed, and the patients get well. Perhaps, indeed, had I thought that the fumigating plan would have produced salivation, I might not have been inclined to adopt such a method. However, the good has been accomplished, and so much the better. These instances show us that the old line of practice in such cases is not to be treated with disregard. It seems that, after all, there's nothing like mercury for them, and to this extent."

The patient went on well, and has now left the hospital.

SUGGESTIONS FOR RAISING THE TEMPERATURE OF THE BODY IN CASES OF

MALIGNANT CHOLERA.

To the Editor of THE LANCET.

SIR,—I beg leave, with all due respect to the medical body, to suggest that cholera patients, instead of being enveloped in flannel, should have the skin of a sheep (recently slaughtered as near as possible to the dwelling of the patient) applied with its flesh side reeking with animal heat to the bare skin and entire surface of the human body.

So powerful a *sudorific* is this application, on horses, that my experience enables me to state that it stands next on the list of our sudorifics to a race-horse's sweat in training for a sweepstakes. It appears to act—1st. By imparting its own animal heat. 2ndly. By its weight and close adaptation to the convexities and concavities to which it is applied. 3rdly. By an adhesive quality, or sort of contractile power. 4thly. As a powerful counter irritant, and it will occasionally slightly vesicate and loosen the hair.

These are some of its more palpable effects when applied to horses; but it possesses other merits which I fear I shall not be able adequately to describe, from not clearly understanding the *modus operandi.* I allude to a practice which I have adopted for many years, of applying hot sheep-skins (one or two at a time) secured by horse-clothes on the bodies of exhausted or tired *hunters* which have been left in the field with little signs of life remaining, from the severity of the chase. I mean, having so far lost the power of locomotion as to suffer themselves to be dragged to the nearest stable upon a gate, when, by the following morning, they have been able to walk round a loose box without support.

Reasoning from the well-known fact that the blood of hunted animals which die in the chase does not coagulate, and that their muscles do not become rigid after death, I am inclined to think that the excessive muscular exertion not only exhausts and dissipates the nervous fluid from the muscular fibre, but that this electric or subtle agent is also wholly abstracted from the blood itself, before death ensues. Therefore, in the recovery of horses by the aid of sheep-skins, in whom there has appeared to be almost a total abstraction of the nervous influence, both from solids and fluids, I am inclined to ascribe higher merit to the skin of a recently-killed animal of large size,—that of its being a mere article of warm clothing. To be brief, I have no hesitation in making this bold assertion, that I firmly believe, if the hide of any large animal be instantly transferred from the slaughtered carcass to the back, neck, and loins, of a tired or exhausted horse, the living skin will imbibe from the skin of the flayed animal a *vital principle,* which will be transfused through its pores. I am, Sir, your most obedient and obliged servant,

James Turner.

311, Regent Street, April 6, 1832.

REJOINDER OF "INVESTIGATOR" TO MR. HOOPER.

To the Editor of The Lancet.

Sir,—Some important surgical facts are connected with the subject of Mr. Hooper's letter to you of last week.

Mr. Hooper appears to think that my letter was intended as a malicious attack upon him. Sir, I had not, before I saw the report of the inquest, the slighest idea who was the gentleman there mentioned. A reference to the report will acquit me of any wish to slander Mr. Hooper gratuitously.

It appears by Mr. Hooper's letter, an important part of the evidence was grossly misstated. Instead, therefore, of a "shuffling" answer to the coroner's question, Mr. H. gave a straightforward one. But it does not appear to me by any means a satisfactory one; for when a bladder becomes enormously distended with urine, so far from its power to contract being increased, that power is almost entirely destroyed. But, Sir, how could the bladder be ruptured by the fall, and yet four days afterwards contain a *gallon of urine?* This leaves the case in greater obscurity than it was at first.

How, then, is the difficulty to be explained? If Mr. Hooper pursued his plan of allowing so large a quantity of urine to remain in the bladder, in order to stimulate it to contract, might it not have ruptured from over-distension instead of the fall? I know, Sir, that this is an extremely rare occurrence; but it is better than no reason at all. Mr. Hooper has declined saying anything more on the subject: here I think he has acted unwisely.

I am, Sir, yours, &c.,

Investigator.

March 22d, 1832.

⁂ We have shortened this letter. "Investigator" must remember that he writes anonymously.

COMBINATION OF OPIUM AND ACETATE OF LEAD.

To the Editor of The Lancet.

Sir,—Dr. Addison, in one of his lectures a short time since on Materia Medica, when speaking of the acetate or superacetate of lead, informed his class that he was in the habit of using it in cases of hæmorrhage from the lungs, and that it was one of the best remedies he had tried in that complaint. He gives it in combination with opium, and says that these two medicines are frequently used at Guy's Hospital, made into pills, composed of one grain of the superacetate of lead and half a grain of opium; and that this plan is attended with almost invariable success. Without doubting the latter part of this statement, the following questions naturally arise to the mind of any one who is made acquainted with the doctor's views:—

1st. Is not the combination of opium and superacetate of lead incompatible, and is it not impossible to obtain any mixture of these two substances, without its immediately, on the addition of water, undergoing decomposition?

2nd. Has Dr. A. ever heard or read, that the morphia and meconic acid contained in opium, are supposed to exist in the state of a meconate of morphia?

Does Dr. A. know, that superacetate of

lead and meconate of morphia are classed amongst the incompatible salts, and that consequently, on adding them together, a double decomposition must take place, attended by the formation of meconate of lead, and acetate of morphia?

4th. Can Dr. A. positively say, that the meconate of lead (which is an insoluble salt) is of any use in the remedy proposed? May we not also ascribe the good effects to the acetate of morphia? Perhaps Dr. A. may have tried the acetate of morphia alone?

5th. Would not any pupil incur the risk of being rejected by a competent court of medical examiners, were he to offer such a prescription to their notice as that proposed by Dr. Addison? Dr. A. indeed frequently says, that he has "nothing to do with chemistry," and that "were he asked a chemical question, he should, as likely as not, be unable to answer it." Yet the Dr. is a junior physician, and lecturer on Materia Medica, and the Practice of Physic, in Guy's Hospital.

I am, Sir, your obedient Servant,
GUYENSIS.
March, 1832.

EVIDENCE OF MEDICAL MEN IN CASES OF ALLEGED MALA PRAXIS.

To the Editor of THE LANCET.

SIR,—Since you were so good as to admit my letter last week on the subject of Dr. Blicke's trial, I am induced to trouble you once more on the subject, as I hesitated to make that letter a long one. I wish to add another reason to the one I then gave for the employment of professional caution before entering courts of law to give evidence against medical men.

Actions for alleged mala praxis invariably require the attendance of many medical men in court, and that attendance is on almost all occasions a very long and tedious one. Now for this detention how rarely can the defendant be expected to pay, and yet how valuable is the time which it occupies! There were in court on Dr. Blicke's trial, Dr. Titley, Mr. Samuel Cooper, Mr. Brodie, Mr. Key, Mr. Earle, Mr. Tyrrell, the apostate Lawrence, and many others,—and that not for an hour or so, but the whole day. Take one instance—that of Mr. Lawrence. From ten in the morning till past seven in the evening was he there, a subpœnaed witness I presume, wasting nine good hours—*for nothing*. For he was not put into the box till after six P.M., and when there, Mr. Pollock, looking about weariedly and abstractedly, said, "Really I have nothing to ask Mr. Lawrence now. Mr. Lawrence, you may go, Sir." How deeply mortifying must this have been to any man who had been thus detained! But the dismissal seemed ungracious, so Mr. Lawrence was called back again after being half a minute gone. "Mr. Lawrence, you have heard the evidence of Dr. Titley. Do you think the eversion of the leg occurred after the six weeks, or before?" "Why, I should certainly say after." "That is all, Sir," said the counsel.

I dwell not upon the wanton encroachment on the time of medical men which witnesses for plaintiffs may thus occasion for want of the fair and reasonable previous inquiry which I have suggested. It needs no further exposition. I am, Sir, your obedient servant,

March. L. S. E.

LONDON HOSPITAL.

To the Editor of THE LANCET.

SIR,—Whoever furnished you with the cases published in THE LANCET of Saturday, March 31, overlooked the state of the hydrocele on the 25th ult. (the last date in the report), 15 days later than the assertion, that the operation was unsuccessful. How often has he or you, Mr. Editor, seen or heard of a hydrocele cured in five days! Even in the few opportunities that I have had of witnessing the cure of hydrocele, the parts have generally reswelled after the injection, on account of the inflammation causing a temporary effusion. As to the case in question, the fact is, that the patient was discharged on Monday last perfectly cured, without any farther steps being taken.

Permit me to set a lecture pupil right. In his just complaint respecting the library. He ought to have mentioned the two *senior*, not the two *principal* surgeons. This is the only objection I have to make to his statement. It is well known that it has been the wish of Mr. Scott, and of all the assistants, to adopt a liberal and more efficient regulation of the library, to which the surgeons lay claim as exclusive property, though it has accumulated by subscriptions from the pupils.*—I have the honour to be,

Your obedient Servant,
A LONDON HOSPITAL PUPIL.
April 11, 1832.

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* The remaining portions of our Correspondent's letter should have been authenticated, a circumstance which he will not perhaps overlook in any future communication.

CORRESPONDENTS, &c.

The letter of *Mr. W. B. Marshall* was received too late for us to give even an extract from it. It shall be inserted next week.

Mr. Gutteridge's communication is an advertisement, and can only appear as such on the wrapper, or stitched with the work.

THE LANCET.

Vol. II.] LONDON, SATURDAY, APRIL 28. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXVII.

EPIDEMIC CATARRH, AND MALIGNANT CATARRHAL EPIDEMIC IN CATTLE—BRONCHITIS IN HORSES AND CATTLE—THE PECULIAR BRONCHITIS IN YOUNG CATTLE ACCOMPANIED BY WORMS IN THE BRONCHIAL PASSAGES.

Epidemic catarrh is a more frequent and more destructive disease in cattle than in the horse. In a cold yet variable spring, succeeding to a wet and mild winter, there will sometimes be scarcely a dairy or a stall-yard in which a considerable number of the cows do not labour under the most distressing hoose, and in which few are altogether without cough. This, however, is only the early or most prevalent symptom. Costiveness is observed, on which your castor-oil or Epsom salts, although given in repeated doses, will make no impression, and then, all at once, diarrhœa will come on, which bids equal defiance to your remedial measures. In many cases diarrhœa is present, and obstinate, from the beginning. Tumours about the joints, the neck, the roots of the ears, the head generally, the back and the loins, soon succeed; at first apparently caused by the extrication of gas in the cellular membrane, and recognised by a singular crackling sound when pressed upon. It would appear as if the process of decomposition was going forward during the life of the animal. No sooner do these tumours appear than the fæcal discharge is offensive in a high degree; the breath loses its peculiar and beautiful scent; the vital powers are rapidly exhausting; the powers of the organic system first, and soon afterwards of the locomotive, and the animal system generally. The beast is unwilling to move; it can scarcely move—it staggers as it walks. The loss of flesh can now be traced every day—the coat stares, and clings to the carcase, the appetite has been long since gone, a fetid discharge commences from the mouth and nostrils, and the end is not far off.

Treatment.—Here, even more than in the horse, it is necessary that your measures should be prompt and decisive. These singular cracklings and tumours indicate a peculiarity about these animals which you should never forget. The vascular system is more than usually developed and powerful. Cattle were designed to yield us much nutriment even while living, and plenty of good muscle and fat after death. In proportion to the extra-work thus required from the vessels most concerned with inflammation, is the tendency to inflammatory fever of every description, and to the speedy exhaustion of vital organic power.

Venesection.—Bleeding will always be proper at the commencement of epidemic catarrh in cattle, regulated by the intensity of inflammation, or the apparent approach of debility, and by the effect produced while the blood continues to flow. An ox, in the early stage of inflammatory disease, will bear to lose a much greater quantity of blood than the horse; but the disease having advanced, the injury produced by too copious depletion is proportionably greater.

Aperients.—Physic should immediately be administered. The sulphate of magnesia is the preferable aperient; common salt may be given, but sulphur is hardly powerful enough in the early stage. Some practitioners, however, give equal quantities of the sulphate of magnesia and sulphur, and they imagine with safer and surer effect. The usual dose of the Epsom salts would be a pound, followed up if necessary by half-pound doses at intervals of six hours, and assisted by injections.

Probably from the peculiar structure of the stomachs of cattle, and the expansion of *cuticular* surface, and the comparatively small supply of blood-vessels and nerves, to the stomachs, and even the intestines, on account of the previous and complete mechanical preparation of the food for digestion, there is not the powerful sympathy which exists in the horse between the thoracic and abdominal viscera, and therefore in the early stages of this disease, and although there may be evident chest affection, the mild aperients which we use for cattle may be administered with perfect safety. To aloes, however, I should object. This drug is uncertain in its operation on cattle—it purges only in large doses, and then it irritates and disposes to inflammation.

Medicine and General Treatment.—The sedative medicines recommended for the horse should be administered to cattle, but only in half the quantity, and always in the form of a drink, for then alone they will pass immediately into the fourth or true stomach. In the treatment of cattle, even more than in that of the horse, attention to comfort is required. A warm, but not close or ill-ventilated cow-house is indispensable. The powerful circulatory current of this animal should be as equally diffused as possible. The cracklings and tumours of which I have spoken indicate the necessity of keeping the surface of the body warm, and as you will find that the inflammatory stage quickly passes, and typhus under a more or less malignant form succeeds, I will allow you to do that which I was fearful of doing in the horse, to resort to other means to support the strength, and restore the tone of the system, than merely to arrest the progress of the fever. The different destination of the horse and of cattle, and the different character assumed by their diseases, require a corresponding difference of treatment, and aromatics and vegetable tonics may in small doses be sooner combined with the febrifuge medicine, or supersede it. *Vegetable* tonics, however, must alone be resorted to, which, while they give vigour to the digestive system, and remove the debility consequent on extensive inflammation of the mucous surfaces, have little effect in increasing the force of the circulation. The practice of every scientific veterinarian, who has studied the diseases of cattle, has confirmed this distinction. Ginger, caraways, gentian, and colombo, are the recognised aromatics and tonics of the cattle pharmacopœia.

With these exceptions, the directions given for the treatment of epidemic catarrh in horses is applicable to the same disease in cattle.

THE MALIGNANT CATARRHAL EPIDEMIC.

While epidemic catarrh oftener assumes a malignant form in cattle than in horses, that disease which is malignant from its very commencement is also more common. There are very few years in which it is not endemic. It principally appears in some districts, marshy and woody, or where underdraining has been neglected, or some farms where the cattle have been exposed and half starved; but it has not for many a year spread so widely or been so destructive as in former times. In the records of it which have been handed down to us, and in our present experience of it, it differs materially in its symptoms and its treatment, according to the degree in which it is primarily an affection of the respiratory or digestive passages, or coming under the description of malignant typhus, without any particular local affection. We have to do to-night with that alone which is originally connected with the respiratory passages.

Symptoms.—The cough is frequent and painful; the flanks heave; the pulse is small, hard, frequent, and sometimes irregular; the mouth hot; the root of the horn cold; the fæces oftenest hard and black, but at other times liquid, black, and fetid. In a few days that symptom of almost every febrile affection of cattle decidedly appears—extreme tenderness along the spine and over the loins, so that the beast will shrink at the slightest touch; the cough is more frequent and convulsive, and a brown or bloody matter runs from the nostrils and the mouth, while the animal holds his head extended in order to be able to breathe; the eyes are swelled and weeping; the patient grinds his teeth, and there is frequent spasmodic contraction about the nostrils; the animal rarely lies down, or if he does, almost immediately rises again. But the eyes soon begin to become dim; the pulse is smaller and feeble; the respiration quicker; the flanks tucked up; the tenderness on the loins subsides, and the fæces are liquid and black. The animal moans and lows, and grinds his teeth almost incessantly; the head is agitated by a convulsive motion; blood begins to mingle with the fæces; there is weakness and staggering; offensive breath, nauseous perspiration. Tumours and boils now break out in various parts; if they break, and there is considerable suppuration, the animal usually recovers; if they decrease or disappear, he as certainly dies; yet if they do break, sloughing comes on, extensive and fetid, beyond that which could be thought possible.

Treatment.—Bleeding should be resorted to in the very early stage of the complaint, but not when debility had begun to appear. Aperient medicine should be administered if the bowels were constipated; febrifuge medicines either alone, or combined with vegetable tonics; small doses of the chloride of lime internally administered; plenty of it applied on the animal and about it, and the most rigorous separation of the infected from the sound.

In other Animals.—Sheep possess comparative immunity from this malignant epidemic. The rot carries off enough of them without any other scourge. Dogs have no disease of this character, except when distemper assumes a putrid type; and swine have rarely any disease that can properly be said to be malignant.

BRONCHITIS.

We will pursue our course down the trachea to its bifurcation and termination in the bronchial passages, and there we often meet with inflammation either connected with, or the consequence of, other affections, or principally there, or almost strictly confined to the bronchial passages. We designate this inflammation by the name of *bronchitis.* It is a disease scarcely yet recognised among us, but often coming before us, although confounded with pneumonia, and requiring some peculiarity of treatment.

Use of Auscultation in distinguishing the various Diseases of the Lungs.—Bronchitis is not generally a primary disease. The inflammation of the superior respiratory passages, constituting catarrh, gradually creeps and involves the larynx and the trachea, and at length the farthest and the minutest ramifications of the air-tubes. When you find that it is thus advancing, you will eagerly watch its progress by the assistance of immediate auscultation. Recollect the bellows-like healthy sound of the trachea; that of the healthy lung resembles a deep and distant murmur. The very word expresses the sound. But the diseased lung, how shall you ascertain the seat, the degree, the consequence of inflammation? Careful attention to a very few cases will make you perfect masters of this. The distant murmur of the healthy lung you cannot mistake; nor the crackling crepitating sound of pneumonia, inflammation of the substance of the lungs, or of the membrane lining the cells, or compartments or divisions of the minute *lobuli.* You hear the blood filtering or breaking through these delicate membranes, and accounting for that congestion, that filling of the cells with blood which is found after intense inflammation. The bronchiæ are lined by a mucous membrane, and, under inflammation, the secretion of mucus is increased, and the tubes are partially obstructed. The ear, habituated to the exploration of the chest, will detect this with as much certainty as if the membrane had been fairly exposed to view, by the whizzing sound, as if the air were forcing itself through a succession of bubbles. Inflammation precedes this increased discharge of mucus: even that may be detected. The inflamed membrane is thickened, tense; it assumes an almost cartilaginous structure, and the murmur is not only louder, but has a kind of *snoring* sound. Some have imagined that a metallic ring is mingled with it. I have fancied something of the kind, but cannot say that it was distinct and convincing.

Although we cannot yet carry this so far as the practitioner of human medicine does, we can derive most important advantage from the use of auscultation. We can at least ascertain the seat of inflammation: and the presence of the morbid sound, or the gradual recurrence of the healthy one, will point out the extent of the disease; while other indications, and obtained in the same way, will as faithfully tell us the mischief and disorganization occasioned by the inflammation. Once more, Gentlemen, let me urge you carefully to study the indications presented by immediate auscultation. They will rarely deceive you, and you will not have long accustomed yourselves to this mode of exploration without highly prizing it.

Symptoms of Bronchitis.—The interrupted whizzing sound has clearly indicated a case of bronchitis. There are many corroborative symptoms which you should store in your memories. *The variable temperature* of the extremities will be an important guide,—not deathy cold as in pneumonia, nor of increased temperature as often in catarrh, but with a tendency to coldness, yet not always the same. The *pulse* will assist your diagnosis,—more rapid than in catarrh, much more so than in the early stage of pneumonia, not so hard as in pleurisy, more so than in catarrh, and much more so than in pneumonia. The *respiration* will next be examined, abundantly more rapid than in catarrh, pneumonia, or pleurisy, *generally as rapid and often more so than the pulse,* and accompanied by a wheezing sound, heard at some distance. Mr. Percivall relates a case in which the respiration was more than one hundred in a minute. Mr. C. Percivall relates an interesting case in which the respiration was quick in the extreme; and he remarks, that he does "not remember to have seen a horse with his respiration so disturbed."

In addition to these clearly characteristic symptoms, you will observe a *haggard countenance,* to which the anxious look of the horse labouring under inflammation of the lungs cannot for a moment be compared, and an evident *dread of suffocation,* expressed, not by inability to move as in pneumonia, but frequently an obstinate refusal to move; *cough painful* in the extreme; *breath hot,* yet *no marked pain* in the part, no looking at the side or flanks.

As the disease proceeds, there will be considerable discharge from the nostrils, much more than in catarrh, because greater extent of membrane is affected. It will be muco-purulent at first, but will soon become amber-coloured or green, or greyish green, not from any portion of the food being returned, but from the peculiar hue of the secretion from ulcers of the bronchial passages. Small organized pieces will mingle with the discharge,—portions of mucus condensed and hardened, and forced from the inside of the tube. If the disease proceeds the discharge becomes bloody, and then, and sometimes earlier, it is fetid.

Progress.—The natural termination of this disease if unchecked is in pneumonia. Although we cannot trace the air-tubes to their termination, the inflammation will penetrate into the lobuli, and affect the membranes of the air-cells or divisions which they contain. There is metastasis of inflammation oftener here than in pure pneumonia, and the disease is most frequently transferred to the feet. If, however, there is neither pneumonia nor metastasis of inflammation, and the disease pursues its course, the animal dies from suffocation. If the air-passages are clogged, there can be no supply of arterialized blood.

Frequently a false membrane, and organized to a considerable degree, lines the bronchial passage, under which there exists inflammation of the intensest kind, and penetrating into the parenchyma of the lungs.

Epidemic.—Like every other inflammation of the respiratory passages bronchitis is clearly epidemic. There is a disposition to inflammation in the respiratory apparatus generally, but it depends on some unknown atmospheric influence whether this shall take on the form of catarrh, bronchitis, or pneumonia. I am not, however, prepared to say that it is contagious; at least I never saw a case in which I had reason to suppose that the disease had this origin.

Treatment.—Here again your first step will be to *bleed*, and here too will be the paramount necessity of your personal attendance while the animal is bled. This is a disease of a mucous,—an extended mucous surface,—and although our measures must be prompt, there is a tendency to debility which we should never forget. Although I have sometimes seen the horse distressed quite to the extent which Mr. Charles Percivall describes, yet he would not bear the loss of four pounds of blood without fainting. You will order, or you will take no determinate quantity, but you will bleed until the pulse falters, and the animal staggers, and in a minute or two would fall. This may probably effect your object, if it does not, it is possible that you may not have a second opportunity.

You will be cautious in the administration of a *purgative*, for the reasons that I have again and again stated; but if the bowels are evidently constipated, you must give the small doses of aloes with the febrifuge medicine, and promote their speedy action by injections, so that a small quantity may suffice.

The *febrifuge medicine* so often recommended must be administered here. Ours seems to be as yet a small pharmacopœia. We shall enlarge it by-and-by, when we have to speak of other systems and other diseases; and, in the mean time, if we have that which will generally effect our purpose we have all that we can desire.

Some practitioners use the *white hellebore*. I have already explained my reasons for giving the preference to digitalis, except we have the horse in our own infirmary, and can cautiously and hourly watch the effect of the drug. Mr. Percivall relates a very interesting and instructive case. One of his troop horses had bronchitis; his respirations reckoned upwards of a hundred per minute, he puffed hard at the nostrils, and heaved laboriously and painfully with his flanks, and his countenance betrayed extreme inward oppression. He was bled, and, in the course of thirty-six hours, three drachms of hellebore were administered. Twelve hours after the last dose he was found lying down—one minute extended upon his side, the next lying with his head raised up, puffing violently at the flanks, looking back at his belly, and pawing. The pulse had sunk so low that it could hardly be felt. An ounce of laudanum was given to him in gruel; he soon became tranquil; the respiration subsided, and he recovered.

A *blister* is always indicated in bronchitis—it can never do harm, and it not unfrequently affords decided relief. It should extend over the brisket and sides, and up the trachea to the larynx.

His food, if he is disposed to eat, should be mashes. No corn should be offered, nor should the horse be coaxed to eat.

BRONCHITIS IN CATTLE.

This is peculiarly the disease of cattle. Every minor inflammation of the respiratory passages seems to terminate in bronchitis in these animals, as regularly as it does in pneumonia in the horse.

Cause.—The principal and sadly neglected cause is that *winter cough*, which cruel neglect generally produced, and which inattention and idleness have suffered to continue. Cough is so frequent in the cow-house, that neither the herdsman nor the proprietor takes the slightest notice of it. It is most frequent after feeding or watering, and it goes on for weeks and months, and no harm seems to be done; but, at length, the animal begins to lose flesh, and the milk diminishes. Then, and when it is too late, the farmer begins to look about him. It is now confirmed bronchitis, complicated with inflammation of the parenchyma of the lungs. The false membrane of which I have spoken in the horse lines the bronchial tubes—the inflammation of the mucous coat beneath is intense—the foundation is laid for consumption—the animal becomes a mere skeleton, and they who are accustomed to cattle will understand me when I say, that the poor beast crawls about with merely a dry skin covering a set of creaking joints.

The moment the cow with cough is in the slightest degree off her feed, she should be attended to; indeed, the simple fact of her having violent cough, should induce the farmer to adopt some remedial measures. She should be bled; a dose of physic should be given, and a few febrifuge drinks; she should be placed in a sheltered comfortable place, and have a good mash given her every day.

The prevailing causes of this inflammation of the bronchial passages are various. If any of the southern breeds are transplanted far north, they will require a great deal of attention and nursing, in order to ward off this insidious and fatal affection. I have been twice consulted respecting a beautiful short-horned breed, which had been introduced into the northern counties of Scotland, and the greater part of which died evidently consumptive in less than two years. In the first case, I recommended one cross with the native breed, and then to go back to the Durhams. It perfectly succeeded. A constitution better suited to the climate was acquired, and after one or two generations the form of the short-horn seemed to be scarcely affected, and certainly not one good quality was impaired.

In the other case this was peremptorily objected to: the deterioration of the breed was regarded as certain. Then by gradually using less nursing when the animal was well, and a good deal more as soon as this cough appeared, and by early bleeding, and the use of the febrifuge drink, we prevented any confirmed bronchitis, and the cattle were gradually inured to their new and colder climate.

Unnecessary exposure to, and over-anxious care to preserve them from cold and wet, are equally injurious. Cough, and all its worst consequences, are never more frequent than in a hot and crowded cow-house, and to which fresh air is rarely admitted.

The food of the animal is often not a little concerned in the production of cough, and even bronchitis. I have seen many a chest affection rapidly and fatally succeeding to the substitution of mouldy hay, and bad straw for wholesome food.

In proportion to the purity of the breed, and especially if the *in-and-in* system has been too closely followed, is the predisposition to these chest complaints.

To young cattle bronchitis is particularly destructive. When they are kept in the straw yard during winter upon scanty food, and the forward ones drive the others about, and scarcely permit them to eat at all, pulmonary complaints are exceedingly frequent from the combined influence of wet and cold, and fear and hunger.

Bronchitis from Worms.—There is one species of bronchitis which must not pass without observation. A yearling has often a cough unusually distressing. The intermissions are short, the paroxysms exceedingly violent. The beast is off his feed, hide-bound, his belly tucked up, his coat staring, his flanks heaving, and it is almost heart-breaking to hear him cough. This occurs principally on low marshy, woody land; and is confined to young cattle, and principally yearlings. The farmer knows too well what is the matter, and the uniform fatality of the complaint, and he either slaughters the animal at once, or suffers him to take his chance, and rarely or never requires the veterinarian's aid. The beast at length dies, and the whole of the bronchial passages are found to be completely choked up with worms. They are of the *strongylus* species, and either the *filaria* or the *veinulosus*, but chiefly the former. Many of them are also found in the trachea; and the mucous membrane of the trachea and bronchiæ presents an appearance of the intensest inflammation, and by which the animal was destroyed.

The generation of these parasitical animals will be best discussed when we come to the consideration of the different entozoa of the intestinal canal. In the human being worms are oftenest found in an injurious quantity, in a cold and moist state of the atmosphere; in moist situations; and especially in those that are covered with the vegetable productions peculiar to such a locality; in young subjects; and where the constitution is somewhat enfeebled. We have all these predisposing causes here, and yet the generation of these animals, and the mode by which they are introduced into the system, are perfect mysteries.

Whether they are the cause or effect; whether amidst the vitiated secretion of the inflamed mucous membrane of the bronchiæ, the ova find their proper and destined nidus, and are there hatched into life; or the membrane is irritated and inflamed by the presence of these parasites, deposited ere the part was diseased, are subjects that will be better illustrated hereafter. In point of fact, they are found to occupy the whole of the bronchiæ, and by the irritation which they produce they destroy the beast.

Treatment.—The question that most concerns us now, is the mode by which they are to be got rid of. Nothing can be brought to bear upon them immediately. No turpentine or other fluid destructive to worms can be introduced into the bronchial tubes. Can any deleterious gas be employed? Can it be introduced into the system in a quantity not sufficient to injure the beast, yet with the chance of destroying these vermin? Might pieces of tow, impregnated with a solution of chlorine, be spread over the trachea, and particularly the lower part of it? Or might the animal be shut into a close place, and made to breathe diluted chlorine gas, or nitrous acid gas? A portion of it would pass the watchful sentinels in the nasal cavities, and at the entrance into the larynx, and possibly enough to effect the desired purpose. The animal would be not a little annoyed, he would for a while cough with even greater violence, and the worms dying, or their hold loosened, might be expelled. This is really worth trying, and should you succeed, Gentlemen, you would gain much credit, and confer an essential benefit on your country; for thousands of young cattle are destroyed every year by these worms in the air-tubes.

Oil of turpentine is peculiarly destructive to worms. It is also one of those drugs that enter into the circulation; that are recognizable in the urine and the breath, and produce their effect by immediate contact with the part on which they operate. Can it, through the medium of the circulation, be brought into contact with these worms? It is worth the trial. Or are there medicines which promote expectoration or loosen the morbidly viscid mucus secreted in these tubes, and cause it to be more easily discharged in the act of coughing. There is no doubt that certain drugs effect this purpose in the human being; the exhalents of the bronchiæ under their influence pour out an increased quantity of mucus, and that far less adhesive, and expelled with little difficulty. From the erroneous habit of contracting our Pharmacopœia, and shunning almost every drug in which the human practitioner places confidence, we have made few experiments for this purpose even on the horse, and none on cattle. The turpentine should take the precedence in this mode of treatment, and that failing, it might be worth while to try the effect of squills or gum benzoin, or the balsams of Peru or Tolu.

Worms in the Bronchial Passages of other Animals.—Sheep are far less troubled with these worms than young cattle, but several cases are on record in which the bronchiæ of these animals have been filled with the strongyli. In the deer they are oftener found. They are exceedingly troublesome, and often fatal in the hog, and they destroy thousands of poultry. In the year 1822 almost all the pheasants in Colonel Berkeley's preserves died, and on examining them the bronchial passages were choked with worms. Turpentine might be administered to the sheep and the hog;—to birds it is poisonous.

CLINICAL LECTURE

ON A CASE OF

STRICTURE AT THE SIGMOID FLEXURE OF THE COLON.

BY

FREDERICK SALMON,

Senior Surgeon to the General Dispensary, Aldersgate Street.

DELIVERED AT THE INSTITUTION

March 31, 1832.

Gentlemen,—The task of refuting opinions long entertained on any subject is an exceedingly difficult one, more especially when those opinions have emanated from men who rank high as philosophers possessed either of great learning or great genius. This consideration, coupled with the circumstance that the present is the first occasion upon which I have attempted to deliver my sentiments before an assemblage of my medical brethren, will, I trust, incline you to extend your indulgence towards me. Indeed I think I may ask this favour as an act of justice, not alone for the reasons just stated, but also on account of the importance of the subject, which, though not altogether new, may be said to be one with which the medical public is but ill acquainted. So little indeed is known respecting it, that there are not wanting many among its members who doubt even the existence of such a disease as stricture of the rectum. I myself have the pleasure of ranking amongst my friends some who entertain this belief.

By a reference to the *lectures* of the various teachers of surgery, you will find that while some of them make no mention of this disease, others either allude to it superficially, or describe it as one beyond the power of surgical aid. It would be improper for me to particularize any individual opinion of this kind; indeed such a course might be justly deemed invidious, and would be altogether foreign to my purpose, since my only object is to impart to you sound knowledge, the result of practical experience.

Again, if you peruse the *writings* of those who are considered to have paid more than an ordinary degree of attention to the diseases of the rectum, you will perceive that they also are equally deficient on this particular point, although it may fairly be considered to form a part of their subject. In fact I know of no author to whom I can refer you for much practical information upon it, with the exception of the late Mr. White of Bath. In early life I was acquainted with that excellent surgeon, obtained a knowledge of his opinions, and was induced, from a high confidence in his character, to follow the path he had pointed out. Experience soon satisfied me of the important benefits to be derived from such a course.

I was attending a gentleman afflicted with disease in the bladder and urethra, who after some time became dropsical, an effect which was considered by a distinguished physician as well as by myself, to result from a diseased liver. It soon became necessary to remove the fluid from the abdomen, but upon examination I found the liver (as I believed) so much enlarged, that I deemed it prudent to delay the operation. Indeed the performance of it appeared not altogether free from danger. In a day or two the patient was seized with an attack of dysentery, from which it was expected he would have died, and so near death was he, that I actually guided his hand while he held the pen to affix his signature to his will. I left him late in the evening, fully expecting when I called the following morning that his earthly career would have been closed; not a little astonished, however, was I to find him sitting up in bed eating beefsteaks for his breakfast. He noticed my surprise, and said, "Doctor, you look astonished," to which I replied, "Verily I am." "I will astonish you still more then," said he, pointing to a large number of chamber and close-chair utensils, "I have had upwards of forty stools since 12 o'clock last night, and I should be very well now, only that my fundament is down." At that time I had not paid much attention to diseases of the rectum. As a matter of course I restored the prolapsed bowel, and having so done I thought it prudent, in order to quiet the irritation of the part, to order a small opiate enema; but I found that it could not be thrown up. I then introduced my finger and desired him to make an effort to produce a motion, which brought down a circular contraction of the rectum. I said to my patient, "You have got a stricture in the bowel." "What the devil is that?" said he, "a stricture in my bowel!" I explained to him that the part was contracted, and the caliber diminished. He lifted up his hand, struck me on the shoulder, and said, "D—n it, you are right; I have often strained my guts to fiddle-strings, without getting anything from them." As I walked towards the door of the bed-room the doctrines laid down by the gentleman to whom I have alluded (Mr. White) flashed across my mind, and I thought this a fair opportunity for putting his practice into execution.

I mention these circumstances, not with a view to give an additional colouring to my subject, but in order that it may make a lasting impression on your minds. I narrated what I had discovered to my medical colleague, who laughed at my information; but I must do him the justice to add, that he did so from conviction, for I recollect his stating, that although he had had upwards of twenty years' professional experience, he had never yet seen two cases of stricture of the rectum. Subsequent examination proved that this patient likewise laboured under stricture of the sigmoid flexure of the colon to such an extent, that even a urethra bougie could not be introduced. After a tedious attendance I was, however, so far able to dilate both obstruction, that I could with facility pass a No. 8 rectum bougie. Long before this was accomplished, the whole of the fluid collected in the abdomen, thighs, and lower extremities, dispersed, without the administration of a single medical remedy, save a few doses of castor oil. I remember, likewise, that the fluid in a great measure did not pass by the bladder, but by the rectum. Of this I am certain, for the patient himself told me, that he had sat on the water closet and voided large quantities of fluid, pint by pint, from his fundament. Immense collections of feculent matter were daily brought away by the use of injections, and the supposed enlargement of the liver, which turned out to be nothing more than a distended colon, totally disappeared, and his general health was so far restored, that he was able to resume his ordinary occupations, and to enjoy the society of his friends.

I admit that this may be looked upon as an extreme consequence of neglected stricture at the sigmoid flexure of the colon; yet I am certain, that if you carefully investigate the opportunities afforded you in practice, you will find many results which, though dissimilar, may nevertheless be traced to the same source. In so doing, you must expect to find your path beset with difficulties, but you must make up your minds to encounter the prejudices of education and custom, with the assurance that onward lies the road to extensive utility and well-deserved fame. Having made these few preliminary remarks, I proceed to the elucidation of the subject respecting which we are now assembled.

I have stated to you that the case is one of stricture of the sigmoid flexure of the colon, and I do not know how I can better put you in possession of the facts connected with it, than by giving you a short extract from the notes I made when I first visited the patient who was the subject of it.*

The treatment which I pursued was, first to correct local irritation as far as possible, by the administration of astringent medicines in combination with opium, and by sedative enemas. In about a fortnight or three weeks, the rectum having become pretty quiet, I examined it with a bougie, but found no disease in the organ. At the sigmoid flexure of the colon, however, I encountered an obstruction which only admitted an instrument of the size of No. 5. After each introduction she appeared to be rather better, but the benefit derived from this or any other treatment was only temporary. She grew progressively worse and worse; the attacks of irritation were more and more frequent, and ultimately she died. About six weeks before her death, after one of her most violent attacks of purging, she told her female attendant, that she was sure some substance was lodged in the front passage, and this person, upon a careful examination, discovered there a triangular piece of bone, which she withdrew by means of a pair of pincers.

When first I saw this poor creature, I had no suspicion that the fæces came by the bladder. In fact I confess I did not think so till towards the latter period of her existence, and for this reason; I repeatedly found the urine clear and healthy, and it appeared to me, that if there was a direct communication between the sigmoid flexure of the colon and the bladder, the fæces would in all probability have entered the latter organ by gravitation, and thence have been voided by the canal of the urethra, daily, which they certainly were not. Notwithstanding this circumstance, however, I ought to have known that they must have come by that channel, not so much because I could not discover the communication between the two passages by examination with the finger, but because when I passed the bougie into the rectum, I found no disease in this part which could have given rise to so serious an effect. I several times requested her to allow me to examine the vagina with the speculum, but she would not consent. Had this been done, the case would have been clear beyond a doubt. The patient, as I have stated, died, and I will now show you the morbid parts of her case, a model of which I have brought with me. Here you perceive the serious extent of mischief which occurred. This is the extremity of the descending colon, into which I now introduce my finger, and which is, as you observe, a good deal dilated, from the constant accumulation of feculent matter. Here (exhibiting a model of the diseased parts) is the sigmoid flexure, great part of which is destroyed by ulceration, forming a direct communication with the bladder, which (if we except the destruction of a part of the fundus by ulceration) is not much diseased. It is, perhaps, a little thicker than common, and of small size, both which circumstances might naturally be expected to follow the immoderate and constant irritation which the organ must have suffered. The most singular change in this part was, the peculiar green colour of its mucous coat, which is faithfully shown in the model. The urethra, you perceive, is not much dilated, but it was greatly inflamed. The vagina is healthy, nor is there anything remarkable in the womb, beyond the obliteration of a portion of its fundus from coagulated lymph, and the formation of a small polypus just within the os tincæ. The appendices epiploicæ are enormously enlarged, an effect commonly produced by stricture of the rectum, and which I have invariably found in conjunction with stricture at the sigmoid flexure. In the rectum there is no disease, but its size is very small, a circumstance which you will usually find in cases of obstruction at the sigmoid flexure. The rectum not being distended, as it ought to be, by the natural accumulation of fæces, acquires the utmost degree of natural, or, as some authors have described it, *passive* contraction. The gut may easily be dilated, showing that there is no structural disease of the part.

You cannot but be struck, on looking at the preparation, with the power which nature sometimes exercises during the progress of disease, to prevent the immediate destruction of life. You perceive that the roof of the stricture, if I may be allowed the expression, is formed by an adhesion of the peritoneal surface of the rectus muscle with the upper part of the sigmoid flexure, while the floor of the strictured part is made up of a union between the under surface of the sigmoid flexure, and the upper portion of the fundus of the bladder, so that the ulceration took place into the latter organ. Now before such an effect could be produced as is here shown, it is necessary that an alteration should take place in the natural state of the colon. This drawing represents the usual condition of the parts. But it is requisite that the position of the end of the intestine should be changed, in a manner somewhat similar to that shown by this diagram. Here is the colon, this the sigmoid flexure elongated and brought down in a kind of loop forming an acute angle and resting upon the fundus of the bladder, to which, in this poor creature's case, it was firmly united. Had not this union taken place, the bowel would in all probability have ulcerated just above the strictured surface into the cavity of the abdomen, which I need scarcely add would have been immediately destructive of life.

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* The particulars of this case appeared in THE LANCET of March 24th, and need not therefore be repeated here.

We often find a similar alteration in the position of the sigmoid flexure in patients who have long laboured under stricture of this part. From the accumulation which takes place above the contraction, the bowel becomes preternaturally distended, this circumstance, together with the straining necessary to void the evacuations, enlarges the bowel, and elongates the mesocolon, and in this manner the effect I have shown you follows.

In reviewing this case, you will not fail to observe the difference between the symptoms which occurred in the course of its treatment, and those which accompany malignant disease of the sigmoid flexure, which I do not consider this to have been. Nor are the appearances of the morbid anatomy less decisive. I now show you a preparation of malignant disease in this part. Instead of the bowel being, as in the instance under consideration, hardly thickened, the deposition is at least an inch thick, and the individual was destroyed by the stricture ulcerating into the cavity of the abdomen. Here is another specimen, in which the like results were produced. Here again is a third preparation exhibiting that most horrible af all horrible diseases, fungus hæmatodes of the sigmoid flexure. In this instance an aperture was at last formed through the external integuments, by which the motions were passed. It is a remarkably fine specimen of disease, which I am enabled to show you by the kindness of my friend Mr. Kiernan to whom it belongs. I exhibit these preparations, in order to establish a position which is most important in the treatment of stricture at this part—to wit, that though we find, as these facts incontestably prove, malignant disease attacking the sigmoid flexure, yet we also have what may be termed simple stricture, such as this poor woman's case indisputably was.]

Now having shown you the disease itself, what are the practical deductions which may be drawn from it? Unquestionably the most important is, that we should be aware that such an affection may exist, and that, being conscious of it, we should use every possible caution to detect and relieve it in its early stages. It is very evident that the real nature of this woman's case was not known until it had proceeded to a very considerable extent. Had it been discovered earlier, by a proper examination of the bowel, it is more than probable that she might have been relieved. Indeed I see no reason, but for the accidental circumstance of the lodgment of a piece of bone in the strictured part, why she should not have been alive at this hour. The question then naturally arises in your minds, how these points are to be effected. They may certainly be done by the judicious employment of the rectum bougie, but as it is not possible to do justice to this part of the subject by anything I could say, without practically illustrating the introduction of the instrument, I shall not further dilate upon this point at present. On the use of the bougie I would defer speaking till a subsequent period, when I shall enter most fully into the subject, exhibiting its utility, not only in connexion with these cases, but also in diseases of the rectum generally.

There was one fact in the treatment of this case which I omitted to mention, viz. the extraordinary quantity of opium this patient was enabled to take without affecting her mental powers. Although, during the last fortnight of her existence, she took a grain every hour, her faculties were perfectly clear to the last.

I will not detain you with any further remarks upon the case. The morbid parts are before you, and you may investigate them for yourselves.

ON THE

PATHOLOGY AND TREATMENT

OF THE

CHOLERA.

By Joseph Ayre, *M.D., &c. &c.*

Sir,—The communication which I made to your Journal of the 15th instant, on the cholera morbus of this country, was erroneously entitled an essay on the present malignant type of the complaint. The error was a natural one, for the description, though faithfully given, might readily be mistaken as applying alone to the Indian form. The object, however, of that communication was, as is the present one, to prove an identity in the pathological conditions of the two forms of the complaint, founded on a comparison, first of their symptoms, and next of what is infinitely more important, and the main object of this letter—of an identity in the treatment demanded for them.

The description I gave of the English cholera embraces the severer type of it, and is based on the pathological fact, overlooked or denied by many, of an interruption or an entire suppression of the biliary secretion as its proximate cause. By some of your readers, perhaps the description so given may appear to be drawn from the Asiatic complaint. I must be allowed therefore to state, that the account given of the symptoms, as well as the pathological views founded on them, are substantially the same as were delivered by me in a work* on that subject fourteen years ago, and before I had heard of the Indian epidemic. The very term of *collapse,* which is now so distinctively applied to the first or blue stage of the present epidemic, was so employed by me, and, if I mistake not, was first employed by me to designate that stage in the English disorder; whilst the fever now termed consecutive, which belongs to it, but which is almost unmet with in the Indian complaint, was especially noted in that work as of frequent occurrence, and as a sequela—as it is now—of the stage of collapse, and in some respects a remedy for it. It may be also not unimportant to add, that the materials of that work, which was published in 1818, were collected in 1817, and derived from observations made upon an epidemic cholera of unusual severity, which prevailed in that year in Hull, and other parts of the United Kingdom, and which was consentaneous with an epidemic cholera akin to the present, if not the same, which also in that year first broke out in India. The atmosphere of this country, indeed, was imbued during that year, as it appears to have been during the last, and in the present one, with that specific malaria which generates a severer type of cholera; and whilst some of the cases were attended with the most striking symptoms of collapse, resembling in almost every particular those of the present disease, others, and certainly by far the greater number, appeared under a milder form. When the collapse was not resolved by a sudden reaction of the secretory vessels of the liver, a fever succeeded, in which, to quote the words of my report in the work referred to, "the functions of the liver were so remarkably affected from the commencement, and during the whole progress of the fever, as to give to the disorder a completely bilious character. It began in some cases with vomiting, in others with purging, or with both. The stools were either *black* or *clay* coloured. A spontaneous looseness appeared in a great many cases in the course of the disease, but in none was it salutary excepting where it was decidedly bilious. The fever and other symptoms, especially a cough which (like the head affection of the present epidemic) was symptomatic, were aggravated by opiates, which stopped the alvine discharge; but the fever and looseness, as well as the cough and other symptoms, were all reduced, and I believe only reduced by those free purgative means which promoted the secretion and descent of the bile." Of this epidemic, from my holding hospital and other public appointments, I attended nearly one hundred and fifty cases, which were limited then, as the present epidemic is now, almost entirely to the lowest of the labouring classes. The collapsed stage of the complaint was seldom very severe, and was generally short, so that I seldom was called to attend the complaint before the collapse was resolved into the consecutive fever, or into its natural remedy of a reaction in the secretory vessels of the liver, and its concomitant effects of a vomiting and purging of bile. Some cases however fell under my notice where the collapse was severe, and in those cases I was enabled, by means to be presently noticed, to prevent the occurrence of the consecutive fever. The following is one of these cases, which I extract from the work before alluded to:—

CASE.

Sept. 12, 1817. T. W., aged 14, is affected with a considerable degree of pain and oppression about the region of the stomach, and with an almost incessant retching and vomiting, and a death-like coldness and lividness of the hands and feet, and a blackness about the mouth; the countenance expressing much anxiety and restlessness; the pulse quick and compressible; the bowels confined; the surface of the body and head is covered with a cold perspiration; the matter discharged by vomiting sour and greenish; was seized last night suddenly after a fortnight's indisposition under the chronic form of marasmus (biliary obstruction); wine has been given to him under a persuasion that he was at the point of death, and various means have been employed to restore warmth to the extremities with effect. A small dose of calomel was directed to be given every half hour for six successive hours.

Second day. Only a few of the pills ordered were taken, as his friends considered him to be dying, and that no means could be useful. The coldness and discoloration of the extremities continue, and the sickness and oppression are unrelieved. *The pills to be taken punctually* every half hour for six hours.

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* Practical Observations on the Nature and Treatment of Marasmus, and of those Disorders allied to it which may be strictly denominated Bilious. 1st edition, 1818.

Third day. Has taken all the pills, and the warmth and natural colour of the extremities have returned, and the sickness, and pain, and oppression, very much relieved; the bowels still confined. Epsom salts ordered, and the pills omitted.

Fourth day. The sickness and other symptoms returned in a considerable degree during the night, and he passed this morning several stools which are said in colour to resemble soot. The extremities are now however warm and of their natural colour. To have his pills again every half hour for six hours.

Fifth day. Has taken all his pills of calomel, and had several evacuations from his bowels of a better colour. Has no sickness, and the pain and oppression are very much relieved. The pills to be omitted; the aperient to be continued. From this time the patient rapidly improved, and in a day or two was convalescent.

In the above case are assembled all the symptoms which distinguish the collapse of the present epidemic, with the exception of the purging and cramps, which are not always present, but when they are they are usually together, and of the suppression of the urine, which is not noticed, but which most probably occurred in this case; for paucity of urine, and even a long suppression of it, is a symptom frequently noted in that work as an attendant on that complaint. Epidemics, however, of the same disease in different years vary from each other in the prominency of particular symptoms, and in their general intensity, without these variations being at all dependent upon any essential change in the condition of their proximate cause, or demanding any material deviation from the prescribed indications of cure. The epidemic cholera which prevailed in England in 1817, and which prevails more or less every year, was far less severe than the present, but the milder cases of that now prevailing run the same course in its consecutive fever as those of the other epidemics; and were the severe cases of collapse in the present disease the exception instead of the rule—as it is in others—the resemblance between them would be complete. Thus the fever, which is but secondary in importance in the present disease, would be then, as it is in others, the prominent affection, and be named and recognised only as a bilious fever ushered in by a vomiting and purging, and a temporary coldness of the extremities. The mild and malignant cholera cannot indeed be more unlike than are the distinct and confluent small-pox, the simple and putrid measles, or the scarlet fever, which is sometimes only distinguishable by its scarlet efflorescence, and that most pestilent form of it the angina maligna; and it is only in accordance with what we know of the nature of epidemic diseases to assume that the severest form of each respectively is but a variety of the mildest kind, and that each is convertible into the other according to the right or wrong methods adopted in their treatment, and still more as they are affected in their origin or course by certain unappreciable conditions of the atmosphere. But it is necessary now to proceed to the consideration of the treatment.

It was observed in my former letter that the liver, as a secreting organ, is acted on sympathetically by the stomach, it being out of the course to receive any direct stimulus from the ingesta, and that it receives a healthy stimulus and secretes a healthy bile when the food is wholesome, and the stomach in a healthy state to digest it; whilst, on the contrary, a morbid stimulus will be imparted to it, and a repression of secretion be induced, when the ingesta is unwholesome, and especially if to this be added a disordered stomach to receive it. Now it is upon the law of this established sympathy between the stomach and liver that I have framed my rule of practice in this complaint; and which, as founded on the pathological fact of an interrupted or suppressed secretion of bile, proceeds alone upon the principle of restoring it, and of resorting to those means exclusively which can do it most promptly. Of these means an experience now of many years has taught me that calomel is above all comparison the chief. The property which this medicine possesses of acting upon the liver and thus promoting its secretion, has indeed long been known and acknowledged, but the mode of its so acting does not appear to have been sufficiently considered. From a long practice in the use of it, I am satisfied that it affects the liver by an action which it excites in the stomach, by either altering its secretions, and thus renewing a healthful action in this organ, or by a particular stimulus it imparts to it, and which is sympathetically communicated to the liver. When a patient, labouring under a congested state of the liver from an interrupted or deficient secretion of it, takes a small dose of calomel at bedtime, it commonly happens, that instead of the restlessness of former nights, he sleeps more calmly and sounder than usual, and awakes in the morning with the conviction that he owes his rest to an opiate. This fact has so frequently fallen under my notice, as to satisfy me that the restlessness in this complaint depends upon a disordered action, which the medicine relieves, and that this disordered action is mainly in the liver; and that the beneficial change produced is in the function of that organ, appears from the favourable alteration made in the condition of the alvine discharge pending the use of the medicine, for previously to its employment they are perhaps of the colour and consistence of tar, or of that of pipe-clay, and of an unnatural fetor, verging under its use to the colour of yeast, and thence to their natural condition and appearance. But the power which this medicine possesses over the secretory functions of the liver, is not confined, as is generally supposed, to increasing its activity when in a sluggish state, for it is equally efficient to reduce its secretory action when in excess, its tendency being to restore the action of the liver, whether deficient or excessive, to its natural and healthy state. In those cases, therefore, where the secretion of the bile is deficient, it restores it, and where in excess it represses it. In the first stage of cholera, and in the bilious diarrhœa which succeeds, it is the remedy upon which I have for many years relied. In giving calomel in this disorder, it must be borne in mind, that its primary action is to be upon the stomach, but that it should not act *purgatively* upon that organ, by exciting an action that shall carry it quickly through it: nor as an irritant, by which it may be regulated by vomiting, or occasion a morbid irritation in it. The dose, therefore, should be small, as from half a grain to two grains, that these effects may be prevented, and frequently repeated in urgent cases of venous congestion of the liver, that its effect upon it may be uninterruptedly continued; and if the stomach and bowels are in an irritable state, minute doses of opium may be properly combined with the calomel, to retain it the necessary time on the stomach. To those who consider large doses of calomel as necessary, and have no experience in the use of small ones, and who think in all violent diseases, the maximum dose of a medicine to be necessarily the most efficient, the giving of a grain of calomel with or without a minute dose of opium every quarter or half hour, for a few successive times, may appear very inadequate treatment for the several forms of the English cholera. It is, however, the treatment which I have pursued for the last twenty years, and in no instance have I seen occasion to deviate in the least from the practice.

In the treatment of the cholera morbus, there are two principal indications of cure to be fulfilled; namely, 1st. To remove the congested state of the liver by those means which restore the secretion of the bile; 2nd. To repress the inflammatory action with its attendant fever, which succeeds the stage of collapse, continuing with the means for this purpose, those which are required for the first stage.

When a case of the common cholera of this country is seen early, and before its first stage has become resolved into the inflammatory one, and the treatment by small and repeated doses of calomel been actively pursued, the secretion of the liver becomes restored, and no consecutive fever ensues; and as the debility in the first stage is only temporary, and wholly depending upon the disturbance given to the circulation, the recovery to health is both sudden and complete. Should this stage of collapse not be early attended to and be thus removed, and the patient survives it, the stage of reaction ensues, and then it becomes necessary, in addition to the use of calomel, to employ local depletion by very *copious* leeching on the epigastrium, saline purgatives, extreme low diet, with those other general and local means to remove the local inflammation in that region, and in some of those parts of the system where secondary effects arise from sympathies established between them and the digestive organs. During the treatment so pursued, it must, however, be always borne in mind, that as the inflammatory reaction is caused by the congested state of the *vena portarum* and its branches, and the congestion itself, by their interrupted action as secreting vessels, so the reaction, which has only a limited power to remove the congestion, is itself kept up by it, and can only be permanently removed by a permanent renewal of the biliary secretion, which is the natural remedy to both.

In the foregoing observations it will be seen, that I have confined myself to the consideration of the nature and treatment of the epidemic cholera and its consecutive bilious fever, as common to this country. Believing, however, that it is but a milder grade of epidemic now raging, I was early persuaded that their pathological conditions are the same, and that the same method of treatment is available for both, being the more strongly led to this conclusion from Dr. Keir of Moscow; having referred to my work on the English disease, as affording a just explanation of the pathology of the Asiatic complaint, and in having avowedly adopted the nomenclature employed by me in designating its stages.* On the appearance, therefore, of the complaint in the towns of Sunderland and Newcastle, I proceeded to those places in December last, and through the kindness of Dr. Clanny of the former town, I had an early opportunity afforded me of putting my practice on trial, that gentleman having obligingly intrusted a recent case to my management a few hours after my arrival. The result of the treatment was such as I had confidently anticipated, and corresponded with what I had often witnessed in cases of less intensity, indeed, in the English cholera, where, by the administration of small and repeatedly renewed doses of calomel, combined with minute doses of opium, the biliary secretion was promptly restored, and the occurrence of the consecutive fever prevented. The following is the case, as drawn up from notes taken in the chamber of the patient.

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* See "Edinburgh Quarterly Medical Journal" for April 1831, page 436; also "Papers relative to the Disease called Cholera Spasmodica in India now prevailing in the North of Europe, and published by authority of the Lords of his Majesty's most Honourable Privy Council," second edition, page 11.

CASE.

Mary Thornton, aged 42, married, Wednesday, December 21, three o'clock p. m.,

Became affected late last night with spasms between the toes and in the plane of the feet, and with vomiting and purging. The matters thrown up and discharged from the bowels somewhat resemble thin barley-broth with a bran-like deposit. Has had great thirst since the commencement of the attack, and has passed no water since seven o'clock this morning. The vomiting and purging continue; the pulse was very distinct but very compressible, and about 90; the skin cool, and only slightly discoloured; the voice somewhat hoarse and whispering, and the countenance sunk. Dr. Clanny, whom I accompanied, ordered eight ounces of blood to be drawn, which was received into a stop-cork bottle, with a view to some experiments upon it. Immediately after the bleeding the pulse became more indistinct, and in a few minutes was quite imperceptible at the wrists. There was no faintness, but the skin became colder and more livid. Dr. Clanny thought it a good case for the mustard emetic; but on my suggesting to him the use of small and repeated doses of calomel, combined with a minute dose of opium, he and Mr. Embleton, the parish surgeon, obligingly invited me to take the management of the case, and which I gladly accepted. I then directed that a pill composed of two grains of calomel, combined with four drops of the tinct. opii, should be taken every ten minutes for the next two hours and a half, when I should see the patient again; a blister was also ordered for the stomach, and bags of hot sand to the limbs, &c. At my visit to the patient a second time, about an hour after, I found that the extremities had become more cold and livid, and the collapse more complete, the pulse being quite extinguished in the lower arm. There were no cramps nor any pain; and there had been no sickness since my visit. The pills were commenced at about four o'clock, and one was taken every ten minutes.

Nine p. m. Accompanied by Mr. Embleton. The patient still in the blue stage, and the pulse still extinct at the wrist. Has been only once sick, and no purging; the thirst is somewhat abated, and the patient expresses herself as feeling better. Mr. Embleton thinks her better, but to me she appears only better in not being worse. Completed the taking of her pills about two hours and a half ago. Mr. Embleton suggested a cataplasm of mustard and meal under the left breast which was applied; 15 doses of the two-grain pills with four drops of laudanum were ordered to be again taken every ten minutes as before. Having to visit Newcastle early the next day (Thursday) I was unavoidably prevented seeing my patient before setting off, and confided her to the able care of Mr. Embleton.

Friday, three o'clock p. m., saw my patient, and found that she had taken a second course of pills, and had become considerably relieved whilst taking them. In the course of Thursday she took from Mr. Embleton two boluses, containing each five grains of calomel; but she thinks she threw them up. On Friday morning she passed a small quantity of urine for the first time after forty-eight hours of suppression. She passed again twice during the day; the bowels had also been moved, and the discharges *were natural*; the coldness and lividness had left her some time on Thursday night; the pulse at my visit was 80, and of natural force. Her countenance and tongue clear. She had slept refreshingly, and expressed herself as feeling quite well, excepting in having a slight soreness of the mouth.

On my visiting Newcastle the day succeeding that on which I prescribed for the above patient, I had the pleasure of attending a meeting of some of the medical gentlemen of that town, to whom I named the treatment I had pursued, and my reasons for it; and after my second visit to Sunderland I acquainted two of the gentlemen whom I had met at that meeting of the favourable result of the case. At this period the disease had not long appeared at Newcastle, and had nearly ceased at Sunderland, but the practice consisted generally at that time of inordinate stimulation or copious bleedings, with mustard emetics, large quantities of opium, and scruple doses of calomel, there being, I believe, only one or two exceptions of gentlemen among the faculty there by whom more moderate doses of calomel were given, and more temperate methods of treatment pursued. The plan, however, of giving small doses of calomel at short intervals, combined with minute quantities of opium, was immediately afterwards adopted by many, and under various modifications as to the dose and time of repeating it, became soon, I believe, the rule with all. In the third number of the *Cholera Gazette* there is an official report to the Whitechapel Board of Health from the Board of Health of Newcastle, in which is given, as the practice they recommend, the treatment which I employed in the case at Sunderland, and recommended for adoption at Newcastle. The communication, however, was unaccompanied by any comment or explanation, and which I the more regretted, as I had early and fully communicated my opinions to the members of the Central Board of Health, and had waited long and anxiously in the hope that a detailed report would be published by the faculty of that town, of the results of their trials of this and other plans of treatment; for the single case of which I had to report a successful issue, though satisfactory to myself because that result corresponded to my anticipations, would not, I was aware, be so to others. The long-anticipated time, however, has now arrived for my renewing under my own observation, or that of my friends in this town, the trial of the practice which was begun by me at Sunderland; for the malignant cholera has now reached this place, and the following case, which was the first that occurred, will be found to have been treated by my friend, Dr. Chalmers of this town, precisely in accordance with the principles and practice which I have advocated in this letter, and with an exclusiveness as to the use of the particular remedy, and as to the mode of giving it, that allows of little doubt of the magnitude of their value in the treatment of this complaint.

CASE.

Hull, April 6. Jude Todd, aged 40, labourer, residing in Hales-entry, Market-place.

Became affected with diarrhœa on Monday, April 2d, which he attributed to his having taken several pints of ale on the Saturday, and a dinner of pork on the following day. This diarrhœa continued moderate until Thursday, when in the night it became much worse. He went to his work on the Friday morning, but soon returned home, and fell on the floor in a state of asphyxia. Upon his recovering from it he became affected with cramps in the feet, legs, and thighs, and in the left hand. His extremities also became cold, and somewhat livid, and his pulse was scarcely perceptible. There was also some darkness about his mouth, the countenance was altered, and the eyes somewhat sunken. He became affected with vomiting and purging; the matters discharged by stool being like rice water, and the fluid vomited, which was very copious, was like broth, and ejected with violence. He had likewise great thirst. He was visited almost immediately by Mr. Davis of the Dispensary, who gave him two grains of calomel with one of opium; and directed a sinapism of mustard to be applied to his stomach; this was at nine o'clock a. m.

Half-past Ten a. m. Seen by Dr. Chalmers and Mr. West of the dispensary. Two grains of calomel and half a grain of opium to be taken every quarter of an hour.

Half-past One p. m. Twelve doses of the calomel and opium taken; the sickness and purging continue, and the matters the same in appearance; the cramps still present though less severe. The calomel to be continued with the omission of the opium, and taken every half hour.

Four p. m. The calomel has been regularly taken since half-past one o'clock. The purging and vomiting abated in frequency, and the cramps much less severe, and confined to the legs; the pulse 80, and of good strength, and the surface not cold. The calomel to be continued every half hour, with a saline draught, containing three drops of the tinct. opii.

Seven p. m. The patient relieved somewhat farther in all his symptoms, but has passed no water since the morning. The calomel with the draught, containing three drops of laudanum, to be continued.

Ten p. m. Improved in all the symptoms, but no urine passed, and the stools still ricy. To take one grain of opium with the next pill of calomel. To continue through the night taking the pills of calomel with the draught as before; and one teaspoonful of brandy with each dose.

Saturday, Eleven a. m. Vomiting and purging ceased; has had some sleep, and is much better, but has passed no urine yet. To have an emollient glyster and one grain of calomel with two grains of colocynth every hour, and a saline draught without the opiate.

Nine p. m. The pills of calomel and colocynth have been regularly taken; sickness and oppression quite gone. The pulse has acquired some force, and eight ounces of blood have been drawn; the blood thick and black; has had two stools, which are tinged with bile, and has just passed some urine, for the first time during the last thirty-nine hours. The pills of calomel and colocynth to be continued, and some small doses of Epsom salts to be taken three or four times in the day. On the following day a gentle aperient was alone taken.

I saw this patient in company with Dr. Chalmers first on Friday at three o'clock, and which was seven hours after the treatment began. I saw him again in the evening, and early the following day, and once daily afterwards until Monday, when he was well, being free from all fever, and with every function natural. In a day or two afterwards he walked out.

Since the above first case on the 7th instant, four others have occurred. The first was in a man, who was seized at one in the morning, and who when seen for the first time eight hours after the attack had stimulants given to him, which produced or accelerated an inflammatory re-action, and he died in the evening from its effects, as was shown in the *post-mortem* examination by the marks of inflammation in the mucous lining of the larger curvature, and in the cardiac extremity of the stomach. The second case was of an old man, who died without any one being called to see him. The third was his widow, who was also aged, and for whom no assistance was sought until fourteen hours after the commencement of the attack, and was in a dying state when first seen. The fourth case was a young and athletic blacksmith, who was seized in the night with purging, and a few hours afterwards with vomiting and cramps, followed by some coldness and lividness of the extremities—suppression of urine, and the other signs of collapse; the pulse was distinct. He was first seen at two o'clock p. m., by Mr. Clay, of the Cholera Hospital, and Dr. Alderson, who bled him to eight ozs., gave him twelve grains of calomel with two of opium, and ordered two grains of calomel and a quarter of a grain of opium to be given every hour until the evening, when he was again bled, but with no apparent benefit. The calomel and opium were uninterruptedly continued every hour from this time, until the secretion of the liver was restored. His attack was on Tuesday last, and on Saturday he was well. Neither of these patients had any soreness of the mouth from the calomel. At the moment I am writing, this town is, I believe, without a case of the disease.

The length to which this letter has extended, forbids my yielding at this time to the intention I had formed of entering into a farther consideration of the various points remaining to be noticed on the subject, and particularly in regard to the use of those misnamed auxiliary means, which I fear have been too freely and too frequently conjoined with the treatment by calomel, and too fatally obviated its effects.

I am, Sir, respectfully, &c.
Joseph Ayre, M.D.

Hull, April 23, 1832.

CONTRIBUTIONS TO PATHOLOGY.

No. I.

By John Alexander, *M.D., one of the Medical Officers to the General Dispensary for Children, Manchester.*

CASE OF INFLAMMATION OF THE SPINAL CHORD.

December 16, 1831. Friday. I have this day been desired to visit Mrs. Wanlon, a publican's wife, ætat. 26, of stout frame, though low stature, residing in George Leigh Street, who states the following particulars. Subsequent to her third confinement, which took place fourteen months ago, she had enjoyed an uninterrupted state of good health until Sunday last, when she was attacked with considerable uneasiness in the loins, which gradually, on the Monday and Tuesday, became more and more severe. On Wednesday her usual medical attendant was sent for, and ordered four-and-twenty leeches to be applied to the part complained of, anodyne fomentations to be frequently used, and various purgatives to be administered.

This morning (Friday) her symptoms are—intolerable suffering referred to the site of the third, fourth, and fifth lumbar vertebræ, extending laterally about a hand's breadth. On examining the part complained of, there is no decided increase of pain on pressure, unusual heat, tumefaction, discoloration, or, in a word, any deviation from a healthy appearance of the loins. The mind is anxious, and countenance flushed from suffering. Pulse 125, rather weak, and ill bearing the digital pressure; tongue whitish; bowels obstinately constipated, not having been relieved, although calomel and colocynth pills, with infusion of senna, castor oil, and tincture of rhubarb, have been, during yesterday and this morning, successively administered; urine scanty, but made without difficulty.

We have prescribed a repetition of the leeches, followed by the application of a large blister, and two drops of the croton oil every two hours, until the bowels shall be acted upon.

Ten p.m. This evening we find our patient much easier. The leeches bled freely, and the blister is now rising. The pills have been followed by three dark offensive motions. More urine made. We have given her an anodyne containing sixty drops of laudanum, in order to induce some sleep, which she much longs for.

17. Her night has been tranquil, but today Mrs. Wanlon's case presents a melancholy feature—that of complete paraplegia.

The lumbar pain has entirely subsided, but her command over the lower half of the body is wholly gone, being unable to move either leg, to command the retention of her fæces, or to evacuate the contents of the bladder. She appears better in spirits, the pulse being 100, tongue moist, and some appetite mentioned. We have dressed the blister with savine ointment, and directed the urine to be drawn off in the evening.

18. Much as yesterday, but weaker; a larger caustic issue has been placed on each side of the lumbar vertebræ; opiate repeated.

20. Not the slightest return of command over the limbs, which, though so powerless, have a temperature equal to that of any other part of the body. The alvine dejections are frequent and offensive. Catheter still requisite; pulse 125, and weak. We have ordered her decoctions of bark with nitric acid three times a day, and her draught to be doubled in strength in order to secure sleep, and to quiet the incessant action of the bowels.

24. The slough on the site of the issues is considerable, and the separation almost effected, but there does not appear the slightest amelioration in the symptoms. Indeed the woman's strength is rapidly giving way. We have ordered a free use of wine, the decoction continued, and an addition of twenty-five minims of tr. opii to her draught, as her last night was restless, and much disturbed by startings, dreams, &c.

25. Slough separated; pulse 140. Says she feels much weaker; has slept pretty well the last night, although still observed to start and twitch during her artificial slumber.

26. Yesterday morning a slight but troublesome cough was observed to annoy her, and has continued since to do so; it is unattended with expectoration. Says she is perfectly free from pain; has taken a bottle of port-wine during the last twenty-four hours, and complains chiefly of a sense of sinking; pulse 148, extremely feeble; speech almost inarticulate; abdomen become tympanitic; fæces and urine now *both* passed involuntarily.

27. Mrs. Wanlon was taken in the evening with difficulty of breathing, and was soon relieved from further suffering.

The above notes are extracted from my case book. Three days after decease, with great difficulty, leave to institute an inspection was obtained; for the following particulars of which, owing to unavoidable absence, I am indebted to Dr. Stephens, a talented and indefatigable cultivator of pathology, and lecturer upon anatomy in Manchester.

Post-mortem Examination.

"The body was well proportioned and but little emaciated. There were two eschars opposite the lateral parts of the lumbar vertebræ, about the size of a crown each, evidently intentionally produced.

An incision was made from the middle of the back part of the neck to the os coccygis, and then the muscles were dissected back from each side of the spinous and transverse processes and arches of the vertebræ. In doing this, about the 9th or 10th dorsal vertebra, or perhaps a little higher, but precisely between the scapulæ, we opened into a small abscess on each side of the vertebræ, lying close up to the vertebral column, and accompanied by slight caries of the walling vertebræ. The pus contained in these abscesses was what is termed laudable, and amounted to near four ounces in quantity. The vertebral canal was then laid open to its whole extent. The theca vertebralis, for about four inches between the shoulder blades, and corresponding to the caries and abscesses, was rough, covered with lymph and pus, and greatly inflamed.

On slitting up the theca we found it of a beautiful pink colour in its inner face, corresponding to the diseased external part; its remaining portion, both internal and external, appeared quite healthy. There was not any purulent matter within the theca, but at the lower part, opposite the bulb of the spinal chord, we found about two ounces and a half of a clear limpid fluid, forcibly distending the theca.

The chord having been completely exposed, we now proceeded to its examination, and found its vessels much more tinged than is natural to them. This vascular infiltration was particularly remarked opposite the ninth and tenth dorsal vertebræ, corresponding to the inflamed portion of the theca before alluded to, and here there was an effusion of coagulated lymph (a thin layer, about the diameter of a shilling) on the medulla, under the arachnoid. There was observed also a patch of smaller size on the bulb of the chord.

On very particular inquiry from the family, it was elicited, that Mrs. Wanlon had suffered occasionally for months past, from pain in the back between the shoulders, and was in the habit of requesting a friend to forcibly rub the part complained of, which always relieved her; but during this last illness she referred all her pain to the lumbar region. Dr. Alexander repeatedly examined the spinal column, and could not detect any pain on pressure, or fluctuation in any part of the back; and I can testify that the dead body presented no outward signs of abscess or disease corresponding with that particular part between the shoulders, which I feel disposed to consider the primary seat of the disease under which she died."

Remarks.

Spinal affections, although much has been written, and undoubtedly well written, upon them, still constitute a terra incognita in the great chart of medical science; nor is this fact, although lamentable, difficult to be explained. The nervous system generally, although performing the most vital part in the organization of man, exercises its functions in a manner not cognizable to the senses; and the important operations of that system are, therefore, only darkly viewed through the *effects* which we presume to be consequent upon them.

The case above related is submitted to perusal, not from its presenting any peculiar novelty of feature, but from its embracing a few topics, the brief allusion to which can scarcely prove uninteresting.

The first point to be mentioned, is the *concomitant existence of the two abscesses and spinal disease* in the dorsal region. From the circumstance of uneasiness having long existed in that part; from the presence of well-concocted pus in the abscesses; from the reported appearance of the spinal membranes, and caries of the vertebræ; it is clear, that this joint affection, although overlooked by myself, was of no recent origin. Taking this for granted, were the abscesses productive of the spinal disease, or was the latter the cause of the abscesses? The question is an interesting one. That the spinal affection was the primary complaint although unable to prove, I am inclined to believe, from the following circumstances. The dorsal muscles do not present a usual site for common inflammation and its consequences; and had they been such in this case, great length of time would not have elapsed without the latter producing more striking evidence of its existence. Again, the presence of an abscess on *each side* of the spinal column can more readily be accounted for on the explanation of their cause originating in one common centre, than on the more improbable supposition, that two symmetrical abscesses should spontaneously arise; and, lastly, on this point, we find in a lower, lumbar, region (the seat of the poor woman's more recent suffering) a similar morbid state of the medulla spinalis had arisen.

As in most such instances of disease, obstinate costiveness was an early, and paralysis of the natural functions a later, symptom, there is, perhaps, scarcely a tyro in the practice of medicine to be found who has not observed the remarkable difference of effects induced by slight and considerable pressure on the nervous mass, whether that pressure be operating within the skull,—on the spinal column, or within the delicate theca of an individual nerve. Yet the most experienced in the Hippocratic art remain stationary upon the same step of inquiry's threshold, and fail, however desirous or indefatigable, of discovering the rationale of this interesting fact in pathological science.

Had the case assumed a more chronic character, and the inflammatory symptoms abated—in other words, had it been a less severe one, the strychnine should have been employed according to the plan recommended by my friend Dr. James Bardsley (see "Hospital Facts and Observations"), to whom I conceive the profession and mankind are indebted for his practical monogragh on the employment of that singular medicine. But to return.

In the above report of post-mortem appearances, it is stated, that a thin layer of coagulated lymph was observed under the *arachnoid membrane* of the spinal marrow. Now as anatomical ingenuity has failed to detect any vessels, however minute, in that delicate tissue, I am inclined to consider that lymph to have been chiefly produced by inflammation of the pia mater and the spinal medulla, the sources also of the limpid effusion within the theca. This point, however (as possessing little practical import), would not have been alluded to, but the peculiar and respected opinions of Ollivier and Ribes on Inflammation of the spinal membranes, particularly of the arachnoid, bestow upon it a reflected interest.

In conclusion, it is an opinion of Janson, as quoted by Ollivier, that paralysis of the limbs, without being attended by contraction of them, depends on inflammation of the *nervous structure alone*, whilst the tonic contraction of the extremities, frequently seen, is to be attributed to the complication of *inflammation of the membranes* along with that of the spinal medulla. Now in Mrs. Wanlon's case, the meningeal inflammation was very considerable, and that of the medulla less so; yet tonic regidity never once presented itself either in the earlier or later stages of the paraplegic affection.

DR. ADDISON.

To the Editor of THE LANCET.

SIR,—Permit me to say a few words in reply to some remarks published in the last number of THE LANCET, under the signature of *Guyensis*, containing insinuations against one of the physicians of Guy's Hospital, which I think, Sir, I can prove to have no other foundation than that of malice and ignorance. It appears that Dr. Addison is in the habit of giving superacetate of lead and opium with great success in hæmorrhage from the lungs; the dose being in the proportion of one grain of the former substance to half a grain of the latter. As these two decompose one another, your Correspondent has attempted to show by a series of most *profound* questions, which he informs his readers have arisen *naturally*, that as the meconate of lead produced is insoluble, the whole advantage, if there be any, must arise from the acetate of morphia. There is no occasion, Sir, for me to answer the queries of your Correspondent in the order he has proposed, for one simple fact will *decompose* them all, namely, the meconate of morphia contained in the half grain of opium is so minute, that at least five-sixths of the superacetate of lead will remain unaltered. I advise Guyensis not again to favour the public with his truly *natural* reflections, unless he can combine a grain of common sense with half a grain of chemical knowledge.

Trusting to your impartiality for the early insertion of this, I remain yours, &c.

A Guy's Student.

April 23d.

FORCIBLE SEPARATION OF THE ENTIRE ARM FROM THE BODY.—RECOVERY.

To the Editor of The Lancet.

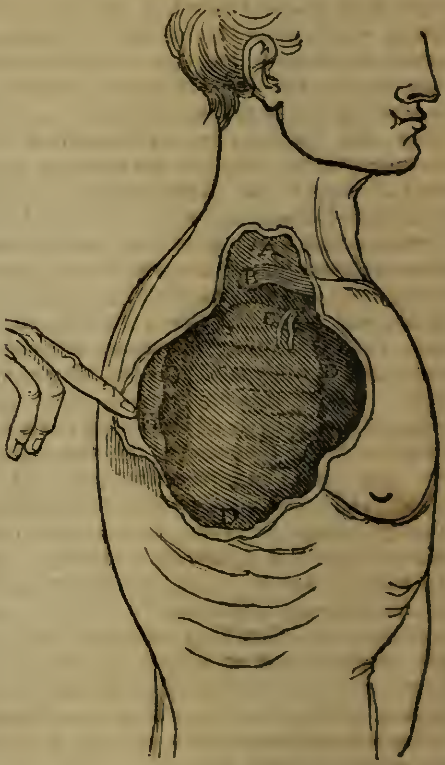
Sir,—An accident of a very serious nature having fallen under my care, I beg to submit to you and your readers the particulars, together with a rough sketch of the parts as they appeared when I first saw the patient.

March 17. George Dawson of Great Clacton, ætat. 13, was in the upper story of a mill, and whilst a sack of meal was being drawn up, incautiously took hold of the chain; his hand was immediately carried over the tiller, and his person lifted about two feet. There not being a space of more than four inches between the tiller and the roof of the mill, the arm was plucked off, and the body fell.

Upon viewing the part it presented a horrible appearance; the ribs were exposed to a considerable extent; the scapula, with all its muscles, was drawn out, leaving behind only the cartilaginous margin, which had separated from its base; the latissimus dorsi, and pectoralis major et minor, were torn asunder about two or more inches from their insertion into the humerus; the deltoid and other muscles were entirely torn away, leaving the clavicle exposed as far as the deltoid had its origin. I removed this portion of the clavicle, which projected like a finger, and, after securing the vessels, covered it with such portion of the integuments as remained, and applied light dressings. The wound is now nearly healed. The only part which sloughed was the portion of integument which covered the top of the shoulder, and consequently sustained a severe bruise between it and the tiller of the mill. His general health suffered very little, he was able to walk out at the end of a fortnight, and the third Sunday went to church. I am, Sir, yours, &c., a constant reader,

S. F. Scarnell.

St. Osyth, Essex, April 10th, 1832.



A. Portion of integument laid back, the only part that sloughed.

B. The projecting portion of clavicle.

C. The subclavian vessels.

D. Torn muscles.

E. Cartilaginous margin of the scapula the integument held back by a finger.

SINGULAR CASE OF

DIAPHRAGMATIC HERNIA.

Communicated by D. O. Edwards, *Esq.*

Mrs. Agnes Pearson, twenty-three years of age, a delicate female, of fine complexion, and exquisite symmetry, had from infancy enjoyed imperfect health. She was subject to frequent attacks of dyspnœa attended with acute pain in the region of the heart. These came on suddenly, without any apparent cause, and went off as unaccountably. They were therefore considered as spasmodic, and treated accordingly. She was married about a year ago, and soon falling pregnant, the attacks of dyspnœa were observed to become more frequent and longer-continued. The gentleman however who was engaged to attend the patient during her confinement was assiduous in his attempts to alleviate these symptoms, and she went on tolerably well to the full period of gestation.

At this time she imagined she felt the labour pains, but as this was her first pregnancy, it is probable she was deceived. On the second day of last March the usual dyspnœa occurred, and became much more severe than before. The customary remedies failed to produce any alleviation. Vomiting and purging succeeded, and was attended with great prostration of strength, and at this time being visited by a cholera hunter, she was regularly returned as a case of that epidemic. Every means of relief resorted to were ineffectual, and she expired about nine hours from the commencement of the paroxysm.

My assiduous and indefatigable friend, Mr. Ellerby Hixon, having become acquainted with the history of the case, was requested by the original attendant to perform a post-mortem examination, and kindly invited me to witness it.

On the day succeeding the death of the patient, Mr. Hixon inspected the body in the presence of Messrs. Davies, Webb, Graham, and Twiss, surgeons, and G. F. Knox.

The body was rather emaciated, and the abdomen as prominent as it is generally at that period of pregnancy. A remarkable lividity was perceptible in the integuments, extending in the shape of a broad band from the left clavicle down to the eighth corresponding rib, and in the form of a circular patch at the scrobiculus cordis.

On raising the sternum my friend was struck with the anomalous appearance of a red highly injected body, filling up the left thoracic cavity. This on examination proved to be *the transverse arch of the colon.* The lung was compressed against the mediastinum, and contained but very little air. Its structure, however, was perfectly healthy.

The intruded viscera were invested with no sac, and were found to consist of the colon as aforesaid, the whole omentum, and the pyloric half of the stomach. The protrusion had been effected through a foramen in the diaphragm a little anterior to the cardia. This foramen was bounded by a tendinous ring, smooth all round, and having no adhesion to the strangulated intestines. On the thoracic aspect of the diaphragm there were in the neighbourhood of this opening some adhesions with the pulmonary pleura evidently of long standing. The incarcerated organs were highly inflamed, of a port-wine colour, and much denser than natural. The hernial portion of the stomach, in particular, afforded a remarkable contrast with that which was unconfined; for the latter was pallid, soft, and *ramollie*, to such a degree, that it was lacerated by the slightest pressure.

There was a large quantity of serous fluid intermixed with flakes of lymph in both thoracic cavities. In the pericardium a few ounces of serum were discovered, which had produced an appearance of maceration on the surface of the heart. Some tubercles were distinguished in the right lung.

The uterus was now opened, and a full-grown fœtus discovered in the involved semitransparent decidua. The decidua uteri, the chorion, and the amnion, were remarkably distinct. The medical attendant stated as a reason for not performing the Cæsarean operation immediately after death, that the fœtus had been some time lifeless.

The hernial ring may from its smoothness and ligamentous character be fairly considered as having existed a long while, if it was not actually congenital, and I concur with Mr. Hixon in the view which he takes of the case. It is this gentleman's opinion that the frequent attacks of dyspnœa sustained by the patient were due to the temporary protrusion of the colon into the thorax, and that the sudden cessation of the symptoms is to be ascribed to the spontaneous evolution of that gut. These eccentric movements were comparatively innocuous, whilst the abdomen was charged only with its ordinary contents; but the gradual growth of the gravid uterus more and more straitened the space allotted for the viscera, and at each incursion into the thorax, a large portion of intestine was expelled and restored to its place with more difficulty, until at length the fatal strangulation took place. It is probable too, that the incipient efforts of labour may have been the exciting cause of the hernia.

The Dublin Journal of Medical and Chemical Science, &c. Dublin: Hodges and Smith. March, 1832. 8vo. pp. 116.

This is the first number of a new journal. It has lain before us without notice longer than we intended, for it contains much valuable matter. We seize an opportunity, however, before the close of the month, to present our readers with an analysis of the original portion of its contents.

FRACTURES IN CHILDREN.

The first paper is a brief but interesting notice by Mr. Hart, of Dublin, of a subject not prominently discussed even in our best surgical treatises,—namely, the partial fracture of the long bones in children. Mr. Hart first offers some explanatory observations on the relative proportions of animal and saline matters in the bones at different periods of life, and he then proceeds to his immediate subject.

> "While the long bones of children are still soft and flexible, they are subject to a kind of injury, the occurrence of which is incompatible with the brittleness of the same organs in adults. This injury is a fracture which extends through a part of the diameter of the bone, the remaining part becoming bent in the manner in which a branch of a tree yields to an attempt to break it while it still retains its sap. It has fallen to my lot to meet with five cases of this injury within the last three years, one of which occurred in the humerus, two in the radius, and two in the femur; and as this kind of fracture is not particularly described in any of our systematic works on surgery, nor in any periodical publication to which I could obtain access, I shall briefly notice the particulars of one case, and conclude this paper with one or two remarks on it."

It is unnecessary to quote the case, as the annexed summary of the diagnostic symptoms of the fracture will sufficiently answer for the practical information of our readers.

> "The diagnostic symptoms of this affection are very simple, they are the following:—Pain and a bent state of the bone injured without absolute shortening of the limb, on the contrary it is lengthened on the side to which the ends of the fractured part of the bone project. By attending to these circumstances it will be always easy to distinguish this injury from ordinary complete fracture."

The first indication in the treatment, Mr. Hart informs us, is to straighten the bent bone. To effect this, he states much care and delicacy of manipulation are required, for if it be rudely attempted with a force too great or too suddenly applied, the part of the bone which was merely bent may be broken, and the fracture rendered complete. As for the next indication, or the preservation of the adjustment of the fractured surfaces, it is of course to be accomplished according to the ordinary mechanical principles and methods of practice.

EXPERIMENTS WITH PRUSSIC ACID.

The second article is an amusing trifle by Mr. Scanlan, on the effects of prussic acid upon the tipula or crane fly, (*vulgo* "Harry longlegs,") by means of which the curious mechanism of its ovipositor was observed. Kirby and Spence state that this insect, when suddenly caught, shoots out its eggs to a distance of ten inches, but the mechanism by which this singular act is accomplished, is not described by these able entomologists. Mr. Scanlan thus solves the riddle.

> "In the month of October, two years ago, when the common tipula or crane fly, is seen flying about in every direction, I caught one of these insects, and held it by the wings over the mouth of a large bottle of medicinal prussic acid which happened to be near me at the time; the tipula, which before had struggled hard for its liberty, immediately on being presented to the prussic acid vapour, became motionless. I now laid it upon a piece of writing paper, in order to examine it with a magnifying glass. After the lapse of a few seconds, I observed a convulsive motion, by means of which a single ovum was thrown from the abdomen into the ovipositor, which is a very curiously-constructed apparatus, at the anal extremity. This convulsion of the abdomen, which somewhat resembled the operation of vomiting, appeared incapable of sending the ovum any further, leaving its total ejection to the ovipositor, which it effected in a most admirable manner.

> "The ovipositor of the tipula very closely resembles the beak of a bird, with both the upper and lower mandible divided longitudinally, each limb of each mandible being articulated and susceptible of motion laterally. The extreme end of the limbs of the inferior mandible, when placed together, form a tube. It was at the commencement of this tube the ovum was lodged. The limbs of the upper mandible now separated themselves, and bent down-

wards at a right angle with the lower one, which they closely embraced, and, with a sudden jerk, shot the ovum through the tube formed by the limbs of the lower mandible. About twenty ova were ejected in this way, and flung to a distance of about four or six inches from the insect. The tipula after some time recovered, and flew away. I repeated this experiment upon another female tipula with the same result."

RECENT CATARRH.

Dr. Corrigan furnishes the next essay—one of much practical value—on "The Treatment of recent Catarrh," an affection for which popular and medical remedies are not a little antithetical. Lady Bountiful cures it with brandy and water; Dr. Ollapod counsels Epsom salts and tartar emetic; the one lauds camphor and hartshorn, the other leeches and bleeds, and sets all the machinery of "antiphlogistic regimen" in operation. To put an end to this unseemly discord is the object of our author. A disciple of the "rational" philosophy, he holds with us, that a disease is not made up of symptoms alone, and that the same symptoms may present themselves in a diversity of morbid conditions.

To prove this difference of condition, the effects of treatment sometimes suffice; pathological anatomy, or physical phenomena, not strictly termed symptoms, afford the required evidence on other occasions, and two of these tests Dr. Corrigan conceives to concur in demonstrating the possibly opposite nature of the catarrh which yields to stimulants, and that which is benefited by a different mode of medication. The stethoscope is the auxiliary which here elicits the truth. Extensive experience in various epidemic catarrhs, has afforded the author the opportunity of observing, that in many cases the stethoscope communicates the information, that the minute bronchial tubes are exempt from inflammation in the slightest degree; in other cases again, it teaches that these tubes are considerably affected. In the first variety it is that stimulants do much actual benefit; in the second they are positively injurious. We extract Dr. Corrigan's observations on the treatment of cases of the former kind, with regard to which he says:—

"It seems to be a matter of little consequence which of the diffusible stimuli is administered, but the preparations of ammonia are perhaps the best, and their efficacy is increased by combining them with the camphorated tincture of opium, which, acting both as a stimulant and an anodyne, is probably better than the simple tincture. The dose of this preparation, to be of use, should however be larger than is generally given. Two or three drachms will form but an average dose for an adult. A stimulant thus administered at bed-time sometimes produces perspiration, but as often only an agreeable glow of heat over the entire surface. In the latter case the relief is as complete as in the former. The cough, which in recent catarrh comes in fits, and is very frequent, although itself but an effect of the irritation of the larynx, becomes in turn a cause to keep up that irritation, and hence it is necessary to allay it. A cough mixture given for this object should contain a large proportion of opium. Camphor mixture, with a proportion of ʒi of the ordinary tr. opii, or a proportionate quantity of the camphorated tr. in ℥viii, will form perhaps one of the best anodyne cough mixtures we can prescribe. With many persons, however, the admixture of camphor in any form, produces most disagreeable nausea. If the surface of the body be well protected, it is not at all requisite, with the treatment here recommended, to confine the patient within doors. We know that great changes in the temperature of air inspired, are borne with impunity as long as the cutaneous surface is protected from atmospheric vicissitudes.

"The species of catarrh confined to the trachea, or what may be called irritative catarrh, disappears under this treatment, much more speedily than when an antiphlogistic or depressing treatment is employed, and relapses are less liable to occur. The patient's strength is also preserved, an object of considerable importance whenever affections of the mucous tissues form the type of prevailing disease."

The author then offers some very judicious comments on the several objections to this plan of treatment, and concludes his instructive paper, with the expression of his warm wishes (in which we fully participate) for the success of the new Irish periodical.

DIABETES MELLITUS.

The fourth article "On the composition of the urine and blood in diabetes mellitus," is from the pen of the editor of the journal, Mr. Kane, professor of chemistry to the Apothecaries' Hall. Though creditable to the author's analytical ingenuity, the essay adds nothing to our positive knowledge regarding the disease, inasmuch as he merely proves that, contrary to a general opinion, the quantities of sugar and urea in the urine are not vicarious, but that the latter principle always is present in its natural quantity. This fact has, however, been long known to many experimentalists, and is noticed in Dr. Elliotson's treatise on diabetes. The process, however, devised by Mr. Kane for the separation of sugar from the urine, is an improvement of much value in organic analysis.

"In the subsequent analyses, the method pursued was as follows:—a given quantity of the urine was desiccated cautiously as long as it lost weight, retaining the temperature constantly below 212°, the residue gave the relative proportions of water and of solid matter in the urine. To determine the quantity of sugar, an excess of solution of acetate of lead was added to the urine, and the precipitate separated by the filter. The lead in the filtered liquor was then thrown down by sulphuretted hydrogen, and the sulphuret of lead having been separated by the filter, the liquor was evaporated on a water bath to dryness, and the residue weighed; in some instances the confused crystalline mass was dissolved in spirit and crystallised, but this part of the process was not found necessary."

The author's experiments on the blood were conducted with the double view of ascertaining the presence or absence of sugar; and, secondly, the relative quantities of water, oxide of iron, and the organised principles. The first object was thus pursued.

"The analyses were conducted so as to determine two points, 1st, the existence or not of sugar in the blood; and 2nd, the relative quantities of water, oxide of iron, and the organised principles, as albumen, fibrine, hematosine, and the phosphuretted fats (cerebrine of Vauquelin).

"In order to find the sugar, if any had existed in the blood, the serum was carefully evaporated to dryness, and the residue digested at a temperature = 100° Fah. in alcohol, sp. gr., 830 for an hour. The alcoholic solution, which was of a very pale yellow colour, was then distilled to one-half, when a few flocculi of the white phosphuretted fat ot Denis were deposited, which were removed by filtration. The clear liquor was then evaporated gradually, and frequently allowed to cool and deposit crystals, until the whole was evaporated to dryness. During these repeated evaporations and refrigerations, successive drops of saline matiers, principally common salt, were obtained, deeply coloured yellow by osmazome, and mixed with a trace of fatty matter; no sugar could be by any means discovered; the saline crystals, however, were coloured yellow by the animal matter, from which it required a second crystallisation to free th-m, and owing to which they yielded by the action of nitric acid traces of oxalic acid; their taste, however, and their purification by re-crystallisation would totally prevent any person from mistaking them for sugar. To estimate the utility of this process, five grains of sugar dissolved in a little water were mixed with a pound of blood previous to coagulation, and were afterwards readily discovered in the serum by following it.

This experiment is almost sufficient evidence of the absence of any appreciable quanity of sugar in the specimen of blood examined. We confess, however, that the generation of oxalic acid by the agency of nitric acid on the known matter procured, taken with the circumstance, that the taste of a very minute portion of sugar would necessarily be much obscured by the saline matters with which it was associated, leaves us still in doubt, whether a modification of the analysis and the employment of a larger quantity of blood might not have led to the detection of the saccharine principle. Mr. Kane also is apparently not unaware, that M. Denis has by no means proved the existence of *osmazome* in human blood, M. Lecanu having in his report to the *Société de Pharmacie* of Paris (Journal de Pharmacie, September, 1831), satisfactorily shown the numerous fallacies of the processes employed by Denis for its detection; neither is M. Denis's general mode of analysis of the blood, which Mr. Kane follows in his estimation of the quantities of albumen and fibrine, perfectly free from sources of error. The albumen especially is liable to be underrated by his mode of proceeding. Nevertheless the process is sufficiently delicate to entitle us to receive Mr. Kane's results as sufficient evidence that the most important proximate principles of the blood suffer no remarkable alteration in quantity, either as a cause or consequence of this fatal disease.

ANEURISM OF THE SUBCLAVIAN.

The fifth paper is one of great interest, containing details of two cases of aneurism, treated by Mr. Porter of the Park-street school. The first was an example of aneurism of the subclavian artery above the clavicle, in a male patient, aged 47. The tumour was situated at the right side of the neck, was of an oval form, its anterior edge resting on the trachea, its posterior extending beyond the edge of the trapezion muscle. Superiorly it reached half-way up the neck. Below it rested upon, and apparently depressed, the clavicle. Its transverse measurement was 5⅜ inches; perpendicular, 3 inches. The ordinary symptoms induced by such a disease were all present and of no unusual character. The general health of the patient was good. From his general history it appeared, that the growth of the aneurism was slow, more than two years having elapsed since its first appearance. From a minute examination of the tumour some days after the patient came under Mr. Porter's care, and when the local symptoms were considerably aggravated, it was found that the pulsation could be felt nearly as far forwards as the anterior edge of the scalenus muscle, and so low in the neck as to come within a finger's breadth of the sternum. The ordinary difficulty and danger of tying the subclavian artery before it reaches the scalenus being thus almost infinitely increased, it was determined that a ligature should be placed on the innominata. Before we proceed to extract the account of the operation, we may mention, that Mr. Porter informs his readers, that in consultation on this case it had been proposed to tie the artery at the distal side of the tumour, but that he had declined it; in the first place, because he conceived that the success of this operation in cases of subclavian aneurism had not been sufficiently established; and, secondly, because he denied that any good which the distal operation could accomplish, had already in all probability been effected by the pressure of the tumour, no arterial pulsation being perceptible beyond the aneurism.

"*The Operation.*—The patient having been placed on a table, his neck supported by pillows, so as to allow his head to hang over them, an incision of about two inches in length was made, commencing at the left sterno-clavicular articulation, and carried transversely to terminate above the edge of the clavicle on the right side. From the left extremity of this incision, a small perpendicular one was made, and the little flap dissected upwards. This incision brought into view the fascia of the neck, which was then divided. The sternal attachment of the right mastoid muscle was then divided on a director passed beneath it. The deep fascia of the neck now exposed, appeared to be of great strength and firmness; it was torn through with a forceps, and then divided on a director to the extent of the first incision. A very small artery was wounded, and immediately secured by ligature. The carotid artery could now be felt pulsating strongly under the sterno-hyoidei and sterno-thyroidei muscles. After some delay, the external edge of the right sterno-hyoideus was raised up, and a director being passed beneath, the sterno-hyoidei and thyroidei, on both sides of the neck, were completely divided. The sheath of the vessels was now opened, and the right carotid exposed, which seemed to have been pushed forward on the trachea by the pressure of the tumour,—the nervus vagus was seen in its usual relative position, lying in front of the subclavian, and the superior angle of the bifurcation of the carotid and subclavian was distinctly observable. The knife was now laid aside, and an attempt made to separate with the finger the attachments of the innominata, in order to pass the needle round it. On the left of this vessel the insulation could be easily effected, and had it been necessary the finger could have been passed down to the aorta. In front it was more difficult, principally by reason of the operator's finger being compressed between the trachea and sternum. The patient was exceedingly unsteady, and could not be prevailed on to be silent for one moment, whilst at every cry that he uttered, the trachea was forcibly pressed against the posterior surface of the sternum. As the operation was proceeded with towards the right of the vessel, the difficulties increased. No limit could be found to the artery; it appeared like an immense pulsating aneurismal sac, rough, and uneven to the touch, and communicating a sensation to the finger, every way unlike that imparted by feeling the vessel at its left side. *Compression on the innominata was now tried, and it stopped the pulsation in the tumour*, showing that the aneurism was one of the subclavian artery, but what the nature of this enormous tumour might be, passing deeply below the clavicle, and behind the ribs, closely connected with the innominata and the portion of the subclavian within the scalenus, did not so clearly appear. It might have been a dilatation of these vessels, caused by the pressure of the tumour, but the circumstance of being enabled to control its pulsation made against that idea, for compression could not be applied to even a quarter of the vessel so dilated. It might be that an aneurism of the aorta existed, distinct and separate from that of the subclavian, and pushed itself upwards in close and immediate connexion with the innominata, but the extraordinary regularity of the circulation every-where but in parts influenced by the subclavian tumour, would scarcely warrant a belief that the arterial system was so extensively engaged. Again, it seemed possible that the aneurism of the subclavian might have taken this extraordinary direction into the chest, and under that idea considerable exertion was made to get beyond it, and reach a healthy part of the artery, nearer to the aorta, in order to throw a ligature round it, but it was impossible; and after the most painful exertions during an hour and a quarter, the operation was of necessity abandoned, the wound dressed with a couple of sutures and adhesive plaster, and he was placed in bed."

The progress of the case after this terrible operation is of a most singular kind, and contrary to all reasonable expectation. In six weeks the tumour had diminished to one quarter of the size it presented on the admission of the patient, the pulsation was scarcely perceptible, he was recovering both feeling and power in the arm, his health was excellent, and altogether he seemed as well as if he had never been the subject of so painful and perilous an operation.

AXILLARY ANEURISM.

The second case was one of axillary aneurism, in a patient advanced in life, and for which the subclavian artery was tied external to the scalenus muscle. The patient was an emaciated, debilitated, man, 63 years old. On his admission into the Meath Hospital, the disease had been but of five weeks' standing; nevertheless, so rapid was the growth of the aneurism, that it had already elevated the clavicle, and penetrated beneath the left pectoral muscle. The fingers of the left hand were curved. (Mr. Porter, who appears to be a very select philologist, terms this condition "*aduncated*!") He complained of great pain in the arm below the insertion of the deltoid muscle. His pulse was tolerable; firm, perceptible, and regular, in both wrists. His appetite was good, and the various other functions as healthily performed as is usual in persons of his time of life.

The case was still encompassed with many and peculiar difficulties, of which Mr. Porter affords a lively description. The advanced age of the patient rendered it probable that he would sink under the operation. The arterial tunics might have become ossified, a condition which would ensure the occurrence of instant hæmorrhage, which in such a situation could not be arrested. His age, again, rendered it likely that the adhesive inflammation essential for the obliteration of the vessel would not run its natural course. Notwithstanding all these difficulties, the magnitude of which every practical surgeon must appreciate; the progress of the tumour was so rapid, it having nearly doubled its size in three days, that by the sanction of a numerous consultation, the operation was proceeded with. As in the first case, it was here proposed to tie the artery on the distal side; but this Mr. Porter "rejected at once, not having any faith in that method in cases of subclavian aneurism." The *presse-artere* was also suggested, and refused on the more rational grounds of its use having, in the author's experience, given rise to some of the most terrific cases of secondary hæmorrhage he had ever seen.

"The patient being laid on the table in such a position, as to allow the light to fall strongly on the situation of the vessel; his neck extended as far as he could bear, and his arm applied closely to his side; an incision of about two inches and a half in length was made, immediately along the superior edge of the clavicle, and terminating internally at a point corresponding with the middle of the scalenus muscle. From the internal extremities of this, a small incision was carried upwards, and the little flap dissected and thrown up. The superficial fascia of the neck was thus exposed, and a vein of about the diameter of a crow-quill was seen running along, close to the edge of the clavicle. This fascia was then divided in a transverse direction above the vein, to the entire extent of the first incision, and a quantity of loose fat and cellular tissue exposed, which was torn through, and pushed towards the external angle of the wound with the handle of the knife, and the nail of the fore-finger. At a considerable depth in the neck, a very strong fascia came into view, beneath which lay a vast number of very minute veins, giving to the membrane a purple colour, and as this fascia rose in the wound and receded again, according to the alternations of inspiration and expiration, it seemed exactly as if the pleura occupied some extraordinary situation in the neck, and was here endangered. This fascia having been cut through, and divided on a director to the entire extent of the wound, a quantity of thick granular fat and small indurated glands was exposed, which embarrassed the operation so much, that some of these glands were of necessity removed. The wound had now been carried so deeply, that in order to explore its bottom, it was requisite to have its edges drawn asunder by small silver spatulas, bent at the extremity. The edge of the knife was scarcely used at all, and the operation prosecuted with its handle, and the nail of the fore-finger.

"At a depth of nearly two inches, the edge of the scalenus came into view, with two large trunks of nerves proceeding from its external margin. To both of these nerves the artery communicated its pulsations, and to the inferior one in so remarkable a manner, that there was no mode of ascertaining whether it was really the vessel or not, except by passing the needle beneath it, making pressure on it, and seeing if such pressure commanded the pulsation in the aneurism. This was tried, and it became evident that the artery was to be sought for at a greater depth. The edge of the scalenus formed an unerring guide, and by following it, and still using the nail of the fore-finger, the artery in a very short time was sufficiently exposed to allow of the needle being introduced beneath it, in a direction from below upwards. Pressure now commanded the aneurism, and the finger was held for a minute on the vessel, in order to ascertain, if possible, the presence of any earthy deposit in its coats, but no such condition could be discovered. The ligature having been passed, some difficulty occurred in tying it, by reason of the depth of the wound, but it was accomplished, and one end of it being cut close to the knot, the other was left hanging from the wound, which was closed with three stitches, supported by a few straps of adhesive plaster. The patient bore this operation, which lasted twenty-five minutes, with great firmness, but towards its termination became very pale, and was carried to his bed much exhausted. He was ordered a draught, containing sixteen drops of the acet. tinct. of opium, in some cinnamon water."

The case proceeded so favourably, that on the 21st day the wound was entirely healed, except at a minute spot where the ligature had hung out. The tumour had diminished to one-half its original size; the coagulum could be felt firm and defined; there was no pulse, or even a thrill perceptible in any artery in the limb beyond it: and thus far the operation had been completely successful.

With this analysis of the leading papers in the first number of our new contemporary, we must conclude for the present. We are happy to perceive an announcement of the second number, with a table of contents of the richest promise. We heartily wish the undertaking success.

Tabular View of the Principal Signs furnished by Auscultation and Percussion, and of their Application to the Diagnosis of the Diseases of the Lungs. By RICHARD TOWNSHEND, M.D., Lecturer on Pathological Anatomy at the Richmond School of Medicine, &c. London: Taylor. 1832.

THIS "tabular view" is exhibited in a chart, which folds into a case occupying about seven inches by four. Such have been the discoveries in modern times with respect to the diagnosis of diseases of the lungs, that we are justified in alleging that the student and young practitioner are here furnished with more accurate indices to diseases of the chest, than in all the volumes which have ever been published upon pulmonary affections, up to the period when Laennec's Treatise on Auscultation and Percussion was first presented to the public. While the principles of all the sciences remain unchanged and unchangeable, how strange, and often how humiliating, are the mutations of fashion! Physicians, almost from the days of Linacre, have reprobated that branch of medicine which is denominated "surgery," as the "*instrumental* branch" of the science — the chirurgical, the manipulating department, — and affecting to despise all mechanical contrivances, the M.D.s, both Dubs and Regulars, have rejected every species of surgical instrument, from the lancet down to that dexterous finger of Sir Astley Cooper which forced its way to the aorta of a suffering being who ended his existence in Guy's Hospital. Laennec, aided by the previous discoveries of Avenbrugger—the immortal Laennec,—has humbled their pride, and told them, that if they would understand the diseases of the organs contained within the human chest, they must learn them only by the eye, the ear, and the touch;—that, in fact, the *stethoscope*, when applied to the organ of *hearing*, shall convert it into an organ of *vision*, enabling the listener to observe, with the clearness of ocular demonstration, the ravages which disease occasionally commits in the very centre of the ribbed-cased cavity of the body.

This "*Tabular View*," exhibiting as it does the principal signs furnished by auscultation and percussion, is an epitome of these indications, as presented in the work of Laennec. It has been drawn up with great care, and the various departments are skilfully arranged; thus rendering it an invaluable map of reference to the practitioner, aud at the same time furnishing the student with the best documentary methods of employing those means by which alone the diseases of the lungs can be detected and understood.

Lithotrity and Lithotomy compared, being an Analytical Examination of the present Method of Treating Stone in the Bladder, with Suggestions for rendering Lithotrity applicable to the Disease in almost all its Stages and Varieties; and Remarks on the General Treatment of Stone and Gravel. By Thomas King, M.D., M.R.C.S., &c. London: Longman. 1832. 8vo. pp. 320, plates.

The contents of the above work may be arranged under three heads: 1st, a description of the genito-urinary apparatus; 2nd, an account of the various modes of operating for the stone with *cutting* instruments; and, 3rd, a description of the manner in which *lithotrity* is performed, and of the instruments applicable to this method of operation. Cases are interspersed through the volume, illustrating the consequences which generally ensue after lithotomy and lithotrity; and the whole is wound up with some valuable remarks on the treatment of calculi.

The work is creditable to Mr. King, as an anatomist and a surgeon; but more particularly so in the former capacity. The anatomical description of the genito-urinary organs, and of the parts concerned in lithotomy, is the best we have seen in any work. The relations between the outlets of the pelvis and the possible diameters of a calculus, are laid down in a very clear manner, and deserve the best attention of operators. Some important anatomical points, also, are brought before the reader, which have escaped the notice of every author who has preceded Mr. King in this department of surgery. The dangers which beset the patient in lithotomy, the numerous and frequently unavoidable sources of failure which may occur to the surgeon, and the difficulty sometimes experienced in conducting even the most favourable cases to a cure, are points upon which Mr. King dwells at some length, and in a very apposite manner. The subject of lithotrity is examined with equal care, and a judicious comparison is drawn between the merits of the rival operations, to the latter of which Mr. King awards a decided superiority. Such is a general sketch of the present volume, which we recommend to the attention of the profession.

THE LANCET.

London, Saturday, April 28th, 1832.

On the first threatening of the danger from CHOLERA we spoke in a voice of serious and emphatic admonition to the public; we warned them not to sleep wrapped in a fancied security; we urged them to bestir themselves in time, to provide their asylums for the sick—their refuges for the destitute, and to administer to the naked and the hungry. By calling on them in this manner to *anticipate* serious evils, we prepared them for the worst of realities, and pointed out that in this state of preparation lay their best chance of immunity from the impending visitation. Knowing as we did the inert character of the mass which was to be moved, and seeing how beset it was by clamorous deluders, we deemed it our duty to point back to the histories of foregone pestilences, that thus either common sense might be awakened, or that, by reference to the known perils of the past, we might rouse a due sense of the possible dangers of the future. How perfectly justifiable was that design the events of the last six weeks have demonstrated. The loudest of the non-medical ranters were silenced, the respectable portion of the profession was convinced of the justice of our opinions, and the reasonableness of our advice; due exertions were every-where made, and such efficient arrangements accomplished, that almost all which man could do *was* done to meet the enemy with a determined and secure front. Are we in error in assuming to ourselves some degree of credit for urging and accelerating the precautionary measures? Looking at the general bearing of the press at the period in question, it is our firm conviction, that had we not assumed the tone we did, not half a dozen cholera hospitals would have been established in London, and crowds of wretched beings would have been left to the streets for their dying couch; official sanitary measures would have been encountered by popular resistance; and the outrages of Hungary would probably have been re-enacted in the streets of the English metropolis. And who could wonder if they had? Or, rather, who does not now wonder that we have been spared such sanguinary scenes? A mercenary, a *trading*-press, goaded the populace on the one hand, whilst on the other, the prejudices of a little crowd of medical men sanctioned the double-dealing of that press, by adopting its pretended opinions, little dreaming how sarcastically they were laughed at by the very individuals who instigated them to the commission of so much folly. Placed as the people were between such stimulants to outrage, and harassed moreover by a new appalling pestilence, we firmly believe that many a benevolent practitioner would have been sacrificed to the blind fury of the multitude, had not *one* influential medical journal dared to stem the torrent,—powerfully aided in its exertions by the labours of the Roman Catholic clergy of the London diocese.

We may be excused for claiming to ourselves in this place no inconsiderable portion of credit for the deportment we observed during the prevalence of the epidemic. We did not make *that* the time for controversy on contagion or *non*-contagion —or disputes as to the novelty or antiquity of the disease; we satisfied ourselves by making the most of our opportunities for observation, and in this employment we were content to listen to all the babble, and niaiseries, and pretended triumphs, of the loquacious orators and copious scribblers, with whom, on the irruption of the cholera, we differed in opinion as to the modes in which it could be extended from place to place. The results of these observations we can now lay before our readers, first recapitulating briefly the opinions we promulgated on the subject.

The first occasion on which our notions of the mode in which the disease progressed were recorded, was in our review of the 1st edition of Mr. Hamilton Bell's work on cholera. In this article (the disease was then at Berlin) we deduced as an inference from all the authentic facts in its history, that it was a new disease, that it was *both epidemic and infectious*, that the nature of the epidemic cause was altogether unknown, and that according to the then statistical features of the malady, propagation by human intercourse was the *most probable* channel by which it had reached the Prussian capital.

In the interval that elapsed between the publication of this article, and the first appearance of the cholera in England, every feature in the disease proclaimed the absolute and direct operation of contagion. With the rapidity of an arrow it crossed by the Spree, and descended the Elbe to Hamburgh. Halting as it were here, it next broke out in one of our *maritime* ports on the coast opposite to that last infected. Here it dwelt for days and weeks, and at last, step by step, and with all the characters of a disease still essentially contagious, it reached Newcastle and the adjacent villages, and lurked in them almost with the demeanour of contagious typhus.

Up to this period we maintain that to all reasonable men, the statistical history of the cholera was such as to permit no other rational conclusion, than that it had reached England from the European continent by human intercourse, and had extended itself in England by that channel alone. Had the disease ceased at that time, *no other conclusion could have been adopted.*

But since that period the statistics of cholera have undergone the most complete alteration. In Great Britain it sprang up in Haddington, in Goole, in Hull, in Ely, in Rickmansworth, and in a dozen other places, which have little or no intercourse with each other, and with the fury of a West Indian hurricane, it has now burst out in Paris, in a few days sweeping off 20,000 victims to its rage. These facts again turned the scale, to show us the operation of the unknown epidemic influence. They prove that infection is no longer the *essential* cause, and even tend to indicate that, for a time, its operation may be materially suspended. Such is the sole inference which an unprejudiced mind, anxious only for the truth, can derive from the contemplation of these events. If the operation of contagion was palpable in the transit of cholera from Berlin to Sunderland, the influence of some other cause is equally proved in the subsequent irruption of the disorder. The man who makes this inference changes no " opinions." Opinions are of a double kind, those founded on facts and reasoning, and those hazarded without a knowledge of either; rational opinions must undergo modification with the facts they flow from.

Even in the localities occupied by the disease, the most rigid scrutiny often fails to trace the operation of infection. In one district of London, thirteen families were, to our own knowledge, infected; but the first case having occurred in each without any appreciable infective cause, the other cases are inconclusive, as they might have originated in the unknown source by which the first was occasioned. This position is as clear as the construction of a common syllogism.

Again, we find from records in our possession, that ninety-three persons were ill of cholera, and that in the same district, on an average, seven persons, principally of the same families, and subjected to all the same external causes, were exposed to each of these invalids without contracting cholera. The linen and bed-clothes of over sixty patients were cleansed by thirty-nine washerwomen, one only of whom contracted the disease, and she resided between two houses, at a few yards distance on either side, and in both of which cases had occurred without assignable cause. Now we cannot force our belief, that mental operation so perfectly involuntary in its character, to the conclusion, that of those 689 exposed persons, more than one would not have caught the disease if *at the time* it had been ordinarily capable of originating from human emanations. The question, however, still remains open here, Did this immunity depend on the non-existence of the contagious poison, or on the existence of some unknown cause, acting as an antidote to that agent? We incline to the latter belief, and further to the impression that *the presence of an epidemic cause* is, to a certain extent, a protection against the human poison, or, in other words, that it is principally in places *free from the epidemic* that the disease can, under ordinary exposure, spread from person to person. That this is not an utterly vain speculation the following facts will sufficiently establish.

That the human body *can* generate the causes of some diseases under which it labours, and that these causes can create the same malady in other persons without actual contact, is proved as a general rule by the history of typhus fever. To the London and Queensberry Fever Hospitals we appeal for incontrovertible proofs in this particular instance.

That the cholera patient can also generate the causes of the same disease is proved by numerous facts already quoted in our pages. Of these, the strongest ever brought forward in any work since the question was agitated, is the set of cases published by Mr. PREBBLE in our 450th number, in which it is established, that a gentleman contracted the premonitory symptoms of cholera in London, that he travelled to East Hendred, where he suffered a violent attack, that his mother, who nursed him, was immediately afterwards attacked, and died in twenty-three hours; and that a second nurse was attacked three days subsequently, and died in fifteen hours and a half. *No case of cholera occurred before, or subsequently, in this locality.* Here then is the operation of infection mathematically proved.

Now as the same cause always under similar circumstances produces the same effects, we are warranted in believing, that the human body always gives out the morbid poison, but that the effects of this are prevented by some inexplicable cause, most probably *the remote cause of the epidemic.* This counteracting power is not apparently confined to cholera. It is a well-authenticated fact, that during *epidemic* small-pox, cases of infection are rare; and that inoculation frequently fails in a large proportion of cases to *communicate* the poison. *But if you take the person out of the sphere of the epidemic, the inoculation will succeed.* (*Vide* Haygarth *passim*, and a memoir by M. Guillon. *Bulletin de Sciences Medicales*, April, 1831.)

At another time we may return to this subject, meanwhile we leave it with the impression, that no other view of the causes of the disease is so well calculated to recile conflicting opinions, and put an end to the controversies the question has so long excited.

THE decline of the CHOLERA in London has, we are sorry to observe, already produced on the public mind an effect which may be attended with disastrous consequences. Many of the hospitals which were established with such difficulty, are now in course of appropriation to other purposes. The local boards are, in some parishes, breaking up, and filth and crowding and nuisances are again quickly reasserting their ancient supremacy. But we beseech the inhabitants of this metropolis not to be too soon allured into an opinion of *security* which may prove fatal. Since the publication of our last Number, *the cholera has, after six weeks' absence, broken out a second time in Halle, Prague, and Breslau,* and in the first of these towns its present ravages are fourfold more violent than on its previous visitation.

Let us then advise the parochial authorities to preserve their hospitals at least six months longer. They have now acquired some little experience in the matter, and that experience should now be turned to the maintenance of the most efficient preparations against the possible—and *we* say *probable* —recurrence of the malady. It is earnestly to be hoped that we shall not see in London—as has recently happened in Paris—the wards of our general hospitals crowded with a sudden influx of patients, whilst, in many cases, there was a total absence of medical attendance during twenty-four hours! With all their rancour and folly, even the non-contagionists having any pretension to character, have not ventured to assert that the cholera hospitals have proved useless establishments. All circumstances considered, therefore, would it not betray an extreme want of prudence to disturb those arrangements already so effectual in preserving the public health, and for the active operation of which there may come, too speedily, a more urgent demand than ever? Had it not been for the precautionary measures so zealously pursued in several departments of this metropolis, there is no estimating the extent of that mortality which the pestilence might have inflicted. If we were to admit, with the clamorous and senseless class of disputants, that the disease is *not* communicable from man to man, is it of no importance that the wretched and comfortless poor should be rescued, in the moment of attack, from their cheerless abodes of poverty and filth, and be deposited in an asylum affording skilful medical attendance, and all the necessaries required in the hour of peril? Viewing the question in this light, mismanagement, or rather improvidence, more culpable cannot be conceived than to dismantle, or convert to other uses, those buildings which have been recently opened for the reception of the sick poor. The expenses attendant on furnishing and fitting up having already been incurred, the additional burden imposed on the parishes by supporting them in their present condition during the next six or twelve months would amount to little more than the rent. Any argument, however, which we could employ to enforce the propriety of such a procedure would fall infinitely short of the effect which must be produced upon every rational mind by the simple exclamation—Behold Paris!

M. Cassimer Broussais, the celebrated Parisian physiologist, has just delivered two lectures on cholera at the Val de Grace, wherein he professes to be a believer in the contagious nature of that disease, an admission which has astonished, not a little, the sceptic *savans* of the French capital.

The *cholera has made its appearance at Havre*! Heaven grant it may not extend to our own shores from this new direction!

We regret to learn that the cholera has begun to rage with great violence in Ireland. The following is a numerical statement of the cases and deaths which had occurred up to the close of the accounts received in London as we went to press.

IRISH REPORT,

April 23.

	Rem. last Rep.	New Cases.	Died.	Recov.	Remain.	Total Cases.	Deaths,
Dublin, 23d	49	50	18	5	76	197	95
Arklow, 21st	1	5	2	0	4	14	9
Cork, 21st	58	44	13	21	68		
Cork, 22d	68	56	17	9	98	232	83
Clondalkin, 22d	0	2	0	0	2	3	1
Ringsend, 23d	0	1	1	0	0	2	2

ROYAL INSTITUTION OF GREAT BRITAIN.

STRUCTURE AND FUNCTIONS OF THE INFUSORIA.

On the 6th instant Mr. Thomas Bell read a paper on the structure and functions of the *infusoria*, a class of animalcula discovered with the aid of powerful lenses in animal and vegetable infusions. Until very recently the most perfect microscopes were incapable of showing more of these minute beings than their mere existence. They were supposed to have no proper form, and their spherical shape was said to be due to the pressure of the surrounding medium. These creatures were first discovered by Lewenhoeck. His researches were ingeniously followed by La Marque and his followers, who, with singular daring, endeavoured to apply the new facts to the construction of a scheme of necessity in the organization of living beings. These vivacious dots or monads were supposed to be the ultimate elements of animate bodies, and to supply by their aggregation the materials of every form of life. The late discoveries of the German physiologists have effected a signal overthrow of these doctrines, for these apparently indivisible points have by means of improved instruments been proved to possess a complete organization, as difficult to be accounted for by the hypothesis of necessity as the human frame itself. This end was obtained by the simple expedient of feeding the animalcules with coloured liquids. A solution of pure indigo is the most eligible fluid, and great caution is requisite to prevent the introduction of any metallic or medicinal pigment, for their destruction would be the immediate consequence, and the desired results could not be obtained. These precautions taken, a variety of appearance was observed in the hitherto seemingly homogeneous globules. In some, numerous spots become evident; these are the digestive cavities or stomach; others present a different character, and one digestive cavity alone is exhibited. The observer succeeded in detecting a mouth and an anus, and the rudiments even of a nervous system were perceived. The first and most simple class is named *polygestrica*, from the great number of stomachs; the second and more complicate is denominated *rotatoria*, from the nature of their movements. Relating to the functions of this second class, numerous facts of great interest were ascertained. They were found to exhibit all the principles of reproduction exemplified in other animals. When divided, each part, as in the planaria genus, became a perfect animal. The propagation by germ or bud was also frequently exemplified, and their ovicula had been distinctly demonstrated. The interest of these researches will be enhanced, when it is stated, that these objects are so minute, that the space occupied by a single drop of distilled water is adequate to contain no less than five hundred millions, and that a cubic inch would include *eight hundred thousand millions*, a number a thousand times greater than that representing the population of the globe. Mr. Bell appended some remarks on the tendency which the rash explication of natural phenomena, by a principle of necessity, especially in animated nature, has to debase the character of philosophy, and to deprive the mind of that exalted pleasure which it derives from the contemplation of infinite design.

CONTAGIOUSNESS OF CHOLERA.

REPLY TO SOME STATEMENTS MADE BY DR. GILKREST.

To the Editor of The Lancet.

Sir,—At the Westminster Medical Society, April 14th, Dr. Gilkrest stated,

> "That he had lately witnessed in Limehouse, some circumstances which gave him additional confidence in his previous opinions. At a house in Tile-yard, a man, called John Coffin (Cocklin), died of cholera, and the next day a girl in the same house also fell a victim. Being Irish, of course a "waking" took place. The bodies were put on a table with merely a sheet over them, and for several days were continually surrounded by the friends and relations. While ill, Coffin had been visited by his sister, brother-in-law, and several others; the girl also had been seen repeatedly by her father, mother, and friends, and the medical men had rubbed the body, but of the whole circle of these persons not one had taken the disease, although the circumstances spoken of occurred a week ago. Now surely this could not be called a contagious disorder."

I think Dr. Gilkrest should first have stated, that Cocklin had attended the wake of the first case, in White's Rents, where he also lived, but removed to Tile-yard to escape the disease; and that on the 21st of February he was attacked and *died* on the 25th. The girl, Julia Donnie, did not die the day after Cocklin, but was in and out of the room constantly where he lay, and slept in an adjoining apartment. She was attacked on February the 24th, at eight o'clock a. m., and *died* at eight p. m., *previous* to the death of Cocklin. The wake only continued two days. Cocklin's sister visited him, and was attacked with severe diarrhœa. Mrs. Mayhoney, living in Poplar, who took an active part in rubbing the girl Donnie, was attacked with the disease, and *died*. Her daughter, Judith Carter, living in Limehouse-fields, a distance of half a mile, came to see her, was attacked on the 3rd March, and *died* on the 6th. Mrs. Mayhoney's niece, who attended on her, was attacked and removed to Poplar Hospital; another niece also, who was allowed to attend on her sister in the hospital, and who also attended on her aunt, was attacked, and both of them, after going into a collapse, and having a very hard struggle, recovered.

It was stated, I understand, to Dr. Gilkrest, that the medical gentleman who accompanied him to the house of Cocklin, and who was one of those who were rubbing the girl Donnie, was then labouring under diarrhœa, and the two following days was so ill that he could not leave his chamber.

The above circumstances are detailed as particularly as this space will admit. They will be seen to be very contrary to those stated by Dr. Gilkrest. At the same time let it be clearly understood, that my object is, not to contend for or against contagion, but that truth may be laid before the medical world, and that no one, whose opinion is already formed, may lay before others incorrect statements.

I am, Sir, your obedient Servant,
VERITAS.*

WESTMINSTER MEDICAL SOCIETY.

To the Editor of THE LANCET.

SIR,—Permit me to correct a mistake in the last LANCET's report of what fell from my lips at the Westminster Medical Society on Saturday se'nnight.

I did *not* allege, as that report states, "That the *Dover* was standing off Limehouse, with vessels close alongside her, and not one case had yet occurred in them," and for this very obvious reason, that I knew to the direct contrary of both these assertions,—there being no vessels alongside H. M. S. *Dover*, and one case having occurred in the tier immediately below the spot occupied by her.

What I did affirm was this. Although H. M. S. *Dover* had lain so long a time in the river with cholera patients on board, the wind blowing from every point of the compass, and her boats and officers and crew, passing and re-passing in all directions, for the greater part of that time, no case of cholera had occurred in the tier either above or below us, or in the neighbourhood directly abreast, *until very recently*, when one patient was sent from a vessel in the next tier below us, and assistance was requested for a boy who had worked in the manufactory that overlooks us from the shore.

As the error which I wish to correct involves a matter of fact, and upon a subject in which no doubt you desire only to record statements that are true, it may not be expecting too much to request you, in any way you please, to alter your reporter's record of what I did not say, to an account of what I did say.

I am also reported to have made mention of the fatigue undergone by the nurses and medical attendants in this institution. Had the gentleman's words who preceded me been taken down also, it would have been seen that I only so spoke in answer to the gratuitous assumption that we have had nothing to do one-half of our time; and this too in part explanation (!) of the curious and interesting fact, that a large majority of those resident on board, and in attendance on the patients, were attacked with the premonitory symptoms of cholera morbus, and some with diarrhœa.

I know the respect due to the Westminster Medical Society, and permit me to say, due also to my own character, too well to bring forward in public any account of the manner in which duty is done, when I must be myself included in the number of those spoken of—unless, as in this instance, for the purpose of demolishing a theory which had need of misrepresentation to support it, although I believed that misrepresentation to be innocent.

I have the honour to be, Sir,
Your very humble Servant,
WILLIAM BARRETT MARSHALL,
Assist. Surgeon, R.N.
M. M. S. *Dover*, River Thames,
April 16, 1832.

REPLY TO THE COMPLAINT AGAINST DR. SIGMOND.

To the Editor of THE LANCET.

SIR,—The assertion made by one of your correspondents, calling himself a Pupil of the Little Windmill-street School, that I have not lectured for some weeks on the Materia Medica, is not true. In compliance with the wish of the gentlemen forming the class, I changed the day and hour of lecturing to enable them to attend the Course delivered at the Royal College of Surgeons.

Having within the last week received from my pupils the most flattering testimony of their kind feeling towards me, accompanied with the most marked expression of their satisfaction, I am the more surprised at the letter which you have inserted. I have only further to observe, that as I have at all times received at my own house any pupil who has been prevented attending my public lectures, it would have been more creditable to the writer had he personally applied to me, instead of anonymously seeking redress through the channel of your columns.

I have the honour to be, Sir,
Your obedient servant,
G. G. SIGMOND.
24, Dover-street, April 20th, 1832.

CORRESPONDENTS, &c.

COMMUNICATIONS have been received from J. F.—Mr. Mart—Mr. Gurney—C. E.—A Subscriber (Mr. B.)—Dr. England (some weeks since)—Amor Mercurii—Mr. J. Walker—Mr. W. Wilson—Dr. Whitelaw Ainslie—Mr. Marshall—Mr. W. Birtwhistle—Mr. S. Millard—A Naval Surgeon.

A Surgeon's Apprentice. With the qualification mentioned, he *can* practise, either in the Canadas or the United States.

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* This signature has been properly authenticated.—ED. L.

THE LANCET.

Vol. II.] LONDON, SATURDAY, MAY 5. [1831-32.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE

IN THE UNIVERSITY OF LONDON,

Delivered March 12th, 1832.

DISEASE OF THE EXTERNAL SURFACE OF THE LIVER, STOMACH, INTESTINES, &C.

Absence of all Symptoms of the Disease, up nearly to the period of Death.

The first case of which I will speak, Gentlemen, is that of John Milliman, aged 36, admitted February the 19th. He was a short, slim, pale, delicate man, and said, that about three years before, he had fallen down the steps of St. Bride's church, and been taken up senseless. Since then he had complained of occasional pain in the cervical vertebræ,—sometimes down the whole spine, but especially in the cervical vertebræ, and he said that those pains were increased by a change of weather. He represented himself as having been under medical treatment for three months after his fall, and he had also been an out-patient of this hospital. He said that his general health was good, and that he had no other complaint.

I could discover nothing the matter with him, except that the spine about the cervical vertebræ was rather tender. There was no protuberance nór heat, but on rubbing the fingers up and down the cervical vertebræ, there was some little degree of uneasiness experienced by him. He directed my attention to no other part of the body. The only complaint he made was of pain in the neck. With this exception he said he was perfectly well. I did not satisfy myself that there was anything the matter with the cervical vertebræ. It is easy for a person to say that he has pain in any particular situation, and on placing your hand upon any part, it is likewise very easy for him to say it is tender. There was no swelling nor redness; no heat, and he could move himself backwards and forwards, and turn his head round very well. If there was any pain at all, it appeared to me to be *merely* a pain,—that is to say, a sort of neuralgic affection.

Treatment.—A blister was placed over the cervical vertebræ, and he took two drachms of the carbonate of iron three times a day. This was on the 10th of February, and the doses were increased up to the 28th. The medicine was given more frequently, but he was no better. Then on the 29th he said he was much better, and that he had but little pain. At that time, however, his symptoms all at once changed, or, rather, very evident symptoms presented themselves, although he said he was so much better, and had lost all his pain in the back of the neck. He was observed to speak incoherently. He was observed likewise to have become exceedingly thin, and his pulse was very weak. He had been allowed full diet the whole of the time, and now a pint of porter was added to it. In two or three days, that is to say on the 5th of March, he wandered still more in his head. He was observed to keep his bed from weakness, and had become very much emaciated. He then for the first time complained of something somewhere else, but had made no complaint till I was very much struck with his altered appearance, and determined on making an examination of every part of the body. Nothing was found the matter with the chest, but on examining the abdomen, I found that most likely the cause of his sudden emaciation was situated there. I found the abdomen full, very full, and rather firm, and on pressing him in various parts of it, I found he complained of tenderness at the lower part. In this I am sure there was no deception, because he was incoherent, and if he had been disposed to deceive me originally with respect to his neck, being now incoherent, there was no chance of his attempting deception. With respect to the abdomen, he complained of pain regularly when pressed on the lower part. The circumstance of the abdomen being large, rather confirmed my conviction that there was real pain situated there. He then had an attack of diarrhœa, and passed his fæces in bed, and his pulse became very small and rapid. I ordered a blister to be placed on the abdomen, for he was altogether too weak to allow of any loss of blood. Two grains of opium were given him immediately, together with an infusion of catechu, and the diarrhœa was stopped, but the next day his pulse was scarcely perceptible, and while I was in the ward he died.

Post-mortem Examination.—On examining the spine, no diseased appearance was found there, nor in the spinal marrow. The head appeared in a natural state, and the chest the same, but in the abdomen very great disease was found, and it must have been of long standing. Various parts of the cellular membrane under the peritoneum were found beset with innumerable granular bodies,—tubercles, as many persons would call them. The omentum appeared to contain a very large number of these, and in some parts it was very thick, on account of their deposition. The intestines were all glued together, and the omentum with them. On the right side of the abdomen, just between some parts of the intestines where complete adhesion had not taken place, a canal was formed half a foot in length. At the bottom of it the adhesions were still less, so that there was a great cavity or sack produced, but what this was on first opening the abdomen, it was impossible to say. On a more minute examination, however, by Mr. Nordblad, after the part had been removed, it appeared that there was not a real canal, but only a deficiency of adhesion between some portions of the intestines. All the rest of the intestine, the omentum, the parietes of the peritoneum, and the stomach, had been glued together into one mass, so that scarcely any abdominal cavity existed. The intestines thus adhered above, but not downwards, in consequence of which a canal was formed, and from the great want of adhesion at the bottom, there was also a large cavity. The canal was lined with a coagulum of blood exceedingly fine and thin, such as you see if blood escapes and is poured out over a large surface when it is fluid. The coagulum appeared in sheets, as it were. At the bottom of the large cavity to which the canal led, we found a quantity of blood partly fluid and partly in a state of coagulation,—a common coagulum of blood.

I do not know whether I can show you the parts very well now, but I will endeavour to do so. You observe that the omentum is very much thickened by a great deposit of granular substance between the layers of the peritoneum, of which you know the omentum is composed. If you look on each side of the omentum, you will observe the deposition between. Then you see under the peritoneal coat of the intestines the same granular deposition, and here between the two intestines, where the adhesion is not complete, there is a large cavity opening into a large sac at the bottom. This sac is not a new formation, but arises from the adhesion being imperfect. It really presents a very curious appearance; while all other portions of the viscera were glued together, these portions were not, and hence has arisen the large cavity, which of course we could make nothing of at the autopsy. Here is a portion of the peritoneum, and under it you see the deposition of this new substance. No doubt the deposition has taken place in the subserous cellular membrane. Morbid changes for the most part take place not in the serous membrane, but in the cellular substance underneath. I did not know at first whether this cavity was an aneurism, and it was impossible to know until the parts were minutely dissected. There are several tubercles bearing exactly the appearance that we see in the lungs. You now have the real state of the disease well enough shown. On examining the liver, under the peritoneal coat—not in it, but between the liver and the peritoneum that invests it, you find the same deposition. Here is the diaphragm adhering to the liver in every part, so that it was coherent. Here is the peritoneal coat, and here is the tubercular deposition. In order to illustrate the extent of the adhesion, I may show you the stomach, which you observe is adherent to the liver all around, forming one mass with it, and it adheres to the abdominal muscles in front, so that there was complete cohesion of every part. Here is the omentum adherent to the stomach. The gall-bladder is also adherent, so that you perceive the whole is united into one mass.

Remarks.—One cannot but wonder, in the first place, that such an immense mass of disease as this should have existed, and yet the man not make any complaint, nor any of the functions of the bowels be impaired. He was admitted on the 9th of February, and died on the 6th of March, and until just before his death he made no complaint whatever, except of uneasiness at the back of the neck, and sometimes down the spine. He ate well, slept well, and looked well. We saw that he was a slight, delicate, man, but he made no complaint of any sort, and his countenance, habit, and manner, led me to suppose that there was no complaint of any sort existing. As I have already stated, I found nothing the matter with his chest when I examined it, and the disease of his abdomen appeared in itself of so slight a character, that I should not have discovered that he had this extensive disease at all, but being at a loss to explain some of the symptoms which did at last appear, and observing that he became emaciated and very weak, I was of course aware that there was some reason for what had taken place, and I determined, as I before said, to make a general examination of the body, and then I fixed upon the abdomen as the seat of the disease whatever it might be. Gentlemen who were present will recollect that I said at his bed-side, " Here is the disease which has occasioned this emaciation, and I presume, from the abdomen being rather enlarged, and hard, that we shall find disease of the mesenteric glands," and Mr. Nordblad tells me that those glands were in a state of disease. Here they are, and you see that they are not only diseased, but that some of them are actually in a state of suppuration, so that on squeezing them pus comes out. Some are very much enlarged, and in a state of scrofulous disease. Here is one particularly so. When this new set of symptoms appeared, accompanied by the debility, I gave the diagnosis that there was disease no doubt of the mesenteric glands, but I could not feel any organ enlarged. The liver and spleen did not appear to be enlarged, neither was there fluctuation. Therefore it was not effusion, and the man's bowels being regularly opened every day, and having indeed at that moment diarrhœa, it was evident that there was no accumulation in the intestines. It was that circumstance which led me to give the diagnosis which I have stated.

The reason why the disease existed so long without the patient suffering at all,—without his making any complaint, was that the internal surface of these organs was all sound. The inner surface of the stomach and intestines, as far as they were examined, and the substance of the liver, were all healthy. It was merely a disease of the outside of the different organs, and that is a sufficient explanation of the absence of symptoms, and of his general health not being materially affected.

But you will now ask me,—What was the reason of his suddenly becoming pale, emaciated, weak, and expiring so suddenly? I do not know that I can give you a satisfactory answer to this question; but when I consider that amidst those adhesions in the cavity which existed between them, a quantity of blood was found, I imagine that a degree of inflammation must have suddenly taken place along the sides of those portions which did not cohere—that a sudden degree of inflammation took place, attended with an effusion of blood. You saw a sheet of blood existing all down the false canal, and in the sac itself at the bottom was a quantity of coagulated blood. In all probability the change that took place in the midst of the adhesions caused an effusion of blood into the part, and this and the other morbid changes which occurred, were, I should think, from the examination that has been made, the cause to which we must attribute his sudden sinking. There is no suppuration to account for it; the mesenteric glands it is true are suppurated, but very slightly, and there is no part under the peritoneum in a state of suppuration, therefore it could not arise from this source. It is possible either that the changes which took place in this artificial cavity killed him, or the disease existing in the mesenteric glands, might have become so considerable that the chyle became completely obstructed, but whether that is the case or not I do not know, because it is rare even when there is great enlargement of the mesenteric glands, that you have an impediment to the course of the chyle.

The case is rendered very remarkable, from the long absence of the appearance of any ill effects, notwithstanding the disease must clearly have been of considerable duration. But I think that circumstance is sufficiently explained by the disease being situated outside the viscera. The reason of the sudden ill effects which were produced, I will not take upon me to explain very satisfactorily.

CASE OF CHRONIC RHEUMATISM, WITH OCCASIONAL ACUTE ATTACKS,

Treated alternately with Arsenic and Colchicum.—Immediate and ultimate Effects of Arsenic as a Medicine, &c.

The next case of which I will speak is one of chronic rheumatism, which was in William's Ward, and the man was there nine or ten months. It was a very interesting case on many accounts. His name was Samuel Bowman. He was thirty-nine years of age, and was admitted on the 2nd of June last, and then he had been ill three years.

Now his was a very severe form of rheumatism. It had rendered his hands nearly useless, as well as his feet. At the time of his admission his fingers were distorted; some were bent, and some were inflexible. He had very little power over them. Their figure was altered, and there was enlargement of different parts. It was a case of rheumatism which had produced considerable thickening of the ligaments, and more or less contraction of the tendons. His feet also were so much distorted that he could not walk. He was neither better nor worse, he said, from different temperatures. Cold did not make him worse, nor did it make him better, but he told me that his hands and feet were more frequently cold than hot. Their temperature was more frequently below what it ought to be than at the natural degree.

Treatment.—Under these circumstances I determined on giving him arsenic. In chronic rheumatism, where there is no morbid heat at all, nothing inflammatory to be discovered,—where heat does not increase the pain, and where you find the joints are stiff, arsenic is one of the best things you can employ. Now this man took arsenic from the 2nd of June up to the time of his presentation last week; that is, for nine months regularly, with the exception of certain intervals when acute rheumatism came on.

The case illustrates another fact common to all chronic diseases, viz., that notwithstanding the existence of a chronic affection, every now and then an acute attack supervenes upon it. It is very common in chronic affections of all kinds, for inflammation to take place at intervals, and the inflammation must be treated in the usual way, or you will do no good; but as there is a chronic affection existing, you must be on your guard, lest you should too much impair the patient's general strength.

Now this man several times during his stay here had a great degree of pain in his hands and feet, and this was rendered worse by the application of heat. He began to sweat, and his pulse became quick. These attacks were not confined to the parts affected with chronic rheumatism, but extending to the head, hips, shoulders, and various other parts of the body. Under these attacks I always left off the arsenic, and gave him colchicum. He took that medicine in the usual way till he was purged by it, and then, after he was purged, the dose of the medicine was diminished, and when the acute attack was got the better of, he resumed the arsenic again. These attacks occurred two or three times during the nine months that he was staying in the hospital. They were not at all referable to the arsenic; because they will occur in chronic rheumatism where persons are not taking that medicine, and not only in chronic rheumatism in general, but in that atonic kind which requires stimulants, and is attended by mere uneasiness of the part. As soon as the attacks were over he always resumed the arsenic, and under its exhibition his fingers gradually became more useful to him, and so did his feet, until at last he could walk with the assistance of another person. Such distortion, however, was produced, that he will never be able to use his hands or feet perfectly; that is out of the question, and he must remain a cripple more or less, no doubt, for life. But still his fingers and feet became more useful to him than before, his pains ceased. The size of the swollen part was diminished, and he acquired more power over them than he had when he came in.

Now this was a long time for a man to take arsenic, and I was obliged to omit it from time to time, not merely on account of the acute attacks of rheumatism, but also on account of its producing its common constitutional effects.

The effects of arsenic are twofold,—*immediately* upon the stomach, and *ultimately* upon the constitution; and it is the same with many other acrid medicines. When giving these acrid medicines, you must always be careful in watching for their effects. The local effects of arsenic are, heat in the region of the stomach—a sensation of burning and smarting there—tenderness on pressure, together with nausea and sometimes vomiting. Arsenic has a peculiar power of inducing gastritis, and the symptoms arising from inflammation of the stomach more or less active,—more or less in degree.

But all acrid medicines have the same effect. If you give nitrate of silver, you will observe the same. If you give iodine, you will observe the same, and so on with regard to other medicines of the class of acrids.

But independently of arsenic being acrid in the stomach, it has a specific power of inducing gastritis, if not enteritis. I have known this produced by arsenic when it was applied to a distant part of the body. Pains have come on at the pit of the stomach, tenderness on pressure, and vomiting, I will not say to the same degree, but in the same way that gastritis takes place if a person swallows arsenic as a poison, and, therefore, whenever you are exhibiting it, you should regularly at every visit (and you should see the patient pretty frequently) ask if there be any heat at the pit of the stomach, any heat up the throat, and you should feel whether there is any tenderness on pressure. It is right that you should ascertain this yourself. If it so happens that you cannot see the patient very often, you ought to tell him what effect the medicine may produce, so that he may be fully apprised of the period when it is exerting an effect which would be injurious. It is your duty, if you do not tell him what medicine he is taking, to inform him that he is taking something which may produce an unpleasant effect on the stomach; and you should tell him to press upon it, and see if there be pain.

But it also produces constitutional effects, which may make it necessary for you to desist from exhibiting it. In this man those effects were very remarkable: in the first place it has the property of inducing inflammation of the eyes, and this man regularly after resuming the arsenic, and taking it for two, three, and four weeks, had inflammation of the eyes come on. This did not occur once or twice, but a dozen times during the period he was here. According to the dose he was taking, you saw that his eyes were red and filled with water. When I first saw that take place, I immediately omitted the medicine, but after it had occurred two or three times, I continued it for a few days longer, and did not leave it off until the inflammation became a little greater. As long as he said that his eyes were not sorer than at the last visit, I continued the medicine, but when he said they were getting sorer, then I omitted it. Arsenic, too, in many people, produces great heat, and possibly inflammation, of the throat, uvula, and soft palate; this is a very common circumstance: it will also induce heat of the fingers and redness.

It also occasions œdema of different parts. In this man this constitutional effect was produced, for his legs were swollen very frequently, or one arm was swelled, or his face. The face, particularly, is disposed to swell. This man's face was several times swelled, but at the last time his legs became œdematous. On leaving off the arsenic for a short time, the inflammation of the eyes always went down, and the œdema of the legs declined, and when he went out, were diminished very much.

The effect of the arsenic in this man was very satisfactory. It did what no other medicine would have done; but I should have thought it quite useless to have gone on with it, or to have kept him any longer in the hospital, because I conceive that the state of the parts was such as to be, in some measure, beyond the influence of medicine. But he went out much better, and was very grateful for the benefit he had received.

I do not know that arsenic is used in chronic rheumatism as much as it deserves. You will, however, find many writers recommend it in those particular forms of rheumatism where the parts are stiff and enlarged, and not in a state of heat, but, on the contrary, cold and relieved by heat, or, at any rate, where a hot temperature is not uncomfortable. You will find that Dr. Bardsley of Manchester, father I believe of the present gentleman of that name, published in its favour; and in the *Edinburgh Journal* there are some papers by Mr. Hill, also recommendatory of it. It was from the perusal of those papers that I first thought it would be worth while to employ it in this particular complaint. In acute rheumatism it does no good, nor will it do good in rheumatism of a decidedly inflammatory character; and, on the other hand, I never saw colchicum do good in rheumatism, when the parts were not hotter than they should be. Now and then it certainly may appear to do good, because sometimes the most useless medicines will appear to be beneficial, the patient in fact getting well in spite of all the bad practice that is adopted. But I never saw anything that would lead me to imagine that colchicum did good in rheumatism that was not attended by morbid heat of the parts, or in which an increased temperature did not do harm. When the parts are hotter than they should be, or heat increases the pain, then I see colchicum do good; but when the parts are cold, I never saw any benefit arise from it. When, however, there is stiffness of the joints, especially of the smaller joints, and coldness, and when warmth relieves, then I believe arsenic to be a very excellent medicine. The dose may be from 3 or 4 to 12 or 14 minims three times a-day.

Curious Case of Poisoning with Arsenic.

It is this peculiar effect of arsenic on the eyes which made me suspect the other day that a whole family had been poisoned with arsenic. I was consulted on a very curious set of cases. A whole family was seized with nausea and vomiting, and every one of them had watery eyes. This continued sometime; I found all the maid servants with watery eyes, and that they had had nausea and vomiting. There were four or five children also in the same state, as also was the lady of the house. The husband happened to be out of town. None of them had a pulse under 120, and in some it was as high as160. The children were all going about the house, and the servants were engaged at their work. The man servant as well as the maids all had rapid pulses with watery eyes. On looking at their tongues, I found they were red and foul, and they had also had thirst, and a sensation of heat at the stomach. I endeavoured to ascertain whether persons in the neighbourhood were in a similar condition, but that was not the case, not even with their nearest neighbours, and therefore I presumed the affection was not owing to the atmosphere. Then respecting the ingesta, I could discover nothing that was at all capable of explaining the symptoms. I found that they all had the general heat of the body which I have mentioned, as well as of the stomach and eyes; in fact a general inflammatory state.

Seeing these things, and finding that the watery state of the eyes and the nausea had been going on day after day, I suspected that they were under the influence of arsenic. When arsenic produces those constitutional effects which it is necessary to remedy, you find bleeding by far the best method that can be adopted. When arsenic has been taken into the stomach, you endeavour to get it out as well as you can, but after that it is necessary to bleed on account of the gastritis, and the inflammatory state of the system. When you have got it all out of the stomach of the patient, there may still be the greatest danger, not only from the inflammatory state of the stomach, but the general inflammatory state of the body; the great remedy for this is bleeding. I felt convinced that this family must have suffered from arsenic, and they were bled, and some proportionately relieved.

The water which they drank was examined by several eminent chemists, and no arsenic could be found in it, but the persons who had lived in the house before had been mixers of colours, and some of their combinations contained arsenic, and before they quitted the house, it turned out that they had deposited in the kitchen, and in the garden all around the house, a great quantity of arsenite of copper. I did not learn this at first, and I could not imagine where the arsenic came from, but when the husband returned home and stated this, on relating the case to an eminent chemist, he told us that he was satisfied that water coming to the arsenite of copper would produce arseniuretted hydrogen, which was let loose, and produced the effect on the family. He considered that the arsenite of copper was calculated to become pernicious by water being mixed with it. The situation of the house was damp, and water had free access to the arsenic contained in the colouring matter, which had been deposited only last summer under the kitchen and every part of the house. Several barrows full of this stuff were removed. It was an eminent chemist who informed us that, under all the circumstances, he had no doubt that arseniuretted hydrogen had been disengaged, and I had felt satisfied, from the effects I saw, that arsenic must have been the cause of all the symptoms. No medicine was given, nor was anything of the kind required, as the arsenic was not in the stomach, but had pervaded the whole system. It was altogether an extraordinary occurrence, and I never witnessed anything of the kind before. The whole family were affected. All had a pulse from 120 to 160, and the whole of them were affected with a running of the eyes, but they all got perfectly well, except that pain of the limbs came on after they had appeared well for some time.

ST. GEORGE'S HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

DR. SEYMOUR,

April 10, 1832.

HYSTERIA.

GENTLEMEN,—There were some cases of hysteria dismissed on Wednesday last. Of two of these, however, I shall speak to you at some length. One of them was a common case, such as is to be met with every day. The other was a severe one. In the treatment of hysteria, it is highly important that you should distinguish real cases from feigned ones. On this particular point you may read what Sydenham says with great advantage, for the observations which he makes will apply as well to this disease in the present day as in the days in which he wrote. Hysterical symptoms have generally some sympathetic reference to the uterus. In hysteria, as we generally see it, the patient has a very great disposition to laugh or to cry, with a peculiar sensation in the throat, as of a ball rising in it. This latter symptom is entirely a nervous affection. In some cases the patient has spasmodic twitchings, and these constitute what is commonly termed an *hysterical fit;* but there may be hysteria without this. It is, in general, caused by some violent emotion of the mind. You find it as common in the higher as in the lower classes, but with this difference, that among the latter it frequently arises from painful impressions on the mind, or from being closely confined within doors, bad air, &c. In weak persons you have generally an irregular action of the uterus. In some cases you will find pain over the eye denominated *clavus hystericus.* Sometimes you will find pain in the left side over the spleen, or great curvature of the stomach. Sometimes on the other side you will find the patient complain of pain over the region of the liver, accompanied with great flatulence, the discharge of which always relieves the patient. This is generally pent up either in the stomach or colon. Sometimes you will find the patient complain of most excessive pain. Depend upon it this is hysteria, for the excessive pain is here diagnostic of the complaint. If there were inflammation to cause this excessive pain, the pulse would show it. The rising in the throat, the pain over the eye, and the flatulency, are common symptoms of the affection. In some cases, however, the symptoms are worse, and the patient loses the use of his lower limbs, and there is paraplegia and sometimes hemiplegia, but these symptoms are in general cured. Sometimes you find the pulse slow and feeble, and the ankles puffed. If the attack of hysteria be acute, it is most difficult to distinguish. In the pain in the throat you will sometimes be led to consider it as inflammation of the throat, and if you give diffusive stimulants you cure it. I have been frequently disappointed in cases of this kind occurring in the hospital.

(Dr. Seymour here related the particular symptoms of the cases on which he founded these observations; but the only point in them to which we need allude, was that of blisters being applied to expel wind.)

These cases you will perceive, Gentlemen, are very simple ones; but you must learn to treat such as these before you go to more complex ones. The pain in the spleen in this disease is a very common symptom. Hence the term of *spleen*, or *vapours*. Dr. Rowley, who was predecessor to Dr. Hooper at the Marylebone Dispensary, wrote a book stating hysteria to be a disease of the spleen. Patients afflicted with this disease seldom die of it. Of the pathology of the disease I cannot tell you anything, for there is none; therefore, to say that the disease is one of the spleen, a viscus of the pathology of which we know so little, is to endeavour to remove one difficulty by setting up another.

(The notes of a severe case were here read, on which Dr. Seymour continued,)

I never saw so much improvement in any case as in this, on the catamenial secretion being restored after it had been lost for eleven months. The disease was here more functional than organic. Paraplegia and hemiplegia, you know, are in general caused by organic disease of the brain, and they will not go off until the organic disease be removed; but when these symptoms go off and return again as suddenly, they cannot arise from the presence of organic disease, but from hysteria. In my case-book you will find that leeches were ordered to be applied behind the ears, but they were really applied to the groins. In these cases you must always respect the feelings of your patient, and it was for this that I ordered the leeches to be applied behind the ears. Many gentlemen practising midwifery in London (and the late Dr. Gooch was also of their opinion), have informed me that the application of leeches to the groin will reproduce the catamenial secretion. I have never found this to be the case, and in the present instance the leeches did not restore the catameniæ. Sydenham says, that one half of the diseases of females are dependent upon hysteria. There is no disease in which you will meet with more imposition than in this, and that too in cases where the patient's rank in life, her station, and education, would all lead you to believe such imposition on her part to be quite impossible. Females will sometimes insist upon it that they have a disease. I once heard of a case in which a young lady was said not to have passed a drop of urine for six weeks. She had diuretics and every other stimulant to the urinary organs, but all to no purpose. Her medical man now began to suspect the case was all trick; she was narrowly watched, and it was discovered that she *drank her urine*. I was once shown a very large quantity of gravel which a young lady passed with her urine. It was reported to me that she had violent pains in the back and loins. She had gone through the whole list of medicines usually prescribed for these cases, and by way of doing something, she had been sent to the sea-side. Now I do assure you, that the gravel which was shown me was nothing more than sea-sand. The above are cases of downright imposition, and in all such cases as *these*, I would advise you to let the patient and her friends know that you are fully aware that her disease is all imposture, and you will generally find this remark sufficient to cure the worst symptoms.

But there are other cases of mixed ones, in which the patient may perhaps be ill; but she will lay very great stress upon it; nothing will relieve her; whatever medicine she takes only tends to make her ten times worse than she is; purgatives confine her bowels; opiates do not make her sleep; diuretics stop the secretion of urine. If you give her medicines for any particular pain, they will only aggravate it tenfold, and so forth. The pains of which she complains are referable to no known disease; indeed she has pain everywhere. Now all this I need not tell you arises from excessive nervous sympathy, and as such it must be treated. Your remedies must always have a tendency, if only in the patient's imagination, to remove her complaint; and you must change, renew, and alter them, according to the Protean shape which the disease assumes. You will sometimes find the simplest remedies cure, where all others have failed. Belladonna rubbed on the back of the neck will sometimes cure. Doing nothing at all will, in fact, more frequently prove your best remedy, after surgeons and physicians without number have been applied to in vain.

You will at once perceive that all these complaints depend upon excessive irritability of the nervous temperament; and anything that will powerfully excite the imagination of the patient in this susceptible state, will effect a cure. It is in such cases as these that non-educated practitioners, quacks, and charlatans, obtain such great credit to the detriment of our profession. It is in similar cases to these that Prince Hohenloe worked such pretended miraculous cures. As you may well imagine, curative medicines of all kinds have been tried, and in vain. Moxa, cautery, blistering, and every other counter-irritant of that class, have been tried. But above all, in your behaviour to your patient you must never at first let her see that you undervalue her complaint, or disbelieve the statements she makes to you of it, however improbable or unlikely they may appear. If you do, she will only endeavour to deceive you still more, and eventually decline seeing you again. I knew a patient who used to be violently sick every morning, and nothing she took would stop the vomiting, until it was discovered that she took opium every night to bring on the sickness in the morning. In all these, and I may say in every case of hysteria, your best remedies are perseverance, patience, and care. These will do more for you than anything else. I knew of the case of a young lady who could not swallow. The first surgeons and physicians in London were applied to for their advice, as well as quacks without number, but all failed. At last, for want of a better remedy, cold affusion was tried, and this proved successful. You must exercise your ingenuity in varying your remedies every time you see your patient; the cold-bath; the warm-bath; change of air; the country; the sea-side; residence in a foreign climate. Sometimes one of these will cure a patient, or she will fancy she is cured; or if you tell her she has a new disease altogether, and prescribe a plan of treatment such as she has never heard of before, she will frequently get well without anything else. If you find the patient low and weak, you had better give diffusible stimuli, as sometimes, in such cases, you will find your patient will have fits. I knew a young lady who lived upon three tablespoonfuls of tea daily. She could not, or she would not, take anything else; but by diverting her mind, and gradually habituating her to a new plan of treatment, she got quite well. In the course of your professional practice, you will meet with innumerable varieties of this disease. You will hear patients say that they pass calculi from the bladder, and when you come to examine them, they will prove to be common flint stones. But you must have patience with all these cases. If you should meet with a case of imposture, never shrink from telling the patient or her friends that you know she is imposing upon you, for no professional man should lend his time and his trouble to a case of imposition.

But, as Sydenham very truly remarks, it would take a whole day for me to discuss every shape, form, and feature, in which this disease will present itself to you. I have, however, detailed to you most of the ordinary symptoms which you will be liable to meet with, and I hope the account of them will be the means of guiding you in some measure towards a correct treatment of this changeable disease.

GASTRODYNIA.

In the treatment of gastrodynia you will find the nitrate of bismuth of great service. It acts as an absorbent, and relieves the spasmodic affection of the stomach. I have had a case of this disease under my care in this hospital, in which however I found rhubarb and magnesia of very great service, under which the patient got materially better. Gastrodynia is generally caused by the presence of acidity in the stomach, and the patient should desist from vegetable acids of all kinds, as they weaken the stomach. You know that the gastric juice of the stomach is acid, and this acidity is increased when gastrodynia is present. Nevertheless, in some cases you will find the most obstinate and formidable symptoms cured by the simplest remedies. There was a case in this hospital, some years ago, of severe vomiting from the stomach, and it was supposed to arise from cancer of the pylorus. All the great guns were fired off on the occasion; every remedy was tried; conium, blistering, &c., but all without any good effect. Dr. Pearson, who had the patient under his care, was not at all alarmed at all these symptoms. He gave the calcined magnesia for a short time, and this effectually cured this terrible case of "cancer of the pylorus." This will show to you how a violent disease may be cured by the simplest remedies, and by those that are most agreeable to the feelings of the patient.

Ligature of the Penis.—A Jew-boy four weeks old, was suckled by a nurse whom the family became dissatisfied with, and dismissed. Two days afterwards Dr. G. was sent for on account of painful swelling of the boy's penis, when he found a long hair five times wound and tied round the root of the penis. With great difficulty the hair was removed from the deep incision it had made, which emollient applications speedily healed; but had it remained a short time longer, gangrene would inevitably have occurred. "Very probably," says Dr. G., "the nurse wished in this manner to revenge herself for her dismissal."—*Rusts Mag.*, 1831.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXVIII.

THE SYMPTOMS, CAUSES, AND TREATMENT, OF INFLAMMATION OF THE LUNGS IN THE HORSE, CATTLE, SWINE, AND DOGS.

We will now proceed from the mucous membrane of the bronchial passages to that which lines the air-cells of the lungs, or to the proper cellular texture or substance of the lungs,—the interior of the lobuli to which we have traced the air-tubes, and in which the important change of the blood is effected. I have said, that the intimate structure of these lobuli has never yet been demonstrated; but we can scarcely avoid considering them as composed of little cells or pouches, into which the air is at length conducted, and over the delicate membrane constituting the divisions of which myriads of minute blood-vessels are ramifying. The blood is not merely permeating them, but it is undergoing a vital change in them: there is a constant decomposition of the air, or of the blood or of both, and during the excitement of exercise, that decomposition proceeds with fearful rapidity. Then we can readily conceive that a membrane so delicate as this must be, in order that its interposition shall be no hinderance to the arterialisation of the blood, so fragile also, and so loaded with blood-vessels, must be exceedingly subject to inflammation, and that of a most dangerous character.

Causes.—Inflammation of the parenchyma of the lungs is the not unfrequent consequence of all the diseases of the respiratory passages that we have considered. Catarrh, influenza, bronchitis, if neglected or badly treated, or sometimes in spite of the most skilful treatment, will spread along the mucous membrane, and at length involve the termination of the air-passages. At other times we have pure pneumonia. This cellular texture is the primary seat of inflammation. It is often so in the over-worked horse. After a long and hard day's hunt, it is very common for horses to be attacked by pure pneumonia. A prodigiously increased quantity of blood is hurried through these little vessels, for the vast expenditure of arterial blood in rapid progression must be provided for. These minutest of capillaries are sadly distended—irritated; their contractile power is destroyed—inflammation is produced—mechanical injury is effected—the vessels are ruptured—blood is poured into the interstitial texture, and this increases the irritation,—and inflammation and congestion, with all their train of fatal consequences, ensue.

Alternations of Heat and Cold.—These are the most frequent causes of pneumonia. A sudden change from heat to cold,—the change from a warm stable to a colder one—the neglect of the usual clothing—the neglect even of some little comforts—riding far and fast against a cold wind, especially in snowy weather—loitering about when unusual perspiration has been excited—loitering tediously by the side of a covert on a chilly blowing morning:—the experience of every day convinces us, that these are the most frequent causes of pneumonia. The thing is so plain, that I am almost ashamed to detain you for a moment about it. Can any one hesitate as to the effect of cold applied to the extensive surface of the skin—closing so many pores—determining so great a quantity of blood to other parts—causing disturbance every-where, and especially in the pulmonary circulation,—in that part which, from the delicacy and complexity of its organisation, and more especially from the injury it has received by our absurd and cruel management of the horse, is most of all subject to inflammation, and that of the intensest kind?

It has not unfrequently happened, that when horses have been turned out too early to grass, or without gradual preparation, pneumonia has supervened, and especially if other causes have assisted. Few horses even in their stable-state, are so subject to pneumonia, as they who, in poor condition and without preparation, are turned into a salt-marsh. The Plumstead marshes below Woolwich are notorious for this.

On the other hand I can imagine, that a sudden and considerable change from cold to heat, shall be followed by inflammation of the lungs. Many horses perish in the dealers' stables from this cause. The circulation is considerably quickened; more blood, and that more rapidly, is driven through the lungs, previously disposed to take on inflammatory action; yet even here the sudden removal from the heated stable to the cold air for the purpose of examination, has much to do with the production of disease. But enough of the causes of pneumonia.

Symptoms.—Whether it is the consequence of previous disease of the respiratory passages, or inflammation first appears in the cellular texture of the lungs, pneumonia is usually ushered in by *rigor*. The horse is cold all over; this, however, soon passes over, and we have general warmth, or heat of the skin above the usual temperature, but accompanied by *coldness of the extremities*—intense deathy coldness. This is a perfectly diagnostic symptom: it will never deceive you It is an early symptom; it is found when there is little or no constitutional disturbance,—when the pulse is scarcely affected, and the flanks heave not at all, but the horse is merely dull and off his feed. It is that by which you will mark the progress of the disease when others scarcely suspected its existence. The blood is determined to the inflamed part—it is congested in the lungs—a diminished portion of it passes the left side of the heart—it scarcely reaches the extremities, or it is not sufficiently arterialised, and little heat is consequently extricated in its change to venous blood.

The *pulse* is *not* always at first *much increased* in rapidity, and it is rarely or never hard: it is *obscure, oppressed.* The heart is labouring to accomplish its object—the circulation through the lungs is impeded—the vessels are engorged, they are ruptured—blood is extravasated into the air-cells—it accumulates in the right side of the heart—and in the larger vessels and in the venous circulation generally, there is a mechanical obstruction, which the heart has not power to overcome; thence the obscure oppressed pulse,—the ineffectual attempt to urge on the blood, and hence, too, the remarkable result of bleeding in inflammation of the lungs;—the pulse becomes rounder, fuller, quicker. When blood is abstracted, a portion of the opposing force is removed, and the heart, now evidently accomplishing its object, the pulse is developed.

It is only, however, in the early insidious stage that the flanks are occasionally quiet; if the compressibility of the lungs is diminished by the thickening of the membrane, the engorgement of the vessels and the filling of the cells, it will be harder work to force the air out; there must be more evident effort, and that which cannot be accomplished by one effort is attempted over and over again. *The respiration is quickened—laborious*; the *inspiration is lengthened*, to lessen the pain which the distension of the vessels occasions; the *expiration is rapid*, to get rid of the pain caused by the state of distension; and when, after all, the lungs cannot be compressed by the usual means, every muscle that can be brought to bear upon the part, is called into action; hence *the horse will not lie down*. He can use the muscles of the spine and the shoulder with most advantage, as he stands: hence, too, the very peculiar stiffness of position—the *disinclination to move.* There are various muscles whose action can be materially changed when we change the fixed point. The muscles which compose the serratus magnus influence the motion of the shoulder, while the chest remains a fixed point; but when by throwing the whole weight on the fore extremities the limbs are made the fixed point, the chest is materially expanded by the action of the same muscles. Therefore the horse with decided pneumonia can scarcely be induced to move at all; he cannot spare for a moment the assistance which he derives from these muscles; he will continue obstinately to stand until he falls exhausted or dying. How eagerly do we ask when we go into the stable, Was he down last night? And we conclude that we have not made much progress in the case when the answer is in the negative. When he is wearied out and lies down, it is only for a moment. If the inflammation is not subdued, he cannot dispense with the auxiliary muscles. He frequently, and with doleful expression, *looks at his sides*—at one side or at both, accordingly as one or both are involved; there is not, however, the decidedly haggard countenance of bronchitis, and you will recollect, that in bronchitis the horse rarely or never gazes at his flanks,—his is a dread of suffocation more than a feeling of pain. The *head is protruded* and *the nostrils distended*, and *the mouth and the breath intensely hot. The nose is injected* from the earliest period, and there is not merely injection, but the membrane is uniformly and intensely red. The variation in this intensity is anxiously marked by the observant practitioner, and he regards with fear and with despair the livid or dirty brownish hue that gradually creeps on. The continued intensity of inflammation he can probably combat, but he cannot repair the exhausted and disorganised tissue.

Progress of the Disease.—The unfavourable symptoms are, increased coldness of the ears and feet if that be possible, partial sweats, grinding of the teeth, evident weakness, staggering, yet the animal will not lie down; pulse quickening, and becoming weak and fluttering; the membrane of the nose paler, but it is a dirty paleness; the animal becoming stupid, comatose: at length he falls, but he rises immediately; for awhile he is up and down, until he is no longer able to rise; he then struggles severely—groans; the pulse becomes more rapid, fainter, and he dies of suffocation. The disease sometimes runs its course with strange rapidity. I have known a horse destroyed by pure pneumonia in twelve hours. The vessels ramifying over the cells have yielded to the fearful impulse of the blood, and the lungs have presented one mass of congestion.

The favourable symptoms are; the return of a little natural warmth to the extremities;

the circulation beginning again to assume its natural character; and, next to this, the lying down quietly and without uneasiness; showing us that he is beginning to do without the auxiliary muscles. These are good symptoms, and they, too, will rarely deceive.

Terminations of Pneumonia.—Congestion is a frequent termination of pneumonia. Not only are the vessels gorged,—the congestion which accompanies common inflammation, but their parietes are necessarily so thin, in order that the change in the blood may take place, although they are interposed, that they are easily ruptured, and the cells are filled with blood. This effused blood soon coagulates, and the lung when cut into presents a black, softened, pulpy kind of appearance, termed, by the farrier and the groom, *rottenness*, and being supposed by them to indicate an old disease. It proves the violence of the disease, the rupture of many a vessel surcharged with blood; and it also proves that the disease is of recent date; for in no great length of time the serous portion of the blood becomes absorbed, the more solid one becomes organised, the cells are obliterated, and the lung becomes *hepatised*, or bears considerable resemblance to liver. The farrier, is now, however, becoming a little enlightened on this point, and regards this appearance of the lung as indicating intense but recent disease; yet only a few years ago many a dealer was unjustly punished in his pocket for selling a rotten horse, whereas the inflammation and effusion had been produced by some accidental cause or by too early and severe work, after the animal had left the dealer's stable, in which he had been made up for sale.. Here is a very fine specimen of congestion. The cells appear to be all filled; the lung is quite impervious; respiration could no longer be carried on, and the horse died of suffocation. Here is another specimen in which the inflammation appeared also to pervade the whole of the lung; there is a redness; great turgidity of the vessels every-where, but it has not run on to effusion; the vessels were not ruptured.

The Value of Auscultation.—In every case of pneumonia, have early and anxious recourse to *auscultation.* Make yourselves perfectly acquainted with the deep distant murmur presented by the healthy lung. This sound is most distinct in the young horse, and especially if he is a little out of condition. On such a horse the tyro should commence his study of the exploration of the chest. There he will make himself best acquainted with the respiratory murmur in its full state of development. He will next take an older and somewhat fatter horse; he will there recognise the same sound, but fainter, more *distant.* In older and fatter horses there will sometimes be a little difficulty in detecting it at all. Repeated experiments of this kind will gradually teach the young veterinarian what kind of healthy murmur he should expect from every kind of horse that is presented to him, and thus he will be better enabled to appreciate the different sound exhibited under disease.

Attention to Auscultation urged.—Before Laennec had led the human practitioner to adopt the invaluable method of exploration, or the veterinarian had dreamed of such a mode of examination, the most useful guide we had in ascertaining the nature and progress of disease in our dumb patients was the pulse. I think I can safely say that for nearly a twelvemonth I daily studied the pulse of every horse in the Veterinary College, anxiously comparing it with the age, breed, disease, and varying character of disease; and I look back on these hours as spent the most usefully of all in the preparation for professional duty. You, Gentlemen, have now another guide fully as valuable in all the diseases of the chest. Avail yourselves of the same opportunity to derive from it all the advantage it can afford you. It will be time most usefully spent, and the recollection of it will be gratifying to you. In this perambulation of the hospital, and in the cultivation of morbid anatomy—not a succession of barbarous operations and experiments; do not disgrace yourselves by them—they will not afford you matter for pleasing reflection hereafter;—in the cultivation of morbid anatomy I say in the knacker's yard, you will be laying a surer foundation for future reputation and usefulness, than in the attendance on any lectures, however gifted the teacher.

The Crepitus of Pneumonia.—Then make yourselves acquainted with the deep distant murmur presented by the healthy lung. If pneumonia exists to any considerable degree, this murmur will be changed for, or mingled with, as I observed in the last lecture, a curious *crepitating sound.* This once remarked cannot well be mistaken. It is caused by the infiltration of blood into the air-cells; its loudness and perfect character will represent the intensity of the disease, and the portion of the chest at which it can be distinguished will clearly indicate the extent of the disease.

Hepatisation.—The whole lung, however, is not always affected, or there are only portions or patches of it in which the inflammation is so intense as to produce this congestion; enough remains either unaffected or yet pervious for the function of respiration to be performed, and the animal lingers on or perhaps recovers. Then the process which I have just described will take place where the inflammation has been most intense. The serous portion of the blood will be absorbed, and the more solid will become organised, impervious, *hepatised*. By careful examination with the ear, you will be enabled to get at this also. Where the lung is impervious—where no air passes, no sound will be heard, not even the natural murmur. Around it the murmur will be heard, and louder; it will be a kind of rushing sound; for the same quantity of blood must be arterialised, and the air must rush more rapidly and forcibly through the remaining tubes: and if there be considerable inflammation or tendency to congestion, the crepitating crackling sound will be recognised, and in proportion to the intensity of the inflammation. Gentlemen, I do not overrate the advantages to be derived from the study of auscultation. It was strong language lately applied by an able critic to the use of the *stethoscope* by the human practitioner; I would literally apply it to the use of immediate auscultation, that "it converts the organ of hearing into an organ of vision, enabling the listener to observe with the clearness of occular demonstration the ravages which disease occasionally commits in the very centre of the ribbed-cased cavity of the body."

A horse with any portion of the lungs hepatised cannot be *sound*. He cannot be capable of continued extra-exertion; his imperfect and mutilated lung cannot supply the arterialised blood which rapid progression long continued requires, and that portion which is compelled to do the work of the whole lung must be exposed to injury and inflammation from many a cause that would otherwise be harmless. Here are several specimens of hepatisation.

Tubercles.—Another consequence of inflammation of the substance of the lungs is the formation of *tubercles*. There is a great deal of mystery, and there has been some acrimonious dispute, about this. It would seem that at first there are a greater or smaller number of little distinct cysts, cells, into which some fluid is poured in the progress of inflammation; the cells become gorged, distended, and occupy a space varying from a pin's point to a large egg. Here is a beautiful specimen of tubercles of the lung, each nearly the size of a filbert. Here is another in which they are more numerous, but not so large as the smallest millet-seed. This last belonged to a glandered horse.

By degrees the fluid becomes concrete; the little tumour is much harder than the surrounding substance, and so it continues for a while—the consequence and the source of inflammation. It occupies a space that should be employed in the function of respiration, and by its pressure it irritates the neighbouring parts, and exposes them to inflammation.

By-and-by commences another process never sufficiently explained. The tubercle begins to soften at its centre,—a process of suppuration commences, and proceeds until the contents of the cyst become again fluid, but of a different character—now consisting of pus. The pus increases; the cyst becomes more and more distended; it encroaches on the substance of the lungs on every side; it comes into contact with other tubercles, and the walls opposed to each other are absorbed by their mutual pressure; they run together and form one cyst;—morbid anatomists term this a vomica. It is no process of ulceration, no corroding or eating into the substance of the lungs, but a gradually-increasing and regular excavation, and this sometimes runs on to a very great extent, until a considerable portion of the lung is, as it were, hollowed out. By-and-by however the vomica presses upon some bronchial passage, and the cyst gives way; and the purulent contents are poured into the bronchiæ, and got rid of by the act of coughing—a great quantity of pus and often fœtid is thrown up—at other times the quantity is too great to be thus got rid of, and the animal is suffocated. Sometimes it will break through the pleuritic covering of the lung, and pour its contents into the thorax.

Abscesses may exist in the Lungs undiscovered.—It is scarcely conceivable to what extent these abscesses may exist in animals of slow work, without their being discovered by the usual means of examination. Mr. Hales tell us that he gave a physic ball to a cart-mare with bad foot, and she soon afterwards died suddenly. When inquiring as to the cause of death, he was told, and not very good-humouredly, that his physic had killed her. He asked if it had purged her violently? "No!" it was replied, "it had not operated at all." She was opened, and the mystery was all unravelled. The thorax was deluged with pus, and there were then in the lungs several large abscesses, one of which contained at least a quart of pus. The mare had not shown a symptom of chest affection, and the gentleman to whom she belonged, declared that he had believed her to be as sound as any horse he had in his possession.

Sometimes I believe there is true ulceration of the lungs, and seemingly not connected with the presence of tubercles. I cannot tell you how you may ascertain this. I am not sufficiently master of auscultation to describe the difference in the sound, or, rather, as no sound would be given immediately from the part through which no air passed, it would be impossible to distinguish the one from the other. One thing however we know, that where the progress of ulceration is rapid the horse must die—his wind might be impaired, but his life would not always be endangered by the existence of tubercles.

Resolution of Inflammation.—The resolution or gradual abatement of inflammation is the termination most to be desired, and then the engorgement of the vessels will gradually cease, and the thickening of the membrane and the interstitial deposit will be taken up, and the effusion into the cells will likewise be absorbed, and the lungs will gradually resume their former cellular texture; yet not perfectly: there will be some induration, slight but general; or some more perfect induration of certain parts, or the rupture of some of the air-cells, or an irritability of membrane predisposing to renewed inflammation. We may save the horse, but he will not always be as useful as before. These sequelæ, under the terms chronic cough, thick wind, broken wind, merit distinct consideration, but for the present I must proceed to the treatment of pneumonia.

Treatment—Bleeding.—Here again commence with bleeding. You have inflammation of that organ through which all the blood in the frame passes—that organ most of all subject to congestion. Nothing can be so important as to lessen the quantity of blood which the heart is endeavouring to force through the minute vessels of the lungs, distended, irritated, breaking. When you bleed in other cases you lessen the quantity of blood given to the part only in proportion, and that often a very small one, to its share of the general mass of fluid; but here every drop you take away is so much subtracted from the quantity of blood permeating the overdistended and overworked capillaries. Then bleed, and bleed copiously too. In a decided case of inflammation of the lungs, try to knock the disease down at once—it is your best, and sometimes will be your only chance. Take, in a manner, as much blood as you can get; let the stream flow on until the pulse falters, and the animal bears heavy upon the pail: and take the blood which you do take as quickly as you can take it—let your lancet be broad-shouldered, and the orifice large. This is the secret of treating inflammation of a vital organ, and of an organ like this. You weaken or destroy the disease, without permanently impairing the strength of the patient; whereas by small bleedings, and with a small stream, you are sapping the strength of the patient, while the disease remains untouched. Gentlemen, if I can only impress this truth upon your minds,—this polar star in practice—the memory of our transient connexion will not be displeasing—to either of us.

Physic.—Next comes purging—yes—if we dared; for we should get rid of some cause of excitement, and lessen the circulating fluid, and produce a new determination of the vital current: but experience teaches us that in pneumonia there is so much sympathy with the abdominal viscera,—there is such a fatal tendency in the inflammation to spread over every mucous membrane, that purging is almost to a certainty followed by inflammation, and that inflammation bids defiance to every attempt to arrest it. I speak with perfect consideration, when I affirm that, in the majority of cases, a physic ball would be a dose of poison to a horse labouring under pneumonia.

May I not relax the bowels? Yes, if I could stop there. In my own infirmary, and there we should endeavour to get all cases of this description, I may, after the inflammation has evidently a little subsided, venture upon, yet very cautiously, small doses of aloes—half a drachm two or three times in the day in my fever medicine, and I may quicken it by frequent injections of warm soap and water; omitting my purgative however the moment the fæces are becoming pultaceous: but I must be assured that the inflammation is subsiding, and I must have considerable constipation, or I had better let my purgative alone. I have, in an early period of practice, more than once killed a horse labouring under inflammation of the lungs with two drachms of aloes.

Sedatives.—If I must not give physic I must endeavour to find some auxiliary to the bleeding, and I have it in the compound of *digitalis, nitre, and emetic tartar*, that I have so often recommended. The foxglove, a direct and powerful sedative, whose principal effect is produced on the heart, lessening the number of pulsations, and that by a curious intermission of the heart's action; while its secondary effect is also a useful one—namely, that of a diuretic. Some practitioners prefer the *white hellebore*. It is a powerful nauseant—it lowers the frequency and the strength of the pulse, but it does so almost on the principle of a blister, by producing a powerful counter-determination of blood, and that principally to the brain. By the excitement of the brain or its membranes, which it seems to produce, it diverts the inflammation, or a portion of it, from its original seat. In this respect it is as powerful as we could wish, and in our own infirmary, and where we see our patient a dozen times in the day, it may be used with safety: but, out of doors, and where we do not see him more than once in twelve or four-and-twenty hours, it seems a little too much like playing with edged tools. If the tremor, the vertigo, and the purging, which it produces, are carried a little too far, the patient may be lost. Generally speaking the hanging of the head, and the foaming at the mouth, and the sinking of the pulse, may give warning; but our assistant may not observe these symptoms, or they may have existed many an hour before he arrives. In our own hospital it may be admitted, and will not often deceive, but even there we could wish for something as effectual and safer, and you recollect the account in the last lecture of its alarming effects when administered in a case of bronchitis. We however have that which is even more effectual, and which never did harm, the digitalis. In the course of twenty years' practice I do not recollect a single case in which I had cause to repent of the exhibition of digitalis, but I have attributed the deaths of more patients than one to the incautious use of hellebore. The nitre is a refrigerant and a diuretic, and the emetic tartar is a nauseant and a diaphoretic.

General Care.—Having determined on the medicine, I think of the kind of care which the patient needs. I order his legs to be well hand-rubbed, in order to restore if I can the circulation to the extremities; and I direct that comfortable flannel rollers shall encase the legs from the foot to the knee. I cover him up warm. Can there be a doubt about this? I wish to divert the current of blood as much as I can to the skin. I know that cold applied to the extremities will determine the blood to the centre, and cause it still more injuriously to congest upon and around the seat of inflammation; therefore I uniformly clothe him well. As for air, in warm weather, he cannot have too much. In cold weather his box must be airy, but not chilly. I want to determine the blood to the extremities and the skin, but not all the clothing in the world will keep my patient warm, if he is placed in a cold and uncomfortable situation.

Food.—As for food, I think not of it. In nine cases out of ten he will not touch anything, and if he is inclined to eat I give him nothing but a bran-mash, or a little green-meat, or a few carrots.

Blisters.—I now look about me for some counter-irritant. I wish to excite some powerful action in another part of the frame, and which shall divert the current of blood from the part first affected. I recognise it as a law of nature, and of which I here eagerly avail myself, that if I have a morbid action in some vital part, an unusual determination of blood to that part, I can abate, perhaps I can at once arrest, that morbid action by exciting a similar or a greater disturbance in some contiguous and not dangerous part. Therefore I blister the sides and the brisket; I produce all the irritation I can on the integument, and in proportion as I do so, I abate the inflammation within. But then there are certain considerations which I must not forget. The original inflammation may be so intense that it will occupy,—absorb, as it were, the whole energy of the vital principle. The system shall be so oppressed, overpowered, as to be insensible to everything else. Then I must somewhat abate the original inflammation by other means, before I can hope to derive any benefit from my counter-irritant; and, more than this, it is probable that if my stimulus is not powerful enough to abate the first, it may do more harm than good, by increasing or even producing general irritation. Therefore I do not blister immediately. I see what will be the effect of my first bleeding, and my sedative medicines; and if I am making some progress I call in the blister to my aid; I prevent the foe from rallying, and perfectly accomplish his subjection.

Therefore it is that our blisters do not sometimes rise. The original inflammation has not been weakened. It is more powerful than any stimulus we can apply; and we augur badly when this is the case.

The way being prepared for the action of a counter-irritant, we have recourse to a larger surface,—there is more chance of a blister, in preference to a rowel, and decidedly so, for our stimulus can be spread over its being applied to the immediate neighbourhood of the original inflammation, and, most assuredly, from the extent of surface which I can inflame, I can apply a quantity of stimulus beyond comparison greater than a rowel would permit me to do. I might insert a rowel in an obscure case, in order to avoid the trouble and the blemish of a blister, but I should be justly deemed an unscientific practitioner if I had recourse to a rowel in preference to a blister, in a plain case of acute pneumonia. In the *latter* stage of disease the blister will not act, because the powers of nature are exhausted. We must repeat it,—we must rub more of the ointment well in,—we must rouse the sinking energies of the frame if we can; the effort, however, will generally be fruitless, and the not rising of a blister, in the latter stage of the disease, may generally be regarded as the precursor of death, and especially if it is accompanied by the livid or brown colour of the membrane of the nose.

Care and Attention.—Pneumonia, like bronchitis, requires anxious watching. The first object is to subdue the inflammation, and your measures should be prompt and decisive. If the mouth continues hot, and the extremities cold, and the nose red, bleed again and again, and that in rapid succession. The good which you can do must be done at first or not at all. When you have got a little returning warmth to the extremities, you will continue to give your sedative medicines, and without one grain of a carminative or a tonic; and the return of the deathy-cold foot will be a signal for further depletion. The commencement of the state of convalescence requires the same guarded practice as in bronchitis. As many horses are lost by impatience now, as by want of decision at first. If you have subdued the disease, let well alone; guard against the return of the foe by the continued administration of your sedatives in smaller quantities; but give no tonics unless debility is rapidly succeeding; and when you have fairly weathered the storm, and your patient seems to be well, still be cautious; consider the nature and the seat of the disease; the predisposition to returning inflammation, for many a week; let not the lungs be put upon; if the season will permit let a two or three months' run at grass succeed to your medical treatment; but if this is impracticable, put off the period of active work as long as you can persuade your employer to let it be delayed; and after that let the horse return as gradually as may be to his usual employment and food.

PNEUMONIA IN CATTLE.

Although bronchitis is the usual chest-disease of cattle, many cases of pure pneumonia will occur. Working cattle are more particularly subject to it, especially those who after a hard day's labour are abandoned to cold and starvation, or who, the moment they are turned loose, are suffered to slake in the cold brook that thirst by which they had been tormented for many an hour. Cows neglected, or too soon exposed to cold and wet after calving, are often attacked by inflammation of the lungs. It frequently becomes epidemic among them. It occurs oftenest in the spring and autumn, and comparatively rarely in the winter. When a warm spring and premature vegetation succeed to a cold and rainy winter, and at the time that they are first sent to pasture, inflammation of the lungs is very rife among cattle, and especially in low and flat and marshy situations, and where the water long lies stagnant over the flooded meadows. The same ground that gives the rot to sheep disposes cattle to pneumonia.

Symptoms.—The symptoms are analogous to those in the horse; the same rigor, depression, heaving, oppressed pulse, coldness of the ears and legs, and *roots of the horns*, and heat of breath. To these are added diminution of milk, swelling of the teats, loss of appetite, cessation of rumination, and some swelling of the abdomen. A slight cutaneous eruption along the back and sides is a frequent accompaniment of pneumonia in cattle, and which we attentively watch. If the eruption is repelled, the inflammation will become more intense, and probably fatal; if the pustules take their regular course, and break, and desquamate, we augur well. There is rarely any acute inflammation in these animals in which the skin or the subcutaneous tissue does not participate, and that peculiar tenderness about the loins accompanies or succeeds the eruption. There is the same protruded head; the same obstinacy of standing,—the same disinclination to move, and a manner of standing even more peculiar than in the horse, the fore-legs being placed as far apart as possible from the sides. Sometimes there is little cough, at others it is constant and distressing, and the sudden cessation of it is one of the most fearful symptoms that can occur. Occasionally the tumours about the joints, to which cattle are so subject, suddenly appear, and we are pleased to see them; they indicate some metastasis, or possible metastasis of inflammation.

The symptoms of an approaching fatal termination of the disease are a little but not very different from what they are in the horse; the quick, weak, intermittent pulse; stupor; insensibility of the skin; fœtid breath; fœtid discharge from the mouth and nostrils, and more from the mouth than in the horse. The disease is sometimes fatal before the termination of the second day, although occasionally the animal lingers on to the seventh or eighth day.

The appearances after death are thus far different, that pneumonia becomes oftener complicated with pleurisy than in the horse; effusion into the chest is oftener found, and in larger quantity. The liver has often acquired an extraordinary size, but the intestines comparatively seldom exhibit much appearance of inflammation. In some cases the membranes of the intestines are even *less* injected than in their natural state.

After apparent cure, there is often a chronic inflammation of the lungs, or more probably chronic bronchitis, which continues for five or six weeks, and at length destroys the animal, of which I shall say more hereafter.

Auscultation.—The exploration of the chest is not so well understood here, and I believe practised by very few; but the time will come, when here too it will receive the attention which it deserves. The respiratory murmur is not so loud in the ox as in the horse. The walls of the thorax are thicker, the integument is thicker, and there is more interposed adipose membrane; but the sound is sufficiently distinct for our purpose, and the changes which it undergoes will afford us very useful indications. There is one thing however against which we must be on our guard. I have observed again and again that the subcutaneous cellular membrane in these animals strangely sympathises with every acute disease, and that there is often a peculiar crackling noise, rarely observed in the horse, and not only evident when the back or loins are pressed upon, or when the animal moves, but in every act of respiration. Something of the kind is heard while the beast ruminates; it is most distinguishable about the lower part of the chest. It somewhat resembles the guggling of a bottle. This is referable to one of the stomachs. We must be careful that we do not confound the crepitating sound of the subcutaneous texture with that of the lungs during pneumonia, nor the guggling in the rumen or reticulum, with that of effusion into the chest, or the existence of abscess in the substance of the lungs.

Treatment.—There must be the same promptitude and decision. We must bleed even more copiously than in the horse, and until the animal is more evidently affected. The same sedative medicine should be administered, but in smaller doses; but purgatives are here admissible, and highly useful. We have in the Epsom salts one that is sure and not irritating; and probably on account of the interposition of such a mass of cuticular surface as the three first stomachs contain, there is not the sympathy which we observe and lament in the horse: the liver, being in contact with the diaphragm most, and sometimes alone, participates in the inflammation Also, as I have observed when speaking of other chest affections in cattle, setons in the dewlap must supersede the blister:—the skin of the ox is too thick and insensible for the blister to produce its proper effect. These setons may be made to produce irritation and engorgement enough if the root of the black hellebore is used instead of the cord.

INFLAMMATION OF THE LUNGS IN SWINE.

The rising of the lights, the popular term for inflammation of the lungs in the swine, is one of the pests of the stye. I am not aware that it is contagious, and yet in a distillery it sometimes destroys a fourth of the hogs in the course of a few weeks. Some atmospheric agency produces a tendency to pneumonic complaints; and the forcing system which is adopted in the styes speedily gives, to that which would otherwise scarcely run beyond catarrh, a character of intense and fatal inflammation. The distressing cough, the *heaving* at the flanks, and the refusal of all food, will be chief indications of the disease. Bleeding will be immediately resorted to—purging, but cautiously attempted; the Epsom salts, will be the safest aperient, and then the sedative medicine already recommended will follow, but with the substitution of the antimonial or James's powder for the emetic tartar. Two grains of digitalis, six of antimonial powder, and half a drachm of nitre, will be a medium dose. The disease will often run its course with an intensity and fatality only equalled by the inflammatory fever of cattle; our measures must therefore here likewise be prompt. The phthisis or consumption, which is the too frequent termination of the disease, even when the animal weathers the first attack, can be combated only by cleanliness and warmth and mildly nutritious food.

INFLAMMATION OF THE LUNGS IN DOGS.

The complaint, although not frequent, is here singularly marked. The extended head—the protruded tongue—the anxious, blood-shot eye—the painful heaving—the hot breath—the obstinacy with which the animal will *sit* up, hour after hour, until his feet slip from under him, and the eye closes, and the head droops from extreme fatigue, while in a moment he is roused again by the feeling of instant suffocation—these are symptoms that cannot be mistaken. Here, from the comparative thinness of the integument and the parietes of the chest, we have the progress of the disease brought as it were completely under our view. The exploration of the chest of the dog by auscultation is a beautiful and a wonderful thing. As however pneumonia in the dog is so generally connected with pleurisy, I shall defer what I have to say respecting it until the next lecture, except remarking that here too our measures should be prompt—our venesection copious—our purging somewhat cautious, and produced by the Epsom salts and not by aloes—the sedative medicines those just recommended for the swine, and the quantity regulated by the size of the animal.

ESSAYS
ON
ANIMAL ACTION.

By William Dobson, *Esq., Surgeon.*

No. III.

RESPIRATION.

There are few who have studied physiology, who have not discovered the greatest discrepancies in the theories of the mechanical action of respiration.

Numerous agents conspire in the performance of breathing. We have to ascertain, as far as is practicable, the relative value of each, and the manner in which they individually and collectively exert their influence.

By the conjunctive actions of the intercostal, the diaphragmatic, and the abdominal, muscles, respiration is supposed to be performed.

Adhering to the important principle of deducing our conclusions from that unerring basis, anatomy, and disregarding the speciousness of theories, however celebrated may be the propounders, taking nothing for data but such facts as are either palpable to the senses, or based on the most striking analogies,—on these grounds the preceding conclusions are carefully inferred.

THE INTERCOSTAL MUSCLES.

We shall commence with the intercostal muscles. They are situated between the ribs, and the obliquity of their fibres is such that they can mechanically approximate the ribs, and in a manner which a vertical disposition would unquestionably have precluded. Now as I conceive respiration to be an involuntary act, the agents which effect it must be involuntary also. Although this supposition is denied by many, yet we shall endeavour to prove its correctness.

Where are we to expect the nerves to arise which supply the intercostal muscles, and under what system of nerves are they usually classified?

Sir Charles Bell, whose name deservedly ranks high in physiological science, conceives that there is a tract of medullary matter, extending throughout the dorsal region of the spinal marrow, placed between the anterior and posterior columns, but as no one can perceive it, we are justified in questioning its existence. That it may exist, and not be discoverable, is quite possible, but until it is discovered we are not warranted in reasoning on its actions. Dissections demonstrate that the intercostal nerves are derived from the anterior and posterior columns of the medulla spinalis, and they demonstrate, negatively, that they have no other origin; that the intercostal nerves arise most unequivocally from the same nervous masses, continued into the cervical region, from which the nerves of the arm originate, those nerves being destined to incite the arm to *voluntary* movements, and possessed of sensation. They have the very identical conjunctions with the surrounding nerves. Each trunk communicates with the contiguous ganglion of the sympathetic by two or more branches, and I affirm that no diversity of *structure* is recognisable.

These are facts, they are the evidences of the senses, and they are strongly corroborated by the testimony of reason.

Are we not compelled then, by the rules of scientific investigation, to consider the motor portions of the intercostal nerves as strictly voluntary? "If the analogy of two phenomena be very close and striking, while at the same time the cause of one is very obvious, it becomes scarcely possible to refuse to admit the action of an analogous cause in the other, though not so obvious in itself."* With this celebrated philosopher I must decidedly concur. But I am nevertheless aware how deceptive are the senses, how frequently they induce in the mind erroneous conceptions. "We can only reason from what we know."

The idea that the intercostal muscles possess an involuntary power of action, has obtained universal and almost implicit credence; but upon no other ground than that of conjecture. I repeat, if these muscles be supplied by the same kind of nerves as those of the arm, and the mechanism be alike, we cannot consistently withhold our assent to the acknowledgment that their functions are identical. No reasonable mind can possibly arrive at an opposite conclusion, for the correctness of the position is so incontrovertibly established, being founded on the basis of demonstration.

The principal argument which can be advanced in support of the idea that the intercostal muscles are voluntary, is the following, that they are so placed as to be enabled to elevate or depress the ribs. But I am inclined to believe, that in proportion as the chest is increased in its circular diameter by the *elevation* of the ribs and their expansion outwards, will the vertical extent be diminished, and therefore little if any effect can result. When the descent of the diaphragm and the action of the intercostals occur simultaneously, a considerable enlargement of the area of the chest must necessarily be produced. Still I am convinced, that for ordinary breathing an increase of the chest's capacity in the vertical direction by the descent of the diaphragm, will cause ample effects. But in the subsequent essay, this circumstance will be more particularly dwelt upon. It will be asked then, If the intercostal muscles be not agents of breathing, what their office? Why were so numerous a set of muscles produced? Reflecting on the conditions of an animal, both man and beast, we observe that the chest has other offices to fulfil than merely to allow of respiration taking place. One great object which the chest is destined to effect, is *voice*. Capacitating the animal to express

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* Herschel's Discourse on the Study of Natural Philosophy, p. 149.—*Lardner's Cyclopædia.*

its varied wants, it is the trumpet of the mind, and sounds the alarm. The intercostal muscles being under the immediate dominion of the will, fix the chest, expand it fully with air, or expel its contents, in obedience to the dicta of the mind. They are important agents in enabling an animal to suppress respiration or accelerate it, as in the efforts of listening and speaking, &c.

The actions of a muscle are contraction and relaxation; but the actions of the voluntary muscles are limited in their duration. They act in obedience to the will, thence soon become fatigued, and the integrity of their actions can be preserved only by the will. Thus they are at rest during sleep, and likewise when from any cause will is abolished, as in apoplexy and compression of the brain, or when the communication between the brain and the muscles is interrupted, either by a section of the spinal marrow or the nerves, or when these are compressed by effusion, or are diseased. If the preceding principle be true, the intercostal muscles can continue their operations only for a very short period: they will cease to act during sleep. To conclude otherwise is so disconsonant from reason, that I am surprised any one could do so who is accustomed to think. As collateral evidence, of these muscles *not* being *involuntary*, the following corollaries may be adduced.

In the infantile period, and even at manhood, the elasticity of the thoracic parietes is such as to admit of a considerable diminution or increase in the chest's capacity, in a circumferal direction. The cartilages of the ribs are soft and pliant, and the articulations free and perfect; and it is observed that the breathing is almost entirely thoracic; obviously dependent on the large abdominal viscera, more especially the liver, which obstruct the play of the diaphragm into the abdomen. But as age advances, the viscera in the abdomen diminish relatively in size; the costal cartilages become denser and ultimately ossified, thence lose their elasticity; the articular movements are restricted, so that in old age the mobility of the ribs is frequently destroyed; the floating ribs, however, generally retain a slight degree of moveableness. In the Museum of the College of Surgeons is a specimen of complete anchylosis of the joints of the ribs, and entire ossification of all parts of the chest, so as to have rendered it a solid and immoveable bony case. It must be admitted, that in this subject respiration had existed without the agency of the intercostal muscles.

In old age any motion of the chest is scarcely visible, but full expansion of the chest at this period is not necessary. The circulation is slower, and breathing is proportionate. As his mental and physical vigour is impaired, less atmospheric air is demanded. The vertical enlargement of the chest appears amply sufficient for all his purposes. His days of oratory and singing, of running and leaping, have now gone by, and his approaching death is evinced by languid actions of the mental and corporeal powers.

We have endeavoured to show that the intercostal muscles correspond in their *anatomical* relations to the system of *voluntary muscles*, and we therefore assign to them *analogous* functions. If they possessed a superadded power, there would exist most unequivocally a superaddition of mechanism. This is an axiom in physiology sufficiently established.

WESTMINSTER MEDICAL SOCIETY.

Saturday, April 28th, 1832.

Mr. Chinnock, President.

Although this was the last evening of the present Session, the attendance was not very numerous, and the array of combatants (for there was little doubt of a renewal of the old skirmish) by no means indicated such an encounter as took place. Before giving a report of it, however, it should be premised that whilst the chief of the anti-contagionists were present, many of those who have taken a prominent part on the opposite side of the question were among the absentees.

CONTAGION OR NON-CONTAGION OF CHOLERA.

The reading of the minutes was followed by some remarks from Dr. Gilkrest, who defended himself from the charge of incorrectness relative to the cases of Cocklin and Donnie.

Dr. Blicke also made some observations on the incorrectness of a report which appeared in a late number of the *Times.*

Mr. Greenwood also made some remarks on the same subject, but the President interfered, observing that the course was not in order. The reports in the public journals could not be made a part of the business of the evening.

Mr. Greenwood complained, that whenever a gentleman rose to speak on the side of contagion an attempt was made to put him down. ("No, no."—"Chair.") Mr. G. then made some remarks on the great discrepancies which existed amongst the anti-contagionists as to the cause and mode of propagation of the cholera. When he looked at facts, he wondered at the denial of contagion. A woman near Ely, having three children, sent one of them to see a friend at a distance attacked with cholera. The child returned, ten hours afterwards was seized and died, after which the two remaining also experienced an attack of the same disease. Now if this could be ascribed to local circumstances, how was it that the children who did *not* go were not first taken ill? An instance had lately occurred within his own knowledge. A girl, quite well, attended the funeral of a person who died from cholera. After the funeral she partook of some refreshments in the house of the deceased, went home, had vomiting and purging, and died. Her sister went to see her, and *she* also died. Mr. G. then referred to the cases lately adduced in The Lancet by Mr. Prebble, and thought that, after such evidence, a conclusion against the disease being contagious was proof of a most illogical mode of deduction. He had called the "declaration" of the French surgeons and physicians "hasty;" the government regulations in France showed pretty clearly that such was the case. Opinions were changing *somewhere,* for Marshal Soult had made certain arrangements for the marching of troops, in order, as the official documents stated, "to prevent the contagion of the disease spreading from town to town." The opinion of Broussais, too, who had given a tone to the system of continental medicine, was now in favour of the contagionists. That great physician had lately declared, that he never attended on any individuul first attacked in a house, without finding that the disease soon broke out amongst several of the other inmates. The same physician further stated, that he regarded the cholera as contagious as the small-pox. Hufeland, too, of Berlin, had pronounced his opinion, that cholera was really to be classed with contagious diseases. It was at least something to have such authorities in his favour. With regard to a gentleman on the other side, Dr. Johnson, who seemed in the last debate to assume great merit for prescience, there was certainly some reason to doubt the extent of his foresight. Did he not, for instance, at the onset deny the contagiousness of the malady? Did he not *admit* "contingent contagion?" (Dr. Johnson here said "Never.") Dr. J. also denied the identity of this disease with the Indian cholera. As to his prediction of the moderate fatality which was to attend the disease in England, he Mr. G. believed that the verification of that assertion was to be ascribed to the judicious measures of the government, and the institution of the cholera hospitals. In conclusion, he would beg to ask the gentlemen of the "Cholera Association," whether they had as yet made any discoveries respecting the peculiarities of the disease, or found any mode of treatment efficacious. Perhaps Dr. Granville would, as a member, act the part of accoucheur to that little body, and deliver it of any offspring of its researches!

Dr. Granville objected to such an application. The Society had nothing to do with the proceedings of other Societies.

The President thought Mr. Greenwood's question not improper; but Dr. Granville declined to afford his services on the occasion.

Dr. Sigmond considered that the general voice was now decidedly in favour of non-contagion, in which opinion he himself joined from conviction acquired by personal observation. Dr. Johnson's doctrines had been greatly cavilled at; but he must say that he considered them very correct, and he was of opinion that the greatest benefits had resulted from them. Dr. Johnson was the first who had directed attention to diarrhœa as a premonitory symptom of the disease, and the importance of guarding against it. This suggestion the Board of Health took up, and it had been acted upon with immense success. The spasmodic effects of the disorder seemed not to have been present in England to any violent extent, whilst in Paris they constituted the most marked symptom, the testicles being drawn up with great force. The idea of a malarious origin, he thought really gained corroboration from the instances of Paris and London. In London we had the noble Thames, with its tides bringing fresh water and breezes, and carrying off the filth of the metropolis, conveyed into it from every quarter. But in Paris there were no sewers, the Seine was a sluggish river with no tide, and the city was without the means of speedily passing off its refuse. Most of the diseases of Paris were gastro-enteritic. In Paris, too, malaria might be expected to move with great rapidity through its clear atmosphere, whilst that of London was clogged with smoke and carbonacious matter that offered great obstacles to the quick-spreading of malaria.

Dr. Gilkrest, in reference to the Ely cases, said Mr. Greenwood had advanced what he called "facts," but had forgotten to mention any authority. He made this remark, because a gentleman from Ely had stated that they could not in any way trace contagion there. Broussais had also been adduced by Mr. Greenwood as a supporter of contagion, but if the Society would allow him, he would read an extract from the supplement to the *Constitutionnel* of April 24th, containing Broussais' lecture on cholera, which would throw quite a different shade on the matter. [Here Dr. Gilkrest commenced the extract, alternately condensing and quoting verbatim.] Well, then, he, Broussais, thinks it probable that the *peste affreuse* which ravaged the world in 1348, was of the same nature as the present disease; that the disease is said to have been preceded, the year before, in many towns in Germany, by convulsive catarrh, or the *grippe*, as it has been called; and as this latter disease prevailed in Paris last year, it was believed by those who were in the habit of observing closely, that the cholera would appear there this year. "In this military" hospital (Val de Grace), said Broussais, "we have had the 'avant couriers,' not properly the *grippe*, as but few cases of the convulsive catarrh have presented themselves here; but about five weeks before the appearance of the cholera, we observed a great *susceptibilité* in the digestive organs, so that they were obliged to withhold several articles which they were in the habit of giving to convalescents, and to renounce medicines which they had been accustomed to give as revulsives in catarrhs and peripneumonies. They had, for instance, been in the habit of giving large doses of *Tartarised Antimony* in the height of the winter, but suddenly it was found that not a single grain could be given to certain patients without serious consequences ("accidens extremement graves"). Two patients in particular were almost without pulse thirteen days after its exhibition, and became exactly like the present cholera cases, except that the pulse was not completely extinct. They were in a state of great stupidity; eyes red; extremities cold; pulse fugitive; vomiting, with frequent stools. The patients continued cold for a long time." Dr. Broussais went on to state, that the epidemic broke out suddenly amongst the poorer classes in Paris, and that the first cases at the Hospital Val de Grace, happened on the 29th March. The following were his own words: "It did not at all appear that contagion existed, for the first persons attacked had certainly not been in communication with persons from England, at least this seems probable. The disease is too "*brusque*" to have been transported in a ship. Had it been brought that way it would have been known. It would have been at Calais or some other port that a patient labouring under the disease would have been landed, and, placed in a house, would have communicated the disease to other persons. Nothing of the kind, however, has been shown to be the case. Although the first attacked do not appear to have received the disease from any body, the following has occurred:—When the disease appears in a house, it almost always attacks several persons in the same dwelling.

I do not know an example of its having been confined to one individual only. I do not doubt that instances have occurred, but at least I do not know of many instances to the contrary. When one is called to a patient, the next morning, or again the day after, two, three, or four, are found to be ill of the disease in the house. This would give rise to a suspicion of infection, ("Hear" from the contagionists)—that the disease is communicated from the patient to the attendants. But, on the other hand, the persons in the house may be considered as existing under the same influence—("Hear" from the *anties*), and consequently as contracting the disease independent of all contagion. At the same time it attacks persons living on different floors—different families whose mode of life is different, so that it would appear that in these houses something existed which multiplied the chances of an attack." Here Dr. Broussais quotes instances to show, that in the manner in which the disease spreads there is something very extraordinary. "It may seem to be transmitted by the air, but how admit this when we see it regularly distributed on the same plain, attack one village, and spare a neighbouring one, whilst there is a constant communication between the inhabitants? Those persons, too, who have, without suffering, visited the focus of the disease, become affected in their turn when the disease reaches their place." Now, then, said Dr. Gilkrest, for Mr. Greenwood's observation, or rather that part of Broussais's lecture in which he delivers the opinion by which that gentleman must have deceived himself; what says this continental physician? "From all these facts I really do not know whether I should admit what is called *infection*. As to *contagion*, it is not possible to admit it, if by this be understood contagion similar to what takes place in small-pox." Here he quotes instances of persons having been inoculated with the blood, wearing the clothes of, and sleeping with, cholera patients with impunity. In his second lecture he remarks, that the epidemic cholera morbus "is certainly propagated by a kind of *infection* difficult to characterize," — referring to the preceding explanation. Thus, said Dr. Gilkrest, we have done with Broussais; and as his authority has been granted, of course we may now claim all his weight on our own side.

Dr. Webster in reply to Mr. Greenwood's observations as to the check put on the disease by the Board of Health, said that during the great plague the regulations were much more strict than those adopted for the cholera, and yet the fatality was very different. The posting of the large red cross on the infected house, with the words, "Lord have mercy upon us," and the order to chew valerian and smell stimulants, were quite forgotten.

Dr. **Johnson** complained of the difficulty to please all parties. He must condole with his opponent upon his fortitude in standing out, now that he was losing the allies in whose support he had hitherto been strong. The Lancet and the *Cholera Gazette* now ceased any longer to advocate contagion. The *Cholera Gazette* had died—not of cholera, but the want of it; not of contagion, but the want of people to support contagious principles. As to The Lancet, it had turned completely round on the question, and now said that, after watching the disease from the Banks of the Ganges to the Weir, and considering it in that journey to have travelled certainly by contagion, the cholera was become a regular puzzle. One of the arguments adopted by that journal was a singular one; for it contended that it was probable that the epidemic influence actually becomes a preservative against contagion. After this, Mr. Greenwood might make all the use he could of the opinions of Hufeland and others.

After some remarks from Mr. Hunt and Mr. Samuel,

Mr. **King** rose to say, that although opposed to Mr. Greenwood in argument, he did not deem him incorrect in stating Broussais to be a contagionist. This opinion he founded on the difference between the English and French version of the word "*infection,*" contending that the French understood by it a modification only of contagion. If, after a long residence in France, he might be allowed to pass an opinion on the subject, he should say that Dr. Gilkrest had wrongly interpreted that part of Broussais' discourse. (The remark led to a long philological altercation, which ended in each speaker retaining the opinion with which he started.)

Dr. **Granville**, now at the close of this six months' debate, rose to put a finishing stroke to it, having been the original mover of the discussion. The whole circumstances of the disease had been raked over and over again, and the contagionists had become so evanescent, that only one voice was heard in favour of their doctrine, and now that the greater part of the Society were decidedly anti-contagionists ("No, *no*"), he thought that they ought to come to some specific declaration, from which the public might derive confidence. He would venture to propose a declaration against contagion, which might stand on the minutes as a record of his individual opinion, or have attached to it the names of those members who agreed with him.*

The **President** said that such a proceeding was out of order. The individual opinion might be recorded, but previous to adopting the other proposition, a special notice of such motion was required.

Dr. **Granville** persisted in his previous opinion, and gave the substance of the declaration proposed, as stated hereafter.

Dr. **Johnson** approved the proceeding, if allowable, and thought that every man with courage and honesty would come forward and sign such a document.

The **President** then read an extract from the rules, declaring that a week's notice was required to be given to the secretary, previous to such a motion. (In the propriety of this arrangement a great number seemed to concur), but

Dr. **Granville** cited as a precedent the proceedings adopted when the medical corporations were called upon to legislate for the practice of midwifery.

Dr. **Webster** thought it best, as on the Anatomy Bill, to move the previous question.

Dr. **Fergusson** considered that Dr. Granville would be more correct in moving for an extraordinary meeting, the more especially as so many members were absent. ("That can't be done, as this is the last night.") Well, he would only say, that as there were many contagionists belonging to the Society, no doubt there would be found among them a Duke of Wellington to adopt a "*protest.*" Was it wise, therefore, that the "declaration" should go before the public with such a rider? Would that inspire confidence?

Here a pause ensued, when

Mr. **Greenwood**, being informed that he had the right of reply, went over the objections which had been urged against him. Dr. Webster's observations respecting the regulations adopted in the plague, were, he said, well answered by the remark of De Foe, that in isolating the houses you shut up the dead, dying, sick, and healthy, altogether. There was some difference between this, and removing the sick to a place set apart for them. Dr. Johnson had accused The Lancet of apostatising, but how was he borne out in so doing, when, in the Number for that day, cases were brought forward as mathematically proving infection to have occurred?

Dr. **Johnson** said those cases were adduced by an individual in The Lancet, not by the Editor.

Mr. **Greenwood**.—No matter. They were noticed as proofs in the leading article. As to the attempt of Dr. Granville to make, or to obtain a "declaration" on the present evening, he could only denounce it as a *ruse* of the party, when many of the contagionists of the Society were absent.

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* In the course of this speech Dr. Granville indulged in some remarks on what he was pleased to call "the weathercock changes of The Lancet." We allude to this without burdening our columns with the iteration of "arguments" on other matters in which the remark was included.—Ed.

The contest for a "declaration" was again commenced amidst much confusion, and occupied a very long time. The President protested three times against the unfairness and irregularity of proceeding, but the movers persisted in "dividing the house." The house accordingly *was* divided, the contents ranging on one side of the room, and the non-contents on the other, when the following declaration was read from the chair.

"That the Westminster Medical Society, having devoted the uninterrupted space of six months to the serious and dispassionate consideration of the malady which has been prevailing in England since the latter end of September last, and especially in the metropolis; and having heard the several arguments, doctrines, and facts of the many members practically as well as theoretically engaged in that important inquiry, declare that, in the opinion of the majority of the Society, the evidence brought forward to prove the said malady a *contagious* disease has completely failed, and that every circumstance which has come to the knowledge of the Society shows the disease in question to have begun, progressed, and ended, in the ordinary way of every other epidemic disease."

After this a new point of contention arose in the shape of an amendment to *adjourn*. This being negatived by a majority of *one*, "strangers" were required to ascend into the gallery, and the division on the floor below gave

	Members.
For the declaration	24
Against it	22
Majority	2

The CHAIRMAN, and Dr. STEWART, as the other President, again entered their protests, and adjourned the meeting to that day six months.

Thus ended the CHOLERA DISCUSSION, and the session of 1831-2.

LATE PROCEEDINGS AT THE WESTMINSTER MEDICAL SOCIETY.

To the Editor of THE LANCET.

SIR,—Saturday, the 28th of April inst., was the last meeting of this session of the Westminster Medical Society, and at the conclusion of the debate on the contagious or non-contagious nature of the malignant cholera, Dr. Granville presented a declaration to the President of the Society, which has since gone forth to the world in the columns of the *Morning Post* and *Morning Chronicle* newspapers, as containing the sentiments and opinions of this enlightened body, that "The evidence brought forward to prove this malady (malignant cholera) to be contagious, has completely failed." The President very properly stated that this proceeding of Dr. Granville was irregular, and contrary to the standing rules of the Society, it being one of the rules, that due notice shall be given when the collective sense of the Society is to be taken on any particular subject. Notwithstanding this announcement from the President, the declaration was put in the midst of much noise and senseless clamour, 46 members only being present. 24 voted for it, and 22 against it, leaving a majority of 2 in favour of this precious declaration. Now the members of the once far-famed Cholera Association had collected all their strength on this occasion, but the great majority, and really intelligent portion, of the members, were absent, more particularly those who entertain a directly opposite opinion to that which this declaration puts forth, they not expecting that a subject of such importance to humanity would be disposed of by a mere "ruse de guerre," so unworthy of a grave and scientific body. Professor Burnett, Mr. Lambert, and several other members present, expressed great disapprobation at this unusual mode of proceeding. After this, yourself and readers will, I have no doubt, justly appreciate the merits of the declaration as it deserves. I do think, myself, that a meeting extraordinary ought immediately to be convened of the great body of the members, to take into consideration this marked violation of their rules. I am, Sir, your obedient humble servant,

JAMES GREENWOOD.

9, Bentinck-street, Manchester-square,
May 1, 1832.

Substance of the Investigations regarding Cholera Asphyxia, with Cases and Dissections, communicated by Professor Delpech, &c., to which are added, Observations on the Disease in Edinburgh, and the neighbouring Districts, with numerous Cases and Dissections. By JOHN LIZARS, &c. 8vo. pp. 72. Edinburgh: John Hamilton.

THE first part contains a letter from Monsieur Delpech to the author, detailing some observations on dissections made by him at Musselburgh and Glasgow. This letter has already appeared before the public, and the opinions of M. Delpech have been fully discussed in this journal. We shall now only add, that the correctness of the observations then delivered has been fully confirmed by the results of several dissections made in Paris since the appearance of the disease in that capital. The second part of the pamphlet, by far the most valuable, and deserving in the fullest degree all the praise we can bestow upon it, comprises an account of eleven dissections performed either by Mr. Lizars, or some of his immediate friends. Of these we are able to say generally, that they have been conducted with the utmost care, patience, and anatomical skill. The summary of twenty dissections gives, according to Mr. Lizars, the following results:—

“SUMMARY OF TWENTY DISSECTIONS.

Brain.

Of this number twelve had this organ examined, and, in all, the arteries and veins of the integuments and muscles covering the cranium were distended with the dark blood, which, in some, flowed like tar.

In ten, the blood-vessels of the dura mater were turgid with this blood; and in three, there were fibrinous coagula.

In seven, there was serous effusion under the arachnoid membrane.

In four, the pia mater was congested with blood-vessels.

In seven, the cerebrum was highly vascular; and in one, slightly softened.

In seven, the cerebellum was very vascular; and in three, its substance was slightly softened.

Spine examined in Ten.

In six, serous effusion between theca vertebralis and arachnoid membrane, and, in one of these, the fluid was bloody.

In two, serous effusion between arachnoid and pia-mater.

In six, blood-vessels of spinal chord highly injected with the dark blood; and one with evidence of inflammation between dorsal and lumbar regions.

In six, the spinal or rachidian veins turgid with dark blood.

Ganglionic System examined in Seventeen.

In ten, the neurilema of pneumogastric nerves was injected with blood-vessels; in one, the nerve was enlarged; in another, it was thickened; and in a third, the neurilema was inflamed with ecchymosed patches.

In six, the neurilema of splanchnic nerves was vascular; in two, the ganglia at their origins were vividly injected; and one ganglion was ecchymosed.

In sixteen, one or both of the semilunar ganglia were vascular; in one, it was inflamed; in three, it was enlarged and infiltrated with blood or serum; and, in two, softened.

In eight, the solar plexus highly vascular throughout; in three, the ganglia and nerves enlarged, and one infiltrated.

In four, the renal plexus was very vascular.

In four, the œsophageal plexuses were vascular.

In one, the recurrent of the pneumogastric nerve was vascular.

In five, the cardiac plexus was enlarged, and very vascular.

Thorax.

Heart.—In three, the heart was flabby and pale; in two, collapsed; and many of them had the left ventricle so contracted and firm, as to contain only a drachm of blood. In thirteen, the right side was full of the dark gory blood, part of which was generally in the state of a fibrinous coagulum.

In three, the left side was full of the same blood with coagula.

In three, the right auricle full of dark blood and coagula.

In six, the left auricle full of dark blood and coagula.

In four, left ventricle was moderately filled with blood and coagula, and one affected with softening; in two, coagulum extended into aorta.

In five, right ventricle full of blood and coagula. In one, coagulum extended into pulmonary artery. In two, the parietes were softened.

Pericardium.—In one, this sac was disthended with gas; in two it was dry, like paper, and vascular; and, in a third, dry, vascular, and diaphanous. In four it was vascular. In all, the coronary vessels more or less injected with dark blood.

Venæ Cavæ.—In all, more or less of the dark blood was found.

Pulmonary Veins.—In six, these veins were turgid with the dark blood.

Lungs.—In four, these organs were congested with the dark blood.

Pulmonary Artery.—In one, a large coagulum, which extended into its two large branches. In three, it was full of the dark blood, and in three others the vasa vasorum were highly injected.

Pleura.—In five, highly injected; and in two, there was effusion of lymph.

Aorta.—In all, it contained more or less dark blood, with fibrinous coagula; in six, the vasa vasorum were highly injected—the dark blood, and occasionally coagula, extended into the carotid, brachial, femoral, tibial, ulnar, and radial arteries.

Abdomen.

Peritoneum.—In nine, this membrane was highly injected; in six, evidently inflamed; and in three, there was albuminous effusion, with some turbid serum. In one, the omentum was very vascular, and in another it was inflamed.

Stomach.—Generally of a white colour, both on its peritoneal and mucous tunics, and containing more or less of the rice-water fluid. In seven, there were distinct vascular patches on the mucous coat, with several ecchymosed spots, varying in size from that of a sixpence to that of a half-crown, and in all there was manifest softening. In one, the mucous tunic was eroded.

Small Intestines.—In twelve, there were evident marks of high inflammation, and vivid and extensive injection; in nine, ecchymosed patches; in four, mucous tunic softened in many points; and in one, incipient ulceration. Contents of a viscid white mucous, or greenish colour, and in two they were bloody.

Large Intestines.—Transverse arch and sigmoid flexure of colon, commonly spasmodically contracted. In five, vascularity, with ecchymosis. Two inflamed, with softening, and one with ulceration. Two with dark venous congestion, similar to intestine in strangulated hernia. Contents generally rice-watery, or gruelly and flocculent, occasionally greenish and viscid. In many the colon, with the exception of the caput cæcum, was empty.

Liver.—Very various in colour; two with Bright's yellow deposit. In some the vena portæ were moderately congested, and in one the biliary ducts full of bile.

Gall Bladder.—Generally two-thirds full of rather inspissated olive-green bile. In the twenty cases ten were full of this fluid, the others varied from a little bile to two-thirds.

Pancreas.—Generally healthy.

Kidneys.—Commonly healthy, but varying like the liver, according to the habits of the individuals. Three were slightly congested, one gorged with the dark blood, and another presented a livid appearance.

Urinary Bladder.—In all contracted, and almost empty. When any fluid was present, it was about a drachm of muco-purulent. One however was contracted horizontally, and contained five ounces of limpid urine."

The third portion of the pamphlet contains Mr. Lizars' "Theory of Cholera Asphyxia." We shall endeavour to collect his ideas on the subject, but must confess at the outset, that many passages, apparently embracing the pith and marrow of his doctrines, are to us perfectly unintelligible. Mr. Lizars is a good anatomist, but he expresses himself in very obscure and unscientific terms when physiology or pathological induction form the subject of his discourse. The atmosphere, according to Mr. Lizars' theory, is unseasonable, and predisposes the constitution of the individual for some time before he is attacked, by impairing the whole nervous system, but especially the ganglionic system of nerves. A deficiency of nervous energy to the important organs supplied by the ganglionic nerves takes place, the chylopoietic viscera become deranged, and the individual is thus prepared, on exposure to any exciting cause, as cold, &c., for the reception of the disease.

"The expansive cutaneous filaments of the nerves are now *chilled* and *excited*; the cutaneous capillary exhalants are shut up; these extreme branches of the nerves and arteries produce a concentration of the nervous and circulating systems; re-action is attempted, and on its failure, a determination is produced to the ganglionic system of nerves.

An *inflammatory spasm* of these nerves and their organs is the result; and hence the sluggish respiration and circulation, the decomposition of the blood, &c.

The vomiting and purging are consequent on the inflammatory spasm combined with an overloaded condition of the whole alimentary canal, which must be severely irritated by these ingesta; and hence diarrhœa is almost always a precursor of the violent symptoms.

To prove that inflammatory spasm is the cause of the vomiting and purging, (independent of what has been seen in dissection,) we find that, as the disease advances—as the inflammation continues and destroys the nervous and circulating systems supplying these organs—so the vomiting and purging, or dejections, cease.

The cramps are consequent on the same inflammatory spasm being communicated through the medium of the ganglionic nerves along the arteries, and from the communication of these nerves with the spinal chord and nerves of volition. This is beautifully but painfully exemplified in Mrs. M——'s case, where pressure on the abdomen produced tetanic convulsions of the muscles of the neck and also of the lower jaw.

The blueness of the skin is consequent on the blood not being propelled, with its accustomed vigour and frequency, through the lungs by the heart, and also and chiefly by its not being duly oxygenated.

The coldness is consequent on no caloric being generated in either the capillary pulmonary tissue, or the cutaneous capillary tissue, and by the blood not being arterialized, but remaining carbonized."

The blueness of the skin and coldness are consequent on the diminished action of the heart, the non-oxygenation of the blood, and the non-generation of caloric in the cutaneous or pulmonary capillary tissues.

Such is an abstract of Mr. Lizars' theory; but we have read the original over and over again without being able to comprehend what he means by chilling and exciting the cutaneous filaments, or by his inflammatory spasm of the ganglionic nerves; physiologists have commonly thought, that chilling the expanded extremity of a nerve produces an effect quite opposite to excitement; and inflammatory spasm, communicated along the arteries to the muscles, and thus producing cramps, purging, &c. is an expression to which we can attach no definite idea whatever. It is remarkable also, that Mr. Lizars, in common with Loder and Delpech, has neglected to point out what the anatomical condition of the sympathetic is indicative of in this severe affection. His expressions all point to functional derangement, not to organic change; in one part indeed (p. 45), he speaks "of a determination to the ganglionic system of nerves;" but the reader is unable from the context to decide whether he alludes to a determination of blood, or of nervous energy. Let us, therefore, turn to the general summary of appearances to solve the problem. What is the pathological cause of cholera? Is it some morbid affection of the central system of ganglionic nerves, as Delpech, Loder, and Lizars think?

We may remark, in the first place, on the vagueness of the phrase, "morbid state," "morbid condition." Is it inflammation, hardening, ramollissement, infiltration of the substance, or all or any of them together? Are we justified in concluding that inflammation of the semilunar ganglia will give rise to the same external symptoms, as simple and frequently slight venous injection of their neurilema. Again, it may be asked, Is this an appearance universally found? For on this the whole question hinges. Abercrombie has remarked, that the progress of medicine, as an inductive science, is retarded by the construction of hypothetical theories, or the assumption of principles which are altogether gratuitous and imaginary, and also by the deduction of general principles or conclusions, from a limited number of facts. The same scientific writer wisely advises medical inquirers to restrict their investigations to ascertaining the universality of a fact. Let us then try the theory of Mr. Lizars by this test, and from his own testimony, "The ganglionic sytem was examined in seventeen subjects. In sixteen, one or both the semilunar ganglia were vascular." Granting this vascularity of semilunar ganglia to have produced the disease in sixteen, what was the cause of death in the seventeen? What becomes of the universality of the fact, and of Mr. Lizars' theory? The indeterminate meaning of the word vascularity has been noticed already, and receives a good commentary from the account of the tenth case, from which we extract the following passage:—

"*Nerves.*—Par vagum, splanchnic, and semilunar ganglion on each side, examined, but no very unusual appearance observed in them, except that the small vessels supplying them partook of the general injection; lungs of rather a dark colour."

This, we conscientiously believe, affords a key to many of the other cases; the injection of the neurilema was not a mark of specific affection, but only one of many contemporaneous events. The agency of the injection of the nervous neurilema has been assumed by Reil,* as the efficient cause of typhus fever, because he found this appearance in many of the bodies which he examined. The parallel is close, and a short period will suffice to consign both theories to the same tomb.

Hyperanthraxis; or, the Cholera of Sunderland. By WILLIAM REID CLANNY, M.D., &c. 8vo. pp. 149. Whittaker: London, 1832.

OF all the plagues and pestilences of modern days, not even excepting the multinominal yellow fever, we scarcely recollect another so oft baptized, and with all so inappropriately designated, as *the* cholera. We mean that of Asia, of Warsaw, of London, of Paris, &c. For in order that we should clearly understand of what we are writing, we must combine the term with some such local appendix, just as the crowds of synonimous Mackenzies, and Frazers, and Macconochies, in Caledonia, assume the "title" of their estate, kail-yard, or other holding, as a necessary mark of distinction in society.

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* Fieberlhre Band, iv. p.56.

Already in an article of some length, we have taken the liberty of impeaching the earliest sponsors of this disease, for the extraordinary contradiction which the name they gave it, afforded to its most prominent symptom. Strange as it may seem, though this absurdity was generally acknowledged, every subsequent reformer still fell into an analogous error. A few examples of these misnomers we may repeat, if not for the advantage, at any rate for the amusement, of our readers. MARCUS, of Moscow, for instance, having performed a cholera autopsy, and found the heart flaccid and enlarged, forthwith wrote an essay to prove that the heart was the seat of the disorder, at the same time naming it "*Cardiogmus lethalis!*" M. SCIPION PINEL, having, after similarly extensive experience in Warsaw, found marks of inflammation of the trisplanchnic nerves, invented the term "*trisplanchnitis,*" as the substitute so much desired. A third luminary of great literary and etymological notoriety, contended that "diarrhœa *cephalica*" (!!) should supersede all other appellations—and now we find our friend Dr. CLANNY, with almost unparalleled euphony, fishing in the depths of classic lore for a name, and hooking out, after many laborious experiments, much contemplation, and very cautious induction, the epithet "hyperanthraxis," which he explains in these introductory terms:—

> "I have ventured to give an appropriate name to this pestilence, to which I am prompted by my own experiments. I expect that it will be received by the medical faculty instead of "cholera," literally a flowing of bile, though in this disease when formed no bile flows as far as I have been able to ascertain from personal observation. 'Hyperanthraxis' ex *υπερ* super, et *ανθραξ* carbo, Anglice, excess of carbon."

Our readers doubtless experience a degree of curiosity fully equal to our own, to ascertain the experimental facts on which this assertion is founded. By these, of course, our estimation of the peculiar opinions which the author subsequently advances in connexion with the carbon hypothesis, must be guided. They are contained in the following passages:—

> "After much reflection upon the subject of FREE carbon, I devised and carried into effect the following method for discovering its proportion in the blood of different individuals; and if a multiplicity of experiments which uniformly accorded, not only with sound pathology, but also with each other in their results, be considered as deserving the confidence of the profession, I feel satisfied that this plan of investigation will be found not only worthy their approbation, but also suitably convenient to such practitioners as may not be in the habit of performing chemical experiments.
>
> "Having carefully collected the colouring matter in the manner which I formerly published, and having dried and powdered it, I place it in a crucible, to which is closely fitted a cover, out of the centre of which issues a small tube with a stop-cock. The apparatus is made of platinum, and is perfectly air tight.
>
> "Having a charcoal fire in the furnace in readiness, I fill the crucible with the powdered colouring matter, after which I remove it to the scale to weigh it. When it is returned to the crucible, leaving the stop-cock open, I gradually heat the crucible till the gases are extricated. I now shut the stop-cock, continue the process till the crucible arrive at a white heat, and keep it in that state for a few minutes. I now remove the crucible from the fire, and find in the crucible when cold, pure carbon and a bulky cinder. I wash, dry, and weigh the carbon.
>
> "I weigh the cinder, break it into fragments, submit it again to a white heat, and afterwards collect the carbon, if any, and add it to the above-mentioned quantity. In this way I meet with no difficulty in ascertaining the nature of blood as to its FREE carbon, whether the patient be affected with epidemic cholera, or any other disease. * * * * I added to the platinum tube a bent brass tube, and in a quicksilver pneumatic apparatus, collected the gas from the colouring matter of the blood as it arose. Upon analysis I found that this gas was composed of carburetted hydrogen three parts, and of carbonic acid one part."

In commenting on this process, we address ourselves especially to those who are conversant to a certain extent with chemical literature and manipulation. To such we shall satisfactorily prove, that these experiments not only totally fail in showing FREE carbon in the blood, but that they are inconclusive in every other particular.

It is necessary in the commencement to state, that according to the researches of Brande, Engelhart, and Michaelis, whose experiments have established the fallacy of many of Berzelius' opinions relative to the colouring matter of the blood, that principle consists of an animal matter, associated with iron in a minute quantity, and with some earthy salts. The animal matter according to the minute and excellent analysis of Michaelis consists of, in venous blood—

Carbon....	53. 231	Hydrogen	7. 711
Oxygen ..	21. 666	Nitrogen..	17. 392

The precise mode in which these elements are arranged, whether in binary, ternary, or quaternary combination, has never yet been shown, nor is the problem indeed one of the least importance. Previous to the researches of Dr. Clanny, however, no *chemist* ever pretended that one of these four elementary substances existed in a *free* state, that is, as oxygen, hydrogen, nitrogen, or carbon,' not combined with one or more of the others. Now Dr. Clanny states, that he has proved the existence of *free* carbon, by burning the colouring matter in a close vessel, and finding a carbonaceous residuum in the retort, and *carburetted hydrogen*, and *carbonic acid*, in the receiver.

The following remarkable faults occur in this analysis :—

In the first place, Dr. Clanny only gives carbon, carbonic acid, and carburetted hydrogen, as the products of his experiment, whereas colouring matter containing *nitrogen*, that gas, or some of its compounds, such as ammonia (hydrogen and nitrogen) or cyanogen (nitrogen and carbon) must be procured if the process be correctly performed. But neither nitrogen nor any of its compounds having been obtained, we are forced to conclude that the analysis was altogether imperfect.

In the next place, animal matter, when subjected to this destructive distillation in close vessels, always yields empyreumatic oil and water; Dr. Clanny obtained neither of these inevitable results.

Thirdly, carbon being the only *fixed* ingredient of the elements of the organic portion of colouring matter, it must remain behind when exposed to heat. Consequently a carbonaceous residuum in the retort is no proof of *free* carbon existing in blood, any more than the obtaining of common charcoal by distilling oak-wood in close cylinders, proves that FREE carbon existed in the pieces of timber before they were burned.

Fourthly, The quantity of carbon in the colouring matter having been proved by Michaelis to be 53¼ parts per cent. in standard or healthy blood, great excess in the proportion of that element is a *natural* fact totally unconnected with disease.

Fifthly, Even the results of Dr. Clanny's experiments, compared with the standard just cited, indicate not an excess, *but a deficiency, of carbon*, in cholera blood. Thus he finds in one specimen of cholera blood,

Colouring matter .. 253 per 1000 parts;
Free carbon ...... 66 per 1000;

which gives a proportion of one-fourth, and a fraction, $\frac{55}{66}$ only, to the sum total of the colouring matter,—being not an excess, *but a deficiency of more than 25 per cent.*

The preceding comments, and we might multiply them much farther, are sufficient to show that the experiments of Dr. Clanny are not conclusive—that if they prove any thing, they prove the opposite of his opinions, and that the name he has furnished for the disease must be rejected.

Having thus demonstrated the premises of the author to be altogether erroneous, we cannot of course take into our estimation any of the conclusions which he derives from their consideration.

"I have witnessed (*Lizars' Pamphlet*) many a contagionist medical man feeling the skin and pulse of the patient with gloves on his hands, a handkerchief over his mouth, and a scent-bottle at his nose, and stopping as short a time as possible with him. To do justice to a cholera patient, the medical attendant must remain with him for an hour or two, and put his own hand to the work. Can the contagionist do so?"

Simple man! Yes.

THE LANCET.

London, Saturday, May 5, 1832.

The recent epidemic, having in its sojournment in this city gone far to disprove many of the hypotheses to which it had before given rise—having baffled alike the theories of the aërialist, of him who placed his faith on terrestrial emanations, of the animalculist, the electrician, and the astronomer, has shown us at least thus much, that while we must admit the operation of a powerful contagion, and while some circumstances denote the simultaneous action of an unknown and more general agent, there was at any rate a state of constitution abroad which defended the mass of our citizens from either, or both, of these intrinsically destructive poisons.

Take up which side of parties, which view of debated questions, we may, let us become for the moment the most rancorous of the non-contagionists (if any such remain) or the most raving of the *lunatics* who blame the fair moon for the working of all this mischief, one thing is at least clear and palpable, that it is quite as difficult to understand why of twenty persons residing in the same house, and under apparently similar external circumstances of air, and food, and raiment, habits, health, and occupations, two should suffer, and eight-and-twenty should escape. If this circumstance opposes the opinion of contagion, it is plain, that it is quite as adverse to any other speculation concerning the causes of the disease. These inferences then we must all adopt, that the causes of immunity or insusceptibility on the one hand, and o' the predisposition so remarkable on the other, form a problem for investigation, at the same time most attractive in its nature, and the solution of which is most pregnant with one advantage to mankind, of all the questions which have ever engaged the attention of the medical philosopher.

To examine inductively the nature of the causes which protected the citizens of London from the ravages of the cholera, and the absence of which laid open the inhabitants of Paris to its fury, or conversely to examine the nature of the causes which favoured the operation of the pestilence in Paris, while their absence diminished its mortality in London, are questions of which our scientific societies and the medical journals might undertake the discussion with much advantage. Although an absolute solution of the question might not be arrived at, this good would at least be accomplished, that provided the discussion were conducted according to the rules of common reason and common sense, and in the mode we shall presently point out, the influence of many agents now held to be of great importance would be correctly ascertained, and we should learn at least the amount of our own ignorance on the subject.

An investigation of this kind should, we consider, be thus conducted. Supposing a society formed for the purpose, a catalogue should first be drawn up, embracing and classifying all the known external agents by which communities visited by the disease have been surrounded, such as the state and kind of air, food, habits, customs, &c. On each of these heads discussion should take place seriatim. The statistical history of the disease would readily furnish ample materials to decide on the individual questions, and then another head should be taken up. Proceeding thus by the admission or rejection of probable causes, the ultimate question would be narrowed into a circle of very small diameter, and the chances of successfully arriving at the truth would be proportionately encreased.

We have been partly urged to the preceding train of reflections by the following extraordinary, and to us most unaccountable, fact. Such of our professional readers as have visited Edinburgh will readily recollect the local position and external circumstances of the fishing parts of the villages of Musselburgh and Newhaven. They will remember the peculiar, almost Flemish, costume of the women, the similar and curious construction of the houses, the apparently identical habits and customs, the approximation of each locality to the Frith of Forth, the similar direction of the streets, and the common occupation of the inhabitants. They will recollect also the extensive intercourse between the two villages, and then they will, perhaps, participate in our wonder at the circumstances, that while Musselburgh has almost been desolated by the cholera, not a single case has occurred at Newhaven!

On the proper investigation of this astonishing fact, we would stake much of the probability of a solution of the questions we proposed. Either *the cause* of the malady must never have visited Newhaven, or the inhabitants were not susceptible of its influence if present. But that the cause did visit Newhaven can scarcely be denied by those who look at the general progress of the cholera in Scotland. Some difference, then, not appreciable to ordinary perception, existed among the *circumstances* or *inhabitants* of the two places, owing to which "one was taken and the other left." What then is the difference? To the discussion of this question we will gladly open our columns, and we invite to it the immediate attention of our Scottish correspondents.

The proceedings at the rookery in Sackville-street have at length terminated, and, luckily for the lungs of the chief ravens, without any of the pulmonary calamities which their perpetual and uproarious cawings gave us reason to anticipate.

Encouraged, perhaps, by the speedy approach of the end of the session, one of the noisiest of the aviary took leave to declaim on this occasion on our "weathercock" doctrines, and reproach us with veering round to *his* opinions. The reproach does credit to the orator's candour and self-appreciation. We are glad that he can so rightly estimate the amount of respect his opinions should obtain. As, however, he spoke under the influence of misapprehension, and as we quite agree with him as to the little honour to be gained by a participation in his sentiments on the subject, we beg leave to revert briefly to the opinions we discussed in the leading article of the last Lancet, relative to the mode of propagation of the cholera lately prevalent in London.

In that article, in an honest and upright spirit, which the person alluded to evidently could not understand, we adduced, at the same time, irresistible evidence of the operation of contagion; and we also particularised some strong facts indicative, by negative evidence, of the operation of some other cause. Never accustomed to shape, bend, or distort facts to suit a partisan-purpose, we gave both selections their full value, and deduced our conclusions accordingly; and these conclusions we showed, by reference to our very earliest articles, to be fully consistent with the principles we ever promulgated. We ceded not one jot from any position we ever maintained. In our first article we held the disease to be communicable by contagion. In our last we adduced the most powerful evidence in support of the same doctrine. In our first we admitted the operation of epidemic influence at the time when we wrote. Subsequently, never yielding or modifying that sentiment, we maintained that the progress of the malady during the autumn and early winter months of 1831, was inconsistent with epidemic influence at the time, and referable to contagion alone. We hold this doctrine now in its fullest and most determined sense. Lastly; at the return of the spring, and with the remarkable alterations just shown in the geographical history of the disease, we again deemed it rational to admit the possible agency of a mysterious influence. Where is the apostacy or wavering in this? If the seasons alter, and diseases obey their phases, are we accountable for the event? Are we culpable for keeping an honest record of the things which fall before our observation? We content ourselves, then, with saying, with reference to a charge originating only in the ignorance of our accuser, that we are fully prepared to maintain every opinion we ever advanced on this question. We can do this easily, for our opinions were ever deductions from authenticated facts. These premises still remain for our service, and they admit of no other conclusions than those they have already afforded.

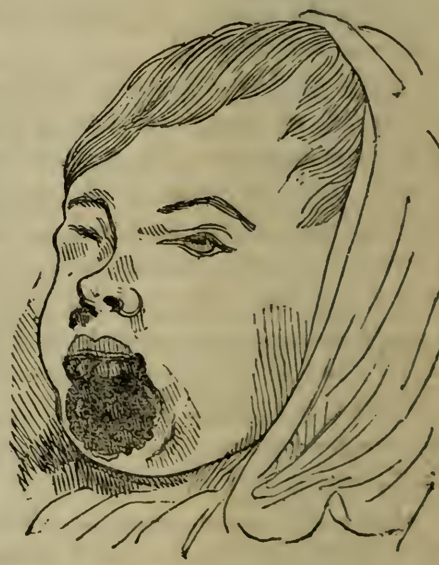
We shall enter into no further argument with this individual. Indeed it is hard to reason with any one who will not take the trouble of studying the subject he declaims on, and who but six months since was so proficient in pathology as to call the petechial hæmorrhage of typhus a cutaneous eruption! and was so skilled in the geography of cholera—that disease which has prevailed on the Himalaya mountains, the Gauts, the Nepaul, and the Caucasus, as to maintain that it was the exclusive tenant of the swamp, morass, and similarly low situations. Recommending this gentleman then to the perusal of a few elementary works on cholera during the summer vacation, we leave the subject for a week.

WESTMINSTER HOSPITAL.

MORTIFICATION OF THE MOUTH FROM THE ABUSE OF MERCURY.

William Somerville, a child about three years old, was admitted under Dr. John Bright, with a gangrene of the mouth and chin. The little patient was brought in by his mother, who stated that this and a younger child of hers being affected with fever, she was induced to consult a young medical gentleman whose washerwoman she was. Twelve white powders were prescribed for each child, to be taken within twenty-four hours. After taking them, the children's faces became swollen, and much water ran out of their mouths, and the breath grew very offensive. In the case of the child admitted, the gums became inflamed and black, and some of the teeth fell out. Three days from the first administration of the powders, a small black spot appeared between the lower lip and chin, this gradually enlarged until it attained the size of a half-crown piece.

On the fourth day, application was made to one of the parochial surgeons in this city, but his numerous avocations prevented him from attending regularly to the case. The symptoms in consequence became more and more severe, and on the fifth day the parents were persuaded to bring the children to the Westminster Hospital.



At this time, the sphacelus on the face of the boy had increased to the size of a dollar, while the adjacent skin, including that of the upper lip, was of a dusky-red hue, becoming lighter towards the external margin of the phlegmonous circle. The subcutaneous veins were turgid, and formed dark-blue zig-zag lines extending over the surface of the throat and chest; all the teeth had fallen out, and the odour was intolerable. The inflammation affected the mucous membrane, there was frequent cough, excessive irritation and restlessness, and the little fellow moaned constantly. His hands were secured, a carrot poultice was applied, and wine and gentle aperients were administered.

March 29th.—The mortification is extending on the right side; the right eye is closed; countenance sunken and anxious; great restlessness; head constantly moving; thirst; respiration rapid; cough short, dry, and frequent; diarrhœa. A large bread poultice, moistened with a chloruret of soda lotion, is applied, but is with difficulty kept on. Wine and water for drink, opiate and cordial medicine.

Evening.—The sphacelus has spread; anxiety of expression; prostration of strength; occasional restlessness. The child is evidently sinking.

The younger child was attended at home by some of the medical officers of the Hospital, and died a day or two afterwards.

Autopsia twenty-four hours after death.

The boy was not much emaciated by his malady or his treatment. The external slough involved the whole of the chin, under lip, and part of the upper lip. On cutting down upon the inferior maxillary bone, it was found denuded of its periosteum, as far back as the root of the coronoid process, black, and perfectly detached from the muscles and mucous membrane of the mouth. There were no gums on either jaw, and in the upper one a few teeth were found loose in the alveoli. The slough included the integuments and muscles of the entire under lip and part of the upper lip, the periosteum, gums, mucous membrane, and about a line's thickness of the muscles forming the root of the tongue. Here there was an evident line of demarcation. No swelling or redness of the pharynx or tonsils was observable. The epiglottis round its edges only presented a vivid red tint. All the salivary glands were tumefied. The tracheal and œsophageal internal linings were quite unaffected.

The lungs appeared to have been formerly subject to inflammation, for several solidified nuclei were discovered pretty equally dispersed throughout the substance of both these organs. The ramifications of the bronchiæ were pretty generally filled with mucus, but no injection of their inner tunic existed. The heart was healthy.

In the abdomen there was a disease of the mesenteric glands, and all the organs were apparently in their natural condition. In the head there was no morbid appearance, except a slight injection of the vessels of the pia mater, most perceptible over the medulla oblongata and upper portion of the spinal marrow. No opportunity was afforded of opening the body of the other child.

Gangræna oris is a disease of which but very meagre accounts are to be met with in the annals of medicine. It is to be feared, however, that its occurrence is far from infrequent, especially in parochial and dispensary practice, where the inadequacy of remuneration to medical attendants renders instances of neglect inevitable. In parishes the medical attendance of the poor is *farmed out* in the most unprincipled manner, the appointment of surgeon being almost invariably given to the lowest bidder. In the country this is a greater evil even than in town, for it is not uncommon to find medical men contracting to give medicine and advice to a whole parish for a sum of 10*l.* sterling per annum. In the more populous parishes of London, we believe that occasionally as large a sum as 100*l.* per annum is allowed to the medical attendant for his services. But no surgeon can be expected to furnish himself with the common necessaries suitable to his station in life from such a sum, and the demands of his private practice upon his time leave but little of it to be devoted to pauper patients.

Those pseudo-charities, called dispensaries, are even more baneful in their effects than is the system of parochial doctorships, for in nine cases out of ten they originate as medical speculations. A few of the more impudent amongst the adventurers succeed by this means in raising themselves to a certain degree of notoriety, if not of reputation, heedless of the injury they inflict on the interests of their more honest professional brethren. The sick poor, in fact, will never be properly treated until after a perfect reform of the poor laws, under which medical officers of acknowledged ability may be ensured a sufficient remuneration to allow of their devoting the whole of their time to parochial medical duties.

To return to the subject more immediately before us. There is hardly to be found on record a single case of gangræna oris in children, and ascribed to its true cause, the excessive administration of mercury. In the *Dictionnaire des Sciences Medicales*, some cases of sloughing in adults, after the exhibition of this mineral, are stated by Cullerier. Baron Van Swieten, in his Commentaries on Boerhaave, notices a case in some respects analogous to the one just related, but he refers it to a *scorbutic* origin. A somewhat similar case is also quoted by Dr. James in his Medical Dictionary. The words of Van Swieten are as follows:—

"Si autem multum tumeat gingiva, simulque admodum rubeat validæ inflammationis signum est; quæ quandoque in gangrænam satis cito terminatur; præcipue si acrior humorum indoles simul adsit. Cito autem gingivæ sic affectæ putrescunt, et malum ad vicina loca propagatur, nisi spiritu salis misto cum melle rosarum sæpius tangatur locus gangrænosus cuique sistatur proserpens putredo. *Vidi aliquoties in pauperum infantibus, qui omnino neglecti fuerant,* *partem ossis maxillæ cecidisse, una cum alveolis et dentibus contentis.*"

A well-marked case of gangræna oris in a child, arising from the abuse of mercury, occurred in the practice of Mr. Broughton two or three years since at the St. George's and St. James's Dispensary, the particulars of which have never been published. A similar case also was met with by Mr. Edwin Thomas, of the Ophthalmic Hospital. Mr. Thomas has copious notes, and intends to give a history of the case in one of the journals. The peculiar sensibility and delicacy of structure of the gums and teeth at this early period of life, throw a great deal of interest on this department of pathology; and there are, doubtless, many intelligent surgeons who possess valuable facts relating to it, which as yet have not been communicated to the public.

SURGEONS TO SMALL VESSELS.

To the Editor of The Lancet.

Sir,—From the zeal which you have on all occasions manifested in upholding the honour of the medical profession, I feel assured it is only necessary to apprise you of an abuse of a late order from government,—that requiring all vessels sailing with upwards of fifty souls to be provided with a "properly-qualified surgeon,"—in order to find a place for some remarks on it in the pages of The Lancet. It is expected by many captains that medical men will give their services in consideration of a free cabin passage only being granted for them, and this I am grieved to say has on one or two occasions been acceded to. It is an abuse too flagrant to require comment. Surely, Sir, in medicine of all things, the servant is worthy of his hire, and I leave it to the consciences of honest men to say whether eight pounds per month is too much for the services of a *properly*-qualified surgeon.

For once, Mr. Editor (almost a solitary instance), we have the remedy in our own hands. It is our own faults if we use it not. Let the members of the College, or other properly-qualified persons, resolve amongst themselves, one and all, not to go for less, and we must be successful.

I am, Sir, yours, &c.,

C. E.

SOUTHWARK CHOLERA HOSPITAL.

Veritas begs us to contradict an assertion which appeared in our last week's journal, relative to the medical attendants at the St. Saviour's Cholera Hospital, Redcross-street (not Winchester-street). "Candour," he says, "compels me to state, that Messrs. Gazelee and Tweedie are not 'indefatigable gentlemen;' at the onset of this disease, they did assist for two days, but since that period Mr. Morley and Mr. Ranking have been actively engaged there: the former as resident surgeon to that hospital, the latter a house pupil with Dr. Whiting, attended without remuneration till he was deterred by contracting the epidemic. Let us adhere, therefore, to the old motto, *Palmam qui meruit ferat.*"*

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

To the Editor of The Lancet.

Sir,—I shall esteem it a particular favour, if you will allow me to occupy a corner of your valuable Journal in correcting an error which has crept into Dr. Robertson's paper on the salts of morphia in the last Number of the *Edinburgh Medical and Surgical Journal*, through an inadvertency on my part.

Dr. Robertson, in mentioning a communication from Dr. Loudon, describes him as practising in Cheltenham, whereas Leamington is the place which should have been mentioned. Now as this mistake might prove injurious to Dr. Loudon, and as I was not aware of it until too late to correct it in the Journal, or even to insert it as an erratum, I have taken this method to counteract any effects which might arise from it prejudicial to Dr. Loudon. I remain, Sir, yours, &c.,

William Gregory, M.D.

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* The above notice should have appeared some weeks since.—Ed. L.

TO CORRESPONDENTS.

A Pupil of the Little Windmill Street School must send us his name (in confidence) before his letter appears.

The letter of *Mr. Clay* reached us too late for insertion in *this* number.

The report of the meeting at the College of Physicians next week.

The request of our much respected correspondent at H., relative to a stamped Number of The Lancet, could not be complied with, because a stamped edition has not been published for many years.

(From a Correspondent.) On Saturday, the 14th instant, a piece of plate was presented to Mr. Alex. Jardine Lizars by his pupils at the Medical School, Brown Square, Edinburgh, on the occasion of his first course of lectures on anatomy, and in testimony of their satisfaction at his peculiar system of teaching that science.

Querist will never get a "satisfactory" answer from *this* quarter. He should apply to his Grandmamma.

Observer. Why so? A thin rat may be worth *killing*, though not worth *skinning*. But Michael's *brains* are *not* in danger.

THE LANCET.

Vol. II.] LONDON, SATURDAY, MAY 12. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY

MR. YOUATT.

Lecture XXIX.

PLEURISY IN THE HORSE, CATTLE, AND DOGS.

I must request your attention this evening to another of the diseases of the chest, scarcely yet recognised in our miserable nosology,—Pleurisy, or inflammation of the investing membrane of the lungs, or the lining one of the thoracic cavity. That inflammation of the substance of the lungs is often propagated to the investing membrane, and that both are at length equally involved, is not disputed. The frequent occurrence of effusion in the chest is an unanswerable proof of inflammation of the serous membrane which it contains; but the confinement of the inflammatory action strictly to the pleura is denied, I believe, in our principal school; and although the possibility of it and its supposed rare occurrence is reluctantly admitted by our best writers, they scarcely enter into the consideration of it, and Mr. Blaine alone deigns to give an outline of its characteristic symptoms. We are principally indebted to Mr. John Field for establishing the fact of the frequency of the malady, and for enabling us to distinguish between it and pneumonia, as readily and as surely as we do between pneumonia, and bronchitis, and epidemic catarrh.

Causes.—The prevailing causes of pleurisy are the same as those which produce pneumonia—exposure to wet and cold—sudden alternations of temperature—partial exposure to cold—riding against a keen wind—immersion as high as the chest in cold water—drinking cold water, and extra work of the respiratory machine. To these may be added, wounds penetrating into the thorax and lacerating the pleura, violent contusions on the side, fracture of the ribs, the inflammation produced by which is propagated through the parietes of the chest.

Extent.—It is sometimes confined to one side, or to one of the pleuræ on either side, or even to patches on that pleura, whether pulmonary or costal. The inflammation of the lungs which occasionally accompanies rabies is characterised by a singular patchy appearance. That produced on the costal pleura, arising from violence or other causes, rarely reaches the pulmonary covering; and that which is communicated to the tunic of the lungs, by means of the intensity of the action within, does not often involve the costal pleura. In some cases, however, it affects both pleuræ and both sides, and spreads rapidly from one to the other.

Symptoms.—The principal knowledge of these, besides those derived from personal observation, I owe entirely to Mr. Field. The profession generally is much indebted to that excellent practitioner. Our continental neighbours appear, however, to be well acquainted with disease, and Hurtrel D'Arboval gives a fair account of it, whose work generally will amply repay your perusal of it. The first symptom is rigor, followed by increased heat and partial sweats; to these succeed loss of appetite and of spirits, and a low and painful cough; the inspiration is a short, sudden effort, broken off before it is fully accomplished, indicating the pain felt from the distension of the irritable, because inflamed, membrane. This symptom is exceedingly characteristic. In the human being it is well expressed by the term *stitch*, and an exceedingly painful feeling it is. The expiration is retarded as much as possible by the use of all the auxiliary muscles which the animal can press into the service, and yet it also is at length cut short—it finishes abruptly in a kind of spasm. This peculiarity of breathing once carefully observed cannot be forgotten. The next character is found in the tenderness of the sides when the costal pleura is affected. This tenderness often exists to a degree scarcely credible. If you press upon the side, the horse will recede with a low painful grunt; he will tremble, and try to get out of the way before the hand touches him. Then comes another indication, both of pain and the region of that pain;—the intercostal muscles, affected by the contiguous pleura, and in their turn affecting the panniculus carnosus without there are twitchings of the skin on the side,—corrugations—waves creeping over the integument. This is never seen in pneumonia. There is, however, as we may expect, the same disinclination to move, for every motion must give intense pain.

Difference between the Symptoms of Pleurisy and Pneumonia.—Pulse. The pulse should be anxiously studied. It presents a difference of character sufficiently satisfactory. It is increased in rapidity, and instead of being oppressed and sometimes almost unappreciable, it is round, full, and strong. Even at the last, when the strength of the constitution begins to yield, the pulse is wiry, although small.

Temperature.—The extremities are never deathy cold; they may be cool, they are oftener variable, and they sometimes present increased heat. The body is far more liable to variations of temperature; and the cold and the hot fit more frequently succeed each other. The mouth is not so hot as in pneumonia, and the breath is rarely above its usual temperature.

The Membrane of the Nose.—A difference of character in the two diseases is here particularly evident. Neither the crimson nor the purple injection of pneumonia is here seen; but a somewhat darker dingier hue, yet not to any great degree, is exhibited on the membrane of the pleuritic horse.

General Appearance and Action.—Both the pneumonic and pleuritic horse will look at his flanks, pointing out the seat of disease and pain; but the horse with pneumonia will turn himself more slowly round, and long and steadfastly gaze at his side—the action of the horse with pleurisy is more sudden, agitated, spasmodic. The countenance of the one is that of settled distress, that of the other brightens up occasionally; the pang is severe, but it is transient—there are intervals of relief. While neither will lie down or willingly move, and the pneumonic horse stands fixed as a statue, the pleuritic one shrinks and crouches,—crouches almost to falling. If he lies down, it is on the affected side, when the disease is confined to one side only. The head of the horse with inflammation of the substance of the lungs, hangs heavily, that of the other is protruded.*

Auscultation.—Hence we derive most important assistance. In a case of pleurisy we have no crepitating crackling sound, referable to the infiltration of the blood through the gossamer membrane of the air-cells; we have not even a louder and distincter murmur, but perhaps no variation from the sound of health; if there is any difference the murmur is fainter; the pleural membrane is thickened, and its elasticity is impaired, and the sound is not so readily transmitted. There is sometimes a slight rubbing sound, and especially towards the superior region of the chest, as if there was friction between the thickened and indurated membranes.

Cough.—To this may be added the different character of the cough, sore and painful enough in both, but in pneumonia generally hard and full and frequent. In pleurisy not so frequent, faint, suppressed, cut short, and rarely attended by discharge from the nose.

These are sufficient guides in the early stage of the disease, and when it is most of all of importance to distinguish the one from the other.

Progress.—If, after a few days, the breathing becomes a little more natural—the inspiration is lengthened and regular, and the expiration, although still prolonged, is suffered to be completed—if the twitchings are less evident and less frequent—if the cough can be fully expressed—if the pulse softens, although it may not diminish in frequency, and if the animal begins to lie down, or walks about of his own accord, there is hope of recovery. But if the pulse quickens, and although it is smaller yet possesses the wiry character of inflammation—if the gaze at the flanks, no longer by starts, becomes fixed as well as anxious—and the difficulty of breathing continues—the difficulty of *accomplishing* it, although the efforts are oftener repeated—if patches of sweat break out; and the animal gets restless, paws, shifts his posture every instant, is unable longer to stand, but hesitates whether he shall lie down, determines on it again and again, but fears, and at length falls rather than lies down, a fatal termination is at hand. For some time before his death, the effusion and its extent are evident enough: he not only walks unwillingly, but on the slightest exercise his pulse is strangely accelerated; the feeling of suffocation comes over him, and he stops, all of a sudden, and looks wild, and trembles; he immediately recovers himself and proceeds. There is also, when the effusion is confirmed, œdema of some external part, and that occasionally to a very great extent. This is oftenest observed in the abdomen, the chest, and the point of the breast.

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* Gibson, the very best of our old writers on farriery, thus describes, although somewhat incorrectly, the symptoms of pleurisy. I give it, for it is almost the only account of pleurisy I can find among our authors. " A pleurisy, which is an inflammation of the pleura, and a peripneumony, which is an inflammation of the lungs, have symptoms very much alike, with this difference only, that in a pleurisy the horse shows great uneasiness, and shifts about from place to place. The fever, which at first is moderate, rises suddenly very high; in the beginning he often strives to lie down, but starts up again immediately, and frequently turns his head towards the affected side, which has caused many to mistake a pleuritic disorder for the gripes. The horse's ears and feet are always burning hot, his mouth parched and dry, his pulse hard and quick; even sometimes, when he is nigh dying, his fever is continued and increasing; and though in the beginning he makes many motions to lie down, yet afterwards he runs back as far as his collar will permit, and makes not the least offer to change his posture, but stands panting with short stops, and a disposition to cough till he has relief, or drops down."

Effusion.—The cause of death is that which is the result of inflammation of a serous membrane, effusion. The cavity of the chest is occupied by the exhaled fluid, compressing the lungs on every side, rendering expiration difficult, and at length impossible, and destroying the animal by suffocation. The very commencement of effusion may be detected by the adept at auscultation, by the cessation of the respiratory murmur at the sternum, and the increased grating—not the natural murmur, but a distinct grating or rubbing. Impress carefully on your memory the importance of observing the difference here—not the crepitating crackling noise as when congestion is going on—not the feebler murmur as congestion advances, and the absence of the murmur where it is completed (and that either over the whole lung, or in patches), and the murmur louder than natural where the lung is yet sound, and that sound part has double duty to perform—but the absence of the murmur, beginning from the bottom of the chest, the part which will be first occupied by the fluid, or sometimes a wheezing, gurgling, noise there when gas is extricated, and something of pneumo-thorax comes on, and the murmur evidently increased above, where the lungs are not compressed, and where they must work.

It is painfully interesting to watch the progress of the effusion—how the stillness creeps up, and the murmur gets louder above, and the grating sound louder too, until at length there is no longer room for the lungs to play, and suffocation ensues.

When the effusion commences, the pain of the animal seems to be materially relieved; but the beating of the flanks is increased, and the nostrils become dilated, and then the dread of suffocation immediately follows.

Post-mortem Appearances.—These are various, and depend upon the disease having been pure or combined. If it originated in pneumonia, or became combined with it, both the lungs and the pleuræ will exhibit marks of inflammation, but it will often be curious to trace the ravages of disease when the membrane only is affected:—the highest degree of vascularity—patches of coagulated lymph, the action being so intense that the fluid coagulates as soon as poured out—filaments, bands, running across from the costal to the pleura. There is rarely in the horse much adhesion; this fortunately does not often take place in an animal valuable for his speed, for even in case of recovery these adhesions would incapacitate him for that extra exertion which we often require of him, and on his capability of which his value much depends. The fluid varies in quantity as well as appearance and consistence. I have known sixteen gallons contained in both sacs, pale, or yellow, or bloody, or often differing in the two sides of the thorax; occasionally, a thick adventitious coat covering the costal or the pulmonary pleura—rarely, I have said, much adhesion, but the lungs purple-coloured, flaccid, compressed, not one-fourth of their usual size, immersed in the fluid, and rendered incapable of expanding, by its pressure.

Treatment.—Here, as in pneumonia, you should bleed promptly and copiously. Next, and of great importance, aperient medicine should be administered—that—the effect of which is so desirable, but which you do not dare hazard when the mucous membrane of the respiratory passages is the seat of disease. Here you have to do with a serous membrane, and there is less sympathy with the mucous membranes of either cavity. Two drachms of aloes may be given in the usual fever medicine, and repeated morning and night until the dung becomes pultaceous, when it will always be prudent to stop. The sedative medicine would be that which has been recommended in pneumonia and in the same doses. Next should follow a blister on the chest and sides—advisable, we maintained, in pneumonia; *there* far preferable to setons; and *here* more particularly so; for we can bring it almost into contact with the inflamed surface, and extend it over the whole of that surface. An airy, but a comfortable box, is likewise even more necessary than in pneumonia, and the practice of exposure uncovered to the cold, even more absurd and destructive. The blood repelled from the skin by the contractile, depressing, influence of the cold, would rush with fatal impetus to the neighbouring membrane, to which it was before dangerously determined. Warm and comfortable clothing cannot be dispensed with in pleurisy.

Treatment during Convalescence.—I was loath to allow you even the slightest tonic in the state of convalescence after pneumonia. I urged you to consider the matter well before you ran the risk of kindling the fire afresh. The after treatment of pleurisy, however, is a somewhat different thing. You have in most acute cases that which is usually a remedial effort of nature, effusion—an unloading of the congested vessels—but the fluid is here poured into a cavity in which it must not remain—in which a small quantity of it will produce embarrassment of breathing, and disorganisation of the membrane on which it rests, and lay the foundation for injurious deposit and adhesion. You have not only to restore the general health of the animal, which you might be content to take sometime in doing, because you would thus be doing it more safely in pneumonia; but you have to rouse the absorbents to immediate action, and to get rid of this extraneous fluid as soon as you can, and therefore I would allow you, or I would urge you, to leave off your sedative medicines sooner, and to substitute for them diuretics. The common turpentine is as good as any, made into a ball with linseed meal, and given in doses of two or three drachms twice in the day. It is a principle as true in horse practice as in that of the human surgeon, that if you can increase the discharge of the watery part of the blood, you indirectly promote absorption. It may happen, however, that the system generally may be considerably debilitated by the violence or continuance of the disease, and the absorbent vessels, among other parts, may share in the debility. Well then, you may begin cautiously to give tonics here, and begin as soon as the violence of the disease has abated. The spirit of nitrous ether is a mild stimulant and a diuretic. Small quantities of ginger and gentian may be added to it, while the turpentine should not be omitted.

Paracentesis.—By auscultation and other modes of examination, we have ascertained the existence of effusion in the chest, and that in spite of all our means it is increasing. Is there any mechanical way of getting rid of it? Yes, there is one to which I would advise you to have recourse as soon as you are assured that there is considerable fluid in the chest, and that it is not diminishing. The operation of paracentesis or tapping may be performed. It is one of the simplest in the world. You will pass the side line over the horse, or simply use the twitch and have one of his legs held up, and, counting back from the sternum to between the seventh and eighth ribs, you will pass a moderate-sized trochar into the chest immediately above the cartilages. You will not have selected the most dependent situation, but as dependent a one as you could with safety select; for you will perceive by this thorax, that there was not room between the cartilages to have punctured lower; you must have injured them in forcing your instrument through, and, what is worse, you would have run great hazard of wounding the pericardium, for the apex of the heart rests on the sternum. Through this aperture, close to the cartilages, you will evacuate the far greater part of the fluid. You will withdraw the stilette, and let the fluid run through the canula. You will not trouble yourself afterwards about the wound; it will heal readily enough; perhaps too much so, for could it be kept open a few days, it might act as a very useful drain. *It should be attempted early.* I have said that you should have recourse to the operation as soon as you are assured that there is considerable fluid in the chest, for you will at least relieve the animal for a while; you will have given some time for repose to the overlaboured lungs, and for the system, generally, to be recruited. You will evacuate the fluid before the habit of morbid exhalation is established—before the lungs are too much debilitated by laborious action against the pressure of the water, and their substance is condensed, and a state of collapse brought on, from which they will be incapable of recovering. Those only of you who have seen the collapsed and condensed state of the lung that had been long compressed by the fluid, can conceive of the extent to which this is carried. I will add a fact important and alarming, that the records of veterinary surgery do not contain a half-dozen cases of the successful performance of the operation. This should not discourage us from attempting it, but should induce us to consider whether we may not perform it under happier auspices, and before the lungs and the serous membrane which lines the cavity have been so disorganised, and the constitution itself so sadly debilitated. I cannot see any harm that could be produced by an early resort to paracentesis. He must be a bungler indeed who wounded any important part, but it really is worth our consideration whether many a horse has not been lost by delay. You will ascertain by auscultation, whether there is fluid in both cavities. If there should be, and, in considerable quantity, it will not be prudent to operate on both sides at once. If much fluid is discharged, there will be acceleration and difficulty of respiration to a very great degree. You will not be alarmed at this; it will pass over, and on the next day you may attack the other side; or open both at once, if there is but little fluid in either.

Having resorted to this operation, you will especially be eager, as soon as you dare, to commence your course of diuretics with tonics. You must rouse the absorbents to action before the cavity fills again.

Tendency to relapse.—I think I have observed, that there is in pleurisy not only a far greater tendency to relapse, but to a return of the disease at some indefinite time, than in pneumonia. The lungs do not perfectly recover from their state of collapse, nor the serous membrane from its long maceration in the effused fluid : œdema, cough, disinclination to work, incapability of rapid progression, colicky pains, as the unobservant practitioner would call them, but, in truth, pleuritic stitches ; these are the frequent sequelæ of pleurisy. This will afford another reason why we should not defer too long the important operation of paracentesis.

Metastasis.—So far as my experience has gone, there is much greater disposition to metastasis than in pneumonia : indeed it is easy to imagine, that the inflammation of a mere membrane may more readily and oftener shift, than that of the substance of so large a viscus as the lungs. The inflammation shifting its first ground, attacks almost every part indiscriminately, and appears under a strangely puzzling variety of forms. Ascites is the most frequent change. Dropsy of the abdomen is substituted for that of the chest ; or, rather, the exhalent or absorbent vessels of the abdomen, or both of them, soon share in the debility of those of the thorax.

Curious case of Pleurisy.—Mr. Percivall relates an interesting case illustrative of this. A mare, in the beginning of January, had an attack of pleurisy, for which she was treated in the usual way, and which, although acute at the beginning, in the course of a few days merged into the chronic form of a more than usual obstinacy. A prodigious œdematous tumefaction, extending from the mammæ to the front of the breast, now appeared, which yielded to aperient and diuretic medicines. On the seventeenth day from the first attack, her respiration and pulse became suddenly hurried, with cold and profuse perspiration, and she began to stagger before she had lost a quart of blood ; she recovered from this, and continued apparently well for a fortnight, when there appeared a swelling on the back of the right fore-leg, which seemed like a sprain, and which yielded to cold lotion and bandaging. Nine days after that there was a puffiness under the jaw ; on the next day but one the swelling returned under the abdomen, and she would frequently lie stretched out at full length, groaning as if gnawed by some inward pain. A week after that she was rapidly losing flesh and strength, and was again seized with acute pneumonia. Her pulse 100, and her flanks heaving ; she was bled. In the afternoon she seemed to be relieved, but she obstinately stood up, pawing occasionally, and died in the following night.

Every part of the pulmonary, pericardic, and costal pleura was thickened and indurated, and thickly clothed with adhesions of effused coagula, while about a gallon of cream-coloured fluid was found within the right side of the chest. The lungs were perfectly sound, the abdomen was distended with a serous fluid almost as limpid and colourless as water, and the mesentery and mesocolon were loaded with interstitial effusions of the same nature. It seemed to be *a disease of the serous membranes*, and points out the connexion of pleurisy with other serous affections.

PLEURISY IN CATTLE.

On this, although it is the peculiar form under which affections of the chest appear in cattle, I shall not detain you; for the causes of the disease, allowing for the different habits of oxen and horses, are similar, and the symptoms are analogous. It is preceded by rigor, and *by a peculiar trembling of the shoulders*. This is the most characteristic and the most fearful symptom of the whole. The inspirations are short, and yet consist of two distinct efforts ; the expirations are long but also interrupted. There are also cold sweats, staring of the coat. The mode of treatment must be, generally speaking, the same as in the horse.

PLEURISY IN DOGS.

Cases of genuine pleurisy in these animals are not unfrequent, and I scarcely recollect a case of inflammation of the lungs, which did not ultimately become connected with, or terminate in, pleurisy. The tenderness of the sides, the twitchings, the absence of the obstinate sitting up, and the presence of a short, suppressed, painful cough, which the dog bears with strange impatience, are symptoms which chiefly distinguish it from pneumonia.

I stated in my last lecture, that the exploration of the chest by auscultation was most satisfactory in pneumonia. It is so likewise in pleurisy, and by placing the dog alternately on his chest, his back, or his side, we can ascertain to a certainty the extent to which effusion exists in the thoracic cavity. Here too, I am sorry to say, that paracentesis has rarely succeeded, and probably, in some measure, from the late period at which it it was attempted. The mode of treatment differs little from that of pneumonia. In several cases, when I have been assured of incipient hydrothorax, balls composed of digitalis, tartrate of iron, and a small portion of calomel, have caused the speedy absorption of the fluid.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

BY

MR. BRODIE,

April 9th, 1832.

Mezereon,—A MOST useful medicine in periostitis, remarked Mr. Brodie.

℞ *Pulv. cort. rad. mezerei*, gr. iv; *divide in pil.* xij, *quarum sumat.* i, *ter in die.*

Half a grain of the powdered bark of the root as a full dose; few stomachs will bear more; and in those cases where you hear of persons having taken more, you will generally find it to have arisen from the bark not having been good.

Stimuli.—I used to think that great gin-drinkers, who were admitted into the hospital for fractures, or other severe accidents, generally got worse upon the low diet of the hospital. I therefore now always allow them a certain proportion of gin. "How much have you been in the habit of drinking a day, my good fellow?" asked Mr. Brodie. "As much as I could get," was the answer. Mr. Brodie smiled, and ordered him ℥iij of gin daily.

INJURY TO THE SPINE.

In a case of this nature, in which the urine was both alkaline and bloody, Mr. Brodie remarked "that the latter symptom was rather an unusual one." In general in these cases, after the urine becomes alkaline, the fæces are black and tarry, and the sacrum becomes sore, and sloughs.

When first this patient (who is since dead) came into the hospital, he complained principally of pain over the right *dorsum ilii*, for which he was cupped with some degree of relief; and on Mr. Brodie's seeing him, and also from Mr. Fernandez's account, Mr. B., as well as many others, suspected it to be a case of *malingering*. The man first lost the use of both extremities on the one side, and then of those on the other; *he could raise himself by his own effort slowly into bed.* Mr. Brodie, however, ordered the case to be treated as if it were really one of organic affection, by small bleedings, expecting that in a few days the symptoms would clearly show whether it was one of malingering or not. On Mr. Brodie's next visit, however, all ideas of *malingering* were entirely set at rest by the state of the patient; his urine and fæces were passed involuntarily, and the former was decidedly alkaline; over the sacrum were large erysipelatous patches of inflammation, and he was, in every respect, worse. In about a week from this period the man died, and on examining his body after death, dislocation was found between the sixth and seventh (?) cervical vertebra, and *ramollissement* of the medulla spinalis beneath.

COMPOUND FRACTURE OF THE LEG.

A man was admitted with this fracture on the 30th of March; it was situated about two inches above the ankle-joint. Effusion came on under the cellular membrane, which extended up to the knee, for the relief of which Mr. Brodie punctured the limb in several places with a needle which he had made for the purpose; it tapered gradually, and was rounded off to a point, so that, as Mr. Brodie remarked, "it could wound no other part than that which came in contact with its point." Mr. Brodie hoped that, by using this instrument, he should be able to collect the serum pure, and in sufficient quantity to examine its qualities, without its being coloured or tinged with blood. This was found, however, not to be practicable, as blood was always mixed with the serum, and it was not collected in sufficient quantity to test it. The man took the saline draught of the hospital with an excess of ammonia, and a poultice was applied to the external wound. The case went on very favourably until Friday the 13th of April, when arterial hemorrhage to the amount of several ounces took place upon Mr. Fernandez's removing the poultice to examine the wound. Pressure was made on the thigh in the course of the femoral artery, and the hæmorrhage was arrested. When Mr. Brodie saw him at 1 p.m., he inquired of Mr. Fernandez from which artery, the anterior or posterior tibial, the hemorrhage had proceeded. Mr. Fernandez replied, that he could not tell with certainty; the first gush he believed was from the anterior tibial, the second from the posterior tibial. The man's countenance appeared anxious, the pulse was quick, the tongue dry, the belly hard, and the bowels open. After leaving the ward, Mr. Brodie remarked—"This man, Gentlemen, is in a very precarious state; he has had bleeding from the external wound. I did not see the bleeding, and I cannot therefore tell you from what vessel it comes, nor can Mr. Fernandez, who did see it. Now if it comes from the anterior tibial artery, why we can cut down upon it and take it up, but the man's leg is infiltrated with serum, which would add much to the difficulty of the operation, and I doubt very much whether the performance of it in the man's present state would not endanger his life; then there is the posterior tibial; why such an operation, under the most favourable circumstances, is not an ordinary one, and I really do not think it would be advisable to perform it in the present instance. Well then, under these circumstances, if the bleeding should return, the only thing that can be done is to take up the femoral artery; but even with this resource, I have known cases, but not so desperate as this, prove fatal in three days after the tying of the artery. In this case, too, this will prove a bad remedy, as it will prevent the due supply of blood to the lower part of the limb, and it is on this supply that we rest all hope of the fracture re-uniting; but, on the other hand, I do not think we can save the man's limb, and I therefore think that if the hemorrhage returns, the best plan will be to take up the femoral artery. We shall thus stop all immediate danger of hemorrhage, and give the man time to get into a better state to bear the amputating the limb. The bleeding is now stopped, but it may recur again. I have directed the man to be kept quite quiet, and we shall see how he is to-morrow.

April 14. This morning the hemorrhage has returned twice. On our visiting him, we found him worse in every respect; pulse 114; tongue dry and brown; belly hard. Mr. Brodie retired for a few moments to consult with Mr. Hawkins, who was the only other surgeon present in the hospital, and having done so, he addressed the pupils in words similar to those above, adding that Mr. Hawkins and he thought that the only chance for the man was to take up the femoral artery. The patient's bed was therefore wheeled round to the light, and whilst Mr. Hawkins made pressure upon the artery in the groin, Mr. Brodie cut down upon it at once, passed an eye-probe armed with a ligature beneath it, and tied it. Mr. Keate and Mr. Babington arrived during the operation, and fully coincided with Mr. Brodie on the propriety of its performance.

16. He is this day much worse; countenance very anxious; pulse quick; tongue dry and brown; hardly able to put it out; difficult respiration from purulent effusion into the bronchiæ; cough; foot and leg white and livid; scarcely possible that he can live forty-eight hours; wine and ammonia ordered.

18. Died at six p.m. yesterday; his body was examined this day, but in such a manner that *no pupil could possibly profit by it.*

After taking up the femoral artery, Mr. Brodie asked Mr. Hawkins and Mr. Babington how long a fracture was prevented uniting by taking up an artery. Neither of these gentlemen could answer the question. I do not know how long it is in human beings, said Mr. Brodie, but in animals, such as rabbits and guinea-pigs, it retards it exactly one week. If you take up the artery, and break the animal's leg, and examine the fracture a week afterwards, you will find that there is not the slightest disposition to union. I have also found that it makes the claws turn in and grow rough and scaleous, and in some instances I have found the teeth fall out. If you make two angular incisions in the upper lip, you will find that in the triangular space between them, the hair will fall out and will not grow; this I suppose arises from a deficiency of nourishing blood.

ERYSIPELAS—VENTILATION.

A few days ago, when Dr. Bright was going round the hospital with Mr. Brodie, he asked whether there were many cases of erysipelas in the wards. Mr. Brodie said that there were very few. "You know that it was the frightful fatality of this disease in the old hospital on which we grounded our representations to the board of governors, of the urgent necessity there was for having a new building erected forthwith, though when we got into it, in spite of large airy wards and improved ventilation, erysipelas was as prevalent and as fatal as ever; it was really heart-breaking to see it. Well, things went on in this way for some time in spite of all our efforts to stop it; however, Mr. Hammick, jun., of the Plymouth Naval Hospital, was here one day, and he said that the erysipelas used to rage there very much, and the way they got rid of it was to dry rub all the floors and everything that required it, and in that way the disease gradually left them. We have ever since adopted this plan as far as we have been able to do it, and have found very good results from it. Erysipelas is neither so formidable nor so frequent as it used to be, and we are freer from it now than we have been for years past."* Whilst on the subject of erysipelas, we may make a few remarks on a plan of treating this disease which has been adopted by Mr. Keate. Mr. Keate draws a piece of lunar caustic over the skin where the erysipelatous redness ends; the good that results from this method of treatment is however somewhat equivocal. In some cases the lunar caustic appears to have little effect, as the inflammation sometimes spreads over this boundary-drawn line—in other cases however it does not. One patient, who was treated with the lunar caustic, died of inflammation of the pleura. Mr. Brodie's plan of treatment is, to apply cold evaporating lotions externally, and give saline medicines internally; he has a patient who has had three separate attacks of erysipelas within the last three weeks who has been treated thus, and who has each time recovered.

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* We are happy to be enabled to add our testimony to the truth of the above statement made by Mr. Brodie; there is not a ward in the hospital but what is large and dry, and what is still more, comfortable and convenient in every respect for the patients. Every ward has two ventilators, each of which communicates below with hot air from stoves on the basement story of the building, and above with the cool fresh atmospheric air.—*Rep.*

Remember, Gentlemen, that whenever a knee-joint gets weak, it is a sure sign that the patient is recovering.

CHRONIC INFLAMMATION OF THE TESTIS.

In a case which has been in the hospital for some time past, and which has been treated with mercury, Mr. Brodie remarked, that the gradual improvement that had taken place in the symptoms, warranted him in concluding that mercury was the best remedy that could be administered in this disease. "Many years ago, when I was a student here—I think it was in 1809—the surgeons of this hospital were in the habit of amputating the testicle for the cure of this disease, and the operation of castration was then as common as it is now rare; these cases used to be mistaken for ulcerated testicle. If you do not do anything for them, but leave them alone, you will find the testicle adhere to the inner surface of the scrotum, the skin will become red, inflamed, and then burst, and a fungous tumour will be protruded; this fungus is, in reality, the testicle itself; in some cases you will have an abscess form in the body of the testis, and a yellow cheesy laminated matter will be discharged, which will continue for a length of time. I used to think that the original seat of this disease was in the epidydimis, but on examining some of these diseased testes, I have found that it is in the inner lining membrane of the excretory duct, in which small granular bodies are interspersed, and if you squeeze these you will find a yellow cheesy matter exude from them. In advanced stages of the disease, the whole body of the testicle becomes filled with these granular masses, which seem to take the place of the subati testes. In some cases, in the most advanced stages, the testicle becomes shrivelled up to the size of the tops of your four fingers, and hangs by a very thin, slender, spermatic chord. I had a case of this chronic enlargement of the testicle some years ago in the hospital, and I amputated it; however, after the operation, the other testicle became diseased, and I then exhibited mercury, and since that time I have always used it successfully. There are some cases, however, that mercury will not cure, and there sarsaparilla does good. I never give potash (in answer to a question from the house-surgeon) but in cases of scrofulous affection of the testicle. This case was preceded by gonorrhœa, and what is very strange is the fact, that this disease may run to a very great length, and yet not stop the natural secretion of the organ. I know a gentleman who had this disease, and who yet had a very large family.

CASE OF HYDATID FOUND IN THE FOURTH VENTRICLE OF THE BRAIN.

To the Editor of THE LANCET.

SIR,—Should you think the following case merits publicity, you will oblige me by inserting it in your valuable journal.

I am, Sir, yours very respectfully,
GEORGE DAGLEISH, Surgeon, &c.
Wigan, April, 1832.

CASE.

On the 20th of March I was requested to visit Rd. Catterall, a labourer, ætat. 27, and found him complaining of the ordinary symptoms of fever in the early stage,—pain in the head and back, weariness of the limbs, and heat of skin, which had been preceded by rigors in the early part of the day. The pulse, however, did not exceed fifty beats in the minute, was full and labouring, resembling that of a person under the influence of pressure on the brain. His tongue was coated with a brown fur, bowels constipated, and he had several times vomited large quantities of bilious matter. Finding from the history of his case that he had been for several weeks affected with *general malaise*, and occasional severe pain in the head, I was led to suspect some organic lesion in the brain, which had occasioned effusion either between the arachnoid and pia mater, or within the ventricles. I therefore bled him to twenty ounces, administered a powerful cathartic, and afterwards two grains of calomel, with two of antimonial powder every four hours, accompanied with a simple saline mixture; cold evaporating lotion was constantly applied to the forehead, and he was ordered to be kept in bed.

On the 22nd he was much relieved, his bowels having acted freely; yet the pulse remained as before. His head was not quite well, and he had occasional throbbings in it, which was the only unpleasantness he seemed to experience. *Hab.*

hirudines xij *temporibus, et applicantur emp. lyttæ nuchæ. Cont. med. alia.*

On the 24th he was materially better, having walked out; was quite free from pain, and complained merely of debility. I wished him to continue the calomel, and gave him small doses of the sulphate of quinine, and considered him so much better, that I fancied my former prognosis might possibly be an erroneous one. With this impression, I requested that if he should find himself worse, he would immediately apprise me.

I heard nothing more of him until the 30th, when I was summoned to attend him, in consequence of a sudden accession of severe pain in the head, which was at times so urgent as to occasion him to shriek and exclaim in a manner indicative of the greatest suffering. His pulse was still at 50, full, and labouring. I tied up his arm, and took away sixteen ounces of blood, and for the first time I perceived an alteration in the pulse. Prior to syncope coming on, it rose to 90, and after he recovered from fainting he expressed himself as being much better, and his pulse shortly fell again to 50. I ordered his head to be shaved, cold lotion to be applied, and two grains of calomel to be given every four hours.

On the 2nd of April I again visited him, and found him very little better, although he thought himself easier. His countenance had now for the first time put on a deathlike aspect, and I found, that for an hour at a time he would remain apparently composed, when, suddenly, severe paroxysms of pain would come on, and continue most excruciating for ten minutes, after which he would fall into a sleep for a short time. In the evening of this day he seemed tolerably quiescent, and his friends expected him to pass a comfortable night, when suddenly he raised himself up in bed and immediately expired.

Post-mortem examination.—I obtained permission to open the head eighteen hours after death, notwithstanding there is in this part of the country such a great abhorrence of necrotomy. After removing the dura mater, the pia mater was found considerably gorged with blood; the arachnoid membrane appeared opaque, and in some parts a little thicker than natural, particularly about the base of the brain. The substance of the brain was softer than usual, and the lateral ventricles containing upwards of twelve ounces of fluid. On opening the 4th ventricle, which was much enlarged, an hydatid presented itself, equal in size to a pigeon's egg, and containing about two drachms of fluid. We were not permitted to examine any other part of the body.

Remarks.—My principal reason for troubling you with this case, is on account of hydatids being rarely met with in the human subject, though I believe one of common occurrence in the lower animals. The above patient had for the last four or five years complained of severe shooting pains in the head, although on the whole his general health was tolerably good. It is somewhat remarkable, that during the whole of his illness he was never in the least degree delirious, excepting when the pain came on in such a violent manner. Vision was not the least affected, nor was there any irregular movement of either eye-ball.

CASES OF

CONGENITAL DEFICIENCY OF BOTH EYES.

By John Walker, *one of the Assistant Surgeons of the Manchester Eye Institution, &c.*

The subjects of this notice are two children (sisters), the elder five years of age, the younger four months. It may be mentioned by the way, that the parents have an intermediate child, a boy, happily free from this lamentable defect. Exactly the same appearances are observable in both. Instead of the natural convexity of the eye-lids, supported by the globe, they have the sunken appearance commonly presented where the contents of the orbit have been evacuated or removed. The aperture between them is very small, not exceeding a quarter of an inch in the transverse direction, but they appear to possess the usual muscular movements.

The orbit appears to be occupied merely with the common cellular substance, which affords little resistance when the finger is applied over the lid. When the children cry, copious lachrymation takes place, proving the existence of a lachrymal gland, but the tears always roll down the cheeks in consequence of the non-existence of the puncta lachrymalia. So that, in a word, these cases may be described as consisting in a total absence of the globe of the eye from each orbit, attended with a like deficiency of the lachrymal passages. For an opportunity of witnessing these singular cases I am indebted to my friend M. Joynson, surgeon, of this town, the medical attendant of the family.

A case of similar description has not been seen by any of my medical friends who have examined these children, nor am I aware of any published account; this notice will therefore, I trust, be interesting, as affording an additional fact in the history of malformations. Malformations of different parts of the eye are not unfrequently seen. At the Ophthalmic Institution in this town, we have witnessed cases of congenital deficiency of the iris, both total and partial; partial deficiency of the palpebræ, &c. An interesting case lately presented itself where the cornea, and consequently the iris, were of a very unusual size, occupying nearly the whole of the globe usually covered by the conjunctiva, so that scarcely any of the white of the eye could be seen. There was no projection nor any appearance of disease, except slight opacity of the cornea. This occurred in both eyes. The infant was brought to the institution a few days after birth.

Manchester, May, 1832.

SALINE TREATMENT
IN
TYPHUS FEVER AND CHOLERA ASPHYXIA.

CLAIMS OF DR. KNIGHT TO ITS ORIGINAL PROPOSITION.

To the Editor of THE LANCET.

SIR,—As the fearless asserter of right wherever you may find it, as one devoted to the best interests of society as far as they are involved in the development of medical talent, I confidently anticipate that you will afford me a brief space in your Journal, whilst I again call the attention of the profession to the use of certain alkaline salts, now so happily developed in the Coldbath Fields prison, though in doing so I should descend to notice my pretensions to priority and originality in this practice. From the statements, originating from medical men holding high official situations, published lately in THE LANCET and other medical periodicals, exemplifying the curative powers of certain saline preparations in the treatment of cholera asphyxia and typhus fever, and from the total silence observed by them relative to the cases that I have from time to time published, illustrative of this practice, I am led to apprehend that I may have laboured under a mistake, when I considered my mode of treating typhus fever and the cholera of these climates, to be not only original, but to have been calculated to form no less than an epoch in the annals of medicine.

Have I claimed that which belongs to another? I am not aware that I have, and I feel convinced that I have not; but if I have, permit me without delay to disclaim any such intention,—a disclaimer which the papers you have, not only lately but formerly, published for me relative to this practice, will fully bear out, incontestably showing that I have acted with good faith. I am led into this train of thought chiefly by the letter of Mr. Wakefield, relative to the "saline treatment recommended by Dr. Stevens," and adopted in the treatment of the patients in the Coldbath Fields prison. I have hitherto imagined, that my letter in *The Times* of 21st June last, was the first publication that, *founded on practice*, advised, and I trust on scientific principles too, and not as a vague speculation, the *saline treatment* in the cholera asphyxia; and that the case published in THE LANCET, January 26th, 1828, p. 624, and not in 1827, as I erroneously stated in a former letter, was the first case on record wherein the carbonate of soda has been stated to be given as a "*principle of practice*" in a fever, frequently of the most dangerous typhoid species; and that the saline treatment with the carbonates and subcarbonates of soda and potassa, occasionally joined with preparations of the nitrass potassa, for the cure of typhus fever, adopted in 1825, the particulars of part of which you did me the favour to publish in THE LANCET, June 1830, and again in February and March last, was the earliest time that these medicines had been so applied. With this impression, and being ignorant that either Dr. Stevens or any other man has any prior claim, I crave permission to reiterate my pretensions, though they are only denied by implication, a mode frequently as insidious as it is impertinent, and to insist upon my right, whatever that may be. *I have given dates, places, and persons' names*; I have never used any kind of secrecy; I have never communicated my views to a few privileged individuals only; openly I have practised, and as openly I have stated the results.

This, Sir, is the basis on which I rest my claim to the merit of instituting a new, and, as I think, a greatly improved practice in the treatment of typhus fever, and fevers with the typhoid type, and cholera asphyxia. It is very easy to show if I am in error, and I challenge any one to do so; but at the same time I disclaim every intention, every wish, to appropriate to myself that which, in my conscience, I do not believe to belong to me.

I am, Sir, your most obedient servant,

PAUL SLADE KNIGHT, M.D.

15, Connaught Terrace, May 5th, 1832.

TREATMENT OF MALIGNANT CHOLERA.

To the Editor of THE LANCET.

SIR,—After the numerous and some ably written articles on the treatment of cholera in your valuable journal, it may appear tedious and presumptuous in me to offer an opinion on the subject, and did I not feel that I was doing an act of justice to the medical profession and to the public, I should have remained silent. Knowing your liberality and desire to promote any information that might be useful, I am induced to draw up a plan of treatment which has been very successful in those cases which have fallen under my care, having reported to the Board of Health upwards of 160 cases, in consequence of my filling the situation of surgeon, &c. to the poor of St. John's, Southwark, the major part of which cases have been of the very worst character.

In the early commencement of the disease, and during the three first weeks, I adopted the different plans laid down by medical authors, which plans proved very unsuccessful; almost every case died. I was then induced to try a plan of treatment of my own, which plan proved successful. When called to a case with all the bad symptoms of cholera, viz., vomiting and purging of rice-coloured fluids, cramps in the arms and legs, at times in the region of the stomach and diaphragm, coldness of the whole body, great prostration of strength, sunken eyes, &c., the pulse scarcely to be felt at the wrist, the fingers and toes drawn and contracted, and attended with great thirst, I commenced with giving, immediately, two grains of the extract of belladonna, washed down in one drachm of the tincture of ginger, with three drachms of water, and repeated in ten minutes if the cramps and sickness continued. Mustard poultices to the whole of the chest, abdomen, and spine, were kept on till the patient complained of great pain and uneasiness, and the pulse increased in volume, which generally took place in about fifteen minutes. I then commenced with giving three grains of calomel with half a grain of powdered opium, every hour for four hours, keeping at the same time the patient from taking any kind of fluid, save and except a teaspoonful of toast-and-water occasionally to wet his or her lips. This treatment, I am happy to say, has proved itself truly satisfactory. Taking the average of cases, I have been able to save eight out of twelve, where all the bad symptoms of cholera have prevailed. After the lapse of six or eight hours, if the patient has not had an evacuation, nor passed water, which is one of the characteristic symptoms of cholera, I gave half a drachm of powdered rhubarb with a little conf. aromat. every three or four hours, with a mild emollient glyster; this generally produced the desired effect, and my patients did well, leaving great debility, &c. At times, after the first stages of the disease, consecutive fever followed; this I have found yield best to carbon. sodæ, in doses of five grains every three or four hours, with mild diluents, veal, or chicken broth, with rice boiled in it; then avoiding all stimulants until all inflammation or fever have subsided. I am, &c.

WM. MISKIN, Surgeon, &c.

8, Broad-street, Horselydown,
May 4th, 1832.

CASE OF

MALIGNANT CHOLERA.

RELATED BY THE PATIENT.

To the Editor of THE LANCET.

SIR,—Having experienced in my own person a violent attack of the prevailing epidemic, I am induced to give publicity to the case in consequence of the very unfavourable auspices under which it was treated. Previously to detailing the particulars of the case, I ought to say that to the unremitting kindness and attention of the medical attendants, my restoration to health is entirely due.

I am 22 years of age, and a pupil of St. Bartholomew's Hospital. About three months since, having wounded my hand in dissecting, so much irritative fever ensued as greatly impaired my general health. In this weakened state of constitution, when the cholera made its appearance in this city I became much engaged with Mr. Kiernan and M. Halmagrand in prosecuting their investigations into its nature, assisting in many post-mortem inspections, as well as several times witnessing the various precursory symptoms of the disorder. On Thursday, the 12th of April, I attended as Mr. Kiernan's assistant to inspect a case of malignant cholera which had proved fatal, in New Street, Cloth Fair, and on the morning of the following Saturday I was present at an examination in the same house, of a woman who had struggled with the disease for several days, and had died in the last or typhoid stage of the complaint. During the afternoon I felt a degree of lassitude, and had two loose and griping evacuations with a painful sensation, as if my stomach and intestines were spasmodically affected, although I had carefully abstained from all indigestible food. About 12 p.m. I was seized with violent spasms of the stomach and bowels, and also of the legs and arms, accompanied by great sickness, the fluid ejected resembling oat-meal gruel, with an admixture of some dark substance having the appearance of tea-leaves. Mr. Burridge, the gentleman who first saw me, immediately sent for Dr. Waller and Mr. Kiernan. My face and tongue were then quite cold, and likewise my upper and lower extremities, which were violently convulsed, resembling the spasmodic twitchings of an animal subjected to galvanic influence. These occurred several times in the space of a minute, and were accompanied occasionally by a much more painful contraction of the muscles, in which the toes seemed forcibly pulled in contrary directions; my eyes were sunken and surrounded by dark areolæ, and the pulse, which was exceedingly small, was above 100. Dr. Waller, who arrived first, instantly applied bottles of hot water to my feet, and warm flannels to the abdomen, at the same time enveloping me in thick blankets, and raising the temperature of the room as much as possible by a large fire; two grains of opium in two ounces of white brandy, with a little hot water, were then administered, and I was requested to resist the inclination to vomit. During the spasms Dr. Waller employed friction with his hand. This treatment was persevered in for some time, with evident signs of re-action. At the expiration of a few hours, a profuse perspiration broke out, and the spasms gradually lessened in severity. The diaphoresis was continued through the night, and during the whole of the following Sunday, I dozed at intervals, with a low muttering delirium. My thirst was excessive. Dr. Waller, Mr. Kiernan, and Mr. Burridge, remained with me all night, and administered occasionally hot brandy and water. At noon, on Sunday, the spasms had almost entirely subsided, and the sickness and inclination to go to stool were very much abated. Towards evening however there was an increased excitement of the arterial system, and a slight return of the cramps, with great restlessness, which were in a great measure subdued by the administration of two grains of opium, with some hot brandy and water. The night was passed in dozing, interrupted by incoherent talking, and I was conscious of a painful sense of weight on the sensorium. On Monday the 16th, I was proclaimed out of danger, when all the unpleasant symptoms had disappeared. The secretion of urine was entirely suspended until the Sunday night. My bowels were relieved three times. On Monday the first motion was scybalous; the second seemed to be that fluid peculiar to the disease, and the third consisted entirely of very viscid and acrid bile. From this time I have gradually improved, although the disease has left great debility and weakness, with irritability of the stomach and intestines, obliging me repeatedly to have recourse to opium.

In this case fear could not in any degree have operated as a predisposing cause, as I had not felt any. It is necessary perhaps to observe, that at the time I contracted the disease, my health was in a much more improved state than when first I visited persons labouring under this malady, and attended the *post-mortem* examinations of their bodies. But this was the only body which I had opened where death had occurred in the *typhoid* stage of the complaint.

I am, Mr. Editor,
Your humble obedient Servant,
PROTHEROE SMITH.

8, Montague-place, St. Bartholomew's, April 24, 1832.

NEW ALKALOID PRINCIPLE.

To the Editor of THE LANCET.

SIR,—I take the liberty of sending you the process for preparing a new alkaloid principle which I have lately discovered, and beg the favour of its insertion in the pages of your valuable publication.

Process.—This substance is found in the root and leaves of the *arum maculatum*, and appears to possess the concentrated, poisonous, and acrid part of the plant. I have called it aronia (from *αρων*, the name given by Dioscorides to the Arum). The best method for obtaining it is, to bruise the whole herb, with the roots, into a pulp, and wash well with boiling water, till everything soluble is taken up. By standing, the infusion deposits a fecula, consisting of starch and chloro-phylle. The liquid is then to be decanted, and nitrate of lead added, until it ceases to throw down anything. Filter, and reject the precipitate; pour a solution of pure potassa into the clear fluid, and a fine orange-coloured precipitate will fall, which must be separated, washed with pure water, dried, and then boiled with dilute sulphuric acid, which will form a soluble sulphate of aronia, and an insoluble sulphate of lead, if an excess of the nitrate was added. Filter, and evaporate. A gummy mass of sulphate of aronia will thus be obtained, from which the base may be separated by solution in water, and precipitation by an alkali.

Properties.—Aronia exists as a white powder like veratria; taste, bitter; taste of the sulphate, very acrid and bitter; it restores the blue colour of reddened litmus paper, is insoluble in water, soluble in alcohol, and insoluble in ether. A grain of it held over a spirit-lamp on a slip of glass turns red when touched with a drop of nitric acid, but is not affected by protomuriate of tin. Two pounds of the arum yield about thirty grains of aronia.

I am, Sir, yours obediently,
G. Bird.

Seymour Street, Euston Square,
May 1.

DR. AYRE.

To the Editor of The Lancet.

Sir,—In consequence of the extraordinary appearances of a case, given by Dr. Ayre, in The Lancet of last week, both incorrectly, and without authority, in which my name appeared, I have the sanction of Dr. Alderson to state, that the case was one of a strictly private nature, and that Dr. Ayre had no participation in the treatment, nor did he receive or request any permission from Dr. Alderson to publish it. The *real* facts of the case are essentially different from those detailed by Ayre, and are corrected as under:—

Dr. Ayre's Report.	*Corrected Statement.*	
"He was first seen by Mr. Clay of the Cholera Hospital, and Dr. Alderson, who bled him to ℥viij."	The case was seen at one o'clock p.m., on April 17th, by Dr. Alderson, Messrs. West and Fullam. He was bled by Mr. West, at Dr. Alderson's request, to ℥x. I first saw the patient at four p.m., and had immediate recourse to the hot-air bath.	
"Twelve grains of calomel and two grains of opium, with two grains of calomel, and ¼ of a grain of opium every hour."	Ordered by Dr. Alderson 10 grs. of calomel and 1 gr. of opium, with 2 grs. of calomel and ¼ gr. of opium every hour. In addition to the hot-air bath, I administered, in divided doses, 2 drachms of tinct. opii to allay the severe vomiting and cramp.	
"He was again bled in the evening, but with no apparent benefit."	At five p.m., finding that no decidedly beneficial impression had been made, on the contrary, that the facies cholerica became more strongly marked, the arterial pulsation at the wrists more indistinct and intermitting, spasmodic action more intense and aggravated, &c., and adopting the physiological views and practical deductions of Bell, I bled the patient to ℥xvj, with the happiest and the most pleasing result. At each jet of blood, the pulse appeared to grow, as it were, under the finger, the over-laboured heart began to resume its natural tone and energy, the spasms relaxed, and the cerulean hue faded from the countenance.	
"The calomel and opium were continued uninterruptedly till the secretion of the liver was restored."	April 18th, two p.m. To repeat the calomel without opium every three hours. On the 19th, gums swollen, omit the calomel altogether. 22d. Convalescent. Mouth sore.	
"Had not any soreness of the mouth from calomel."	The calomel was given with a view to its constitutional effect, and omitted when that was obtained.	
	Hull, April 23, 1832.	
"At the moment I am writing, this town is, I believe, without a case of the disease."	Mrs. Longstaff, seized at 4 a.m., died at 1 a.m. on the 24th.	Reported to the hospital at 4 a.m.; seen by myself at 5, and by Dr. Alderson at 8 a.m.
"Hull, April 23, 1832."	— Pashley, seized at 6 a.m., died on the 25th.	Seen by Mr. Casson and Dr. Alderson at 6 a.m.
	Also a child (Ralph) died at 5 a.m.	Reported by Mr. R. Craven.

The insertion of the above in the next Number of your valuable publication, will much oblige

Your obedient servant,
H. Clay.

Hull, May 1, 1832.

The Dublin Journal of Medical and Chemical Science. No. II. May, 1832.

The second number of this journal has given us considerable satisfaction. With an approach to the requisite augmentation of size, the quality of its original contents is highly respectable, and the arrangement and selection of the miscellaneous articles display no ordinary judgment. Of the spirit, talent, and independence, of its reviews, we can also speak favourably.

CASES OF ANEURISM.

The first article is a brief but instructive paper by Mr. Cusack, on the treatment of three cases of circumscribed, and one of diffused false, aneurism at the bend of the elbow, a topic in practical surgery, concerning which there still prevails some difference of opinion. Some surgeons maintain that both forms of the disease require operation, others contend that compression suffices for their cure. Again, some apply a ligature upon the bend, above the aneurism, and depend upon the absorbents for the removal of the tumour, whilst others open the aneurism and the artery, above and below the wound. Neither, it appears, have the supporters of compression determined unanimously the place where it should be applied, nor upon the degree of the application, some directing violent pressure to be made on the vessel above the aneurism, in order to obliterate its canal, while others use very slight pressure on the aneurismal tumour itself.

In the cases reported, much light is thrown on the comparative merit of two of these modes of practice. The first was in consequence of an accident which the author by an almost pardonable bull, terms, " a puncture in *venæsection*," although no vein had been opened during the operation. On inspection, a firm pulsatory tumour, about the size and shape of a pigeon's egg, was detected in the bend of the arm, along the course of the brachial artery, with which it appeared intimately connected. It became smaller when compressed, but resumed its original size when the pressure was removed. The integuments were perfectly healthy. The case being, as the reporter states, evidently one of circumscribed false aneurism, not complicated with wound of the vein, and unattended with inflammation, it was considered favourable for the trial of compression. The antiphlogistic system of Valsalva was put into moderate operation: a thin compress of wetted lint was laid upon the aneurism, and a roller bandage applied from the fingers to the bend of the arm, care being taken that the compression should not extend above the aneurism. The turns of the bandage above the aneurism were very loosely applied. The bandage was occasionally opened and reapplied during the treatment, which lasted for thirty days, at which time " there was no trace of the aneurism, the brachial artery could be felt pulsating strongly beneath the cicatrix in the integuments, and the radial pulse was as full as at the opposite side. Eighteen months after the patient's dismissal from the hospital he was again examined, and the completeness of the cure ascertained.

The second case was of precisely the same description, and its treatment conducted in the same manner. On the fiftieth day there was no trace of the aneurism, and the artery which had been wounded could be felt pulsating strongly beneath the cicatrix at the bend of the arm.

The third case is rather more interesting than its predecessors. An oval pulsating tumour, the size of an orange, was situated in the bend of the arm; its long axis corresponded with the brachial artery, to which vessel it adhered intimately. The integuments were free from inflammation. For eight days the same treatment was adopted as in the previous examples, but on the 9th, matters assumed a different aspect, which the author thus describes:—

> " On the following morning, he complained of great pain in the arm, principally referred to the tumour, and stated that he had passed a restless night. On removing the bandage, it became evident that the aneurism had ceased to be circumscribed; it extended up along the inner edge of the biceps muscle, towards the axilla, for several inches. The whole swelling, but particularly the upper part, was soft and fluctuating, and the pulsation throughout was not very distinct. As the case had clearly changed its nature, it became necessary to consider what was the best mode of treating it, it now being a case of diffused false aneurism. In consultation it was determined to treat it as a wounded artery; accordingly, an incision was made

along the course of the original aneurismal swelling, including the cicatrix, through both skin and fascia. The coagula having been turned out, a gush of florid red blood directed the operator to the artery at the bottom of the cavity; the aperture in the vessel was perfectly circular, and engaged at least half of its diameter. The hemorrhage continuing after the ligature was applied above the wound in the vessel, another was applied below; the hemorrhage instantly ceased; light dressings were applied, and the arm laid upon a pillow. The ligatures were cast off on the 12th day, and on the 30th day he left the hospital quite well."

From the preceding cases, the author derives the following inferences; first, that the form of aneurism to which pressure is most applicable, is that of circumscribed, false aneurism, free from inflammation of the sac or adjacent parts; secondly, that the pressure should be applied to the tumour alone, and not to the artery above, and that the degree of pressure should be slight, lest the aneurism should subsequently become diffused; and thirdly, that the old opinion of the obliteration of the artery at the part wounded is "*in the majority of cases*" erroneous, and without foundation. We place the preceding words in italics, for the purpose of mingling our commendation of the author's skill in the reporting of these valuable cases, with a friendly hint, that he should not be so apt to draw general inferences from facts so limited in number. How can we rationally conclude, that the phenomena of *three* cases of any disease prove the nature of the majority? This is one of the many examples we perpetually meet of premature generalization, a fault which we trust the author will eschew in future essays.

OPIUM IN LARGE DOSES.

The second paper is by Dr. William Stokes, of the Meath Hospital; and, like all his contributions to the therapeutic science, is of decided and truly practical value. It is entitled "Clinical Observations on the exhibition of Opium, in large doses, in certain cases of Disease." We extract the conclusions he derives from his observations, premising that a cautious perusal of the details has satisfied us of the correctness of every position he advances.

"The following conclusions appear justifiable from the facts now recorded:—

"1st. That in certain cases of inflammation of serous and mucous membranes, where depletion by blood-letting or other antiphlogistic measures are inadmissible, and the system in a state of collapse, the exhibition of opium has a powerful effect in controlling the disease.

"2nd. That under these circumstances, the remedy may be given in very large doses, with great benefit and safety.

"3rd. That its effect then is to raise the powers of life, and remove the local disease.

"4th. That the poisonous effects of opium are rarely observed in these cases; the collapse and debility of the patient appearing to cause a tolerance of the remedy.

"5th. The cases in which the utility of this practice has been ascertained, are as follow:—

"Simple peritonitis, in a stage where bleeding cannot be performed. Low puerperal peritonis. Peritonis from perforation of the intestine; from the opening of an abscess into the sac; or lastly, after the operation of paracentesis in debilitated subjects. Violent diarrhœa, supervening in an exhausted subject. Phagedenic ulceration of the throat, in a similar individual. And cases of chronic gastritis, and gastro-duodenitis in patients exhausted by the long continuance of the disease.

"6th. The cases in which this mode of treatment would be probably useful are,—peritonitis from rupture of the bladder, or uterus, traumatic rupture of the intestine, or after the operation for strangulated hernia."

The doses of opium were very liberal, frequently a grain an hour for eight, ten, or twelve hours; in one case a man took upwards of 100 grs., without experiencing the least narcotic effect.

SPONTANEOUS AMPUTATION OF THE LIMBS OF THE FŒTUS.

In the third paper we find some very interesting observations on this obscure subject by Dr. Montgomery, Professor of Midwifery in the Irish College of Physicians. The fact of the occasional separation of the limbs of the fœtus is mentioned by several writers, the great majority of whom attribute the occurrence to the effect of inflammation and gangrene. It seems, however, that in all the distinctly described cases, no absolute evidence of any such disease was observed; the stumps were, in fact, healed, and no change of colour was perceptible. A curious example of an approach to this occurrence in the author's practice has afforded him the opportunity of clearly explaining at least one of its causes, and one which is totally different from inflammation, or any of its effects.

"Observing the unusual figure of the head, I laid by the fœtus for examination, and having placed it in clean water for the purpose, I was greatly struck with the appearances it presented.

"The circumstance which most forcibly attracted my attention was, the appearance of distinct ligaments on the limbs; and examining closely, I found them in the following condition:—

"There were distinct threads, of, I presume, organised lymph passing from both hands to the legs; at one end these threads, which very much resembled the kind of thread called ardis, had formed a complete ligature round the middle of each hand, causing a distinct depression where it passed, the part of the hand below it being almost completely undeveloped; from the hands these threads descended, at both sides towards the legs, which were crossed, and surrounding them in this position, just above the ankles, so tightly, that fully two-thirds of their whole thickness were thereby divided, without, however, any breach of the skin having taken place, nor was there the slightest appearance of disorganization or discoloration of any of the parts, but as were the hands, the feet also were imperfectly formed, totally undeveloped, and of course misshapen. The fœtus is preserved among the preparations in my museum. The mother was about twenty-five years of age, and at the time labouring under fever, but had been previously in perfectly good health, and had not met with any accident, bodily injury, or mental agitation."

From the condition of the limbs thus produced, and the impossibility of the parts below the ligatures continuing their growth under such circumstances, the author states, that it seems exceedingly probable that had the child continued to live and grow, the parts of the legs below the ligatures would have been separated, and thus have undergone spontaneous amputation. Concerning the formation of these curious threads, and the mode in which they became thus applied to the limbs, Dr. Montgomery prudently enters on no speculation.

EFFECTS OF CERTAIN REMEDIES.

The 4th article is entitled "Observations on the Treatment of various Diseases, by Dr. Graves." The author first details a remarkable case of spontaneous ptyalism of a whitish viscid mucus, secreted by the mucous membrane of the fauces and back of the pharynx, but in the production of which the salivary glands were not at all concerned. The patient suffered severely, and was reduced to a state closely resembling the effects of prolonged diabetes. After resisting various remedies, Dr. Graves ordered one grain of opium every fourth hour, and that night the ptyalism completely ceased. Having recurred again in a few days, the opium was again resumed, and with the same excellent effect. Though a rare affection, this is perhaps one of the most distressing torments to a patient that can readily be imagined. We remember seeing a midle-aged female some years since, labouring under it to a severe degree, and cured by chewing the root of the anthemis pyrethrum.

Dr. Graves appends to the description of this case a few very valuable remarks on some of the secondary effects of mercury. A lady, a patient of his, who many years since took a large quantity of mercury, has ever since been subject to occasional returns of salivation, in every respect resembling those brought on by that mineral. During these fits, which are always brought on by exposure to cold, the mouth is sore, the gums red and swollen, the salivation copious, and the breath strongly impregnated with the mercurial fœtor. This case is deeply important in many points of view, and especially with reference to the medico-legal investigation of cases of suspected mercurial poisoning.

Cold affusion in convulsions, next engages the author's attention; he adduces a striking case in proof of its efficacy, and offers many judicious remarks on the precautions to be observed in its employment.

"A most instructive monograph might be written on the application of cold to the head in various diseases; at present, much mischief frequently arises from practitioners being unacquainted with the different degrees of cold suitable to different states of the cerebral organ, and the different methods of conducting its application, so as to produce relief. In one case of fever, I saw violent mania immediately follow the injudicious application of ice to the head; and in another, much difficulty was experienced in saving the life of a young person, in whom a collapse of the system, without relief of the local affection, had been induced by the too copious and continued application of cold water to the head. Where very violent pain in the head occurs in fever, the *cold dashing* with water from a height, as recommended by Dr. Smith in his excellent treatise, is often a most valuable remedy, but in convulsive diseases like that now described, this application is too violent; in such cases, the stream of water should be small, not poured from a height, and should be discontinued the moment the fit ceases, to be again renewed on the approach of another paroxysm. I am informed by an eminent practitioner of this city, that he twice witnessed fatal convulsions follow the injudicious use of cold affusion in mania. The efficacy of cold affusion in delirium tremens, in asphyxia, in cases of over doses of prussic acid, &c., prove that it is too powerful an agent to be indiscriminately applied."

A case of hydrophobia, and another of tetanus, follow. In the first, strychnine was freely but ineffectually given. In the second, tobacco was diligently used, but with equal inutility. A curious instance of neuralgia of the mammæ is next related, in which, after stupes, narcotic liniments and plasters, salt-water baths and leeches, had been tried in vain, great relief was afforded by half-drachm doses of the carbonate of iron. The author adds to this case some excellent remarks on the mode of administering tonic medicines. We extract these observations, as they are replete with practical value.

" In fact we may consider it as a general rule, that tonics are rarely indicated, where moderate doses do not effect the desired purpose. This applies more particularly to the stronger tonics, such as the salts of iron, of arsenic, and quinine. I can scarcely conceive a case possible, in which a judicious physician will find it necessary—for instance, to give more than ten grains of sulphate of quinine in a day, and yet much larger doses are not unusual here and elsewhere. Whenever the symptoms supposed to call for such a treatment resist moderate doses of sulphate of quinine, we ought to pause, and reflect whether another plan of treatment ought to be adopted. There are two states of the system attended frequently with well-marked rigors, febrile paroxysms, and intermissions closely resembling ague; I mean internal suppuration, and local inflammation without suppuration. Practical physicians are fully aware of this circumstance, *but there is another condition of the system in which symptoms simulating ague arise, totally unconnected with inflammation*, and of which I have seen two remarkable examples. They both occurred in females. One, a lady of a nervous temperament, in about a fortnight after her confinement, was affected with well-marked symptoms of quotidian ague, which grew worse and more violent during the exhibition of very large doses of sulphate of quinine, but she rapidly got rid of her complaint, when, at my suggestion, camphor, aromatic spirit of ammonia, &c., were substituted in its place. In another lady, symptoms of tertian, and afterwards of double tertian, had continued for many weeks, and had reduced the patient extremely, sulphate of quinine, arsenic, and opium, had successively received a fair trial, but in vain. This disease, however, finally yielded to the exhibition of diffusible stimulants, used in combination with *antacids*.

" I cannot point out how such cases are to be distinguished from ague, except it be by the failure of the sulphate of quinine. From local inflammations and suppuration they may in general be distinguished with facility. I may here observe that, in a gentleman treated by Dr. Marsh and myself, violent symptoms of ague depended in the presence of a number of *very small* abscesses in the liver. Here sulphate of quinine given in *lavements* caused a cessation of the rigors, but *did not diminish the other symptoms of fever*; on the contrary, had it been persevered in, the intermittent would have been evidently converted into a continued fever.

" The influence of sulphate of quinine in preventing rigors, even where it cannot remove the cause of constitutional irritation, is well illustrated by its effects where the symptoms depend on stricture of the urethra; and ought to be recollected by every practitioner, lest he be misled occasionally by this partial improvement into an injudicious continuance of the medicine. To conclude my remarks on this powerful remedy, physicians generally prescribe it in too large quantities, also in chronic diseases. Where it is intended to act as a *tonic*, I am persuaded that the dose should never exceed half a grain three times a day, and generally even smaller quantities are sufficient; when a combination of tonic and purgative medicines is required, all our intentions may be answered by a combination of sulphate of quinine in proper quantity, with the compound extract of colocynth, or the pilul. aloes cum myrrhâ."

Our analysis has already extended to such a length, that we are compelled to postpone the examination of some other very interesting papers until our next Number.

THE LANCET.

London, Saturday, May 12th, 1832.

A Correspondent, in allusion to the proceedings at the last meeting of the Westminster Medical Society, and our remarks on the observations of one of the speakers, adverts with surprise to the fact that Dr. James Johnson united himself with those who affected to believe that we had changed our opinions on the question of contagion in cholera. Our correspondent thinks that long habits of examining and analyzing literary and medical questions "should have preserved *him*, at any rate, from the misrepresentations of which he was guilty on that occasion."

As we usually contrive to convey our opinions on the various subjects we discuss in tolerably clear terms, and as obscurity of expression has never been one of the faults of *our* mode of writing, we are forced to conclude that Dr. Johnson must have perused our recent observations too hastily, when he charged us with having abandoned the doctrines of contagion in the very same article in which we dwelt on the strongest proof ever offered of the existence of that agent in the communication of disease. We allude to Mr. Prebble's cases, which we shall over and over again cite as deserving to be considered as the most powerful evidence that was ever offered to the profession on this important subject. No stronger proof was ever afforded of the contagion of measles, scarlatina, typhus, or plague, than these cases present of the contagion of cholera.

In order, however, to prevent any future misconception or misstatement of our views in this matter, we once more reiterate the creed which we have ever upheld from the first moment of its discussion.

We believe and affirm, from irrefragable proof, that the cholera *can* be communicated from man to man, but, at the same time, consider, that the facility or difficulty of receiving the disease, is regulated by external causes, of the nature of which we have no knowledge.

From the consideration of the geographical history of the disease from the time it entered the duchy of Posen to its appearance in England, we believe that it was extended principally by human intercourse.

We admit the *possibility* of the operation of another cause besides contagion. But of the nature of that cause we do not pretend to possess any knowledge, and regard its admission as resting principally on negative evidence of three kinds:—First, the occurrence of cases in places the inhabitants of which have not had any *known* communication with sources of contagion; Secondly, the immunity of numerous persons who have been exposed to actually infected individuals; and, Thirdly, the recent geographical history of the malady.

Now, of the uncertain character of the first kind of these proofs an instructive example has recently been published by Mr. Tweedie. We extract the passage *verbatim*:—

"*Case* 1.—Mrs. Fitzgerald, living in 3, Birdcage-alley, High-street, Borough, was seized with cholera on the afternoon of Sunday, February 19th, and died in the course of the ensuing night. She was taken ill whilst she was out on her way to attend the funeral of one Sullivan, who had died at the back of Tooley-street a few days before, and whom she had visited during his illness.

"*Case* 2.—Mrs. Doyle, who nursed Mrs. Fiztgerald, and was a robust fearless woman, sickened over the body of the latter, became worse, and died on the Thursday following, February 24th, in the same house. These two cases were attended by Dr. Whiting.

"The day after Mrs. Doyle's death a family living in that house, taking alarm at the event of these two deaths, removed themselves and their goods to the house of a man named Daniel Corcoran, 9, Royal

Tent-court, Kent-street, in Newington parish.

"*Case* 3.—Corcoran was taken ill on Sunday afternoon following with the most aggravated symptoms of cholera, and died that night, towards the morning. In addition to this, the widow of a man who had died elsewhere of cholera on the preceding Tuesday, had since then taken refuge at Corcoran's. This was the first case in Royal Tent-court.

"*Case* 4.—On Saturday, 3d March, Owen Maly was admitted into Guy's Hospital for an illness which, after admission, was pronounced to be cholera. He was thereupon removed to the cholera ward, where he died on the night of the 4th. This man, *after close inquiry*, was found to have resided in Royal Tent-court, Kent-street, and to have been acquainted with and visited Corcoran (Case 3). He had a few days' premonitory diarrhœa.

"In illustration of the impediment that so frequently exists to the complete elucidation of the circumstances of a case, I would refer to the official report of Dr. O'Shaughnessy, the highly-talented and industrious inspector of the Camberwell and Newington districts. In No. 4 of the *Cholera Gazette*, p. 157, there is the brief notice of one Daniel Coghlan, living in 9, Royal Tent-court, Kent-street, who was seized with cholera on the 26th February, and died that night; the case having been seen by Mr. Callaway and several other medical gentlemen. The report concludes in these words:—"No communication could be traced with persons or things calculated to propagate this disease." At the time that we were in attendance upon the case, as well as on the following morning, we remained satisfied that such indeed was the fact, and that here was a case in which "the evidence of contagion was entirely wanting." But on the following day, being at Guy's Hospital, I recognised amongst the workmen there an individual who had been present in Coghlan or Corcoran's room during his illness, a relative of his, who knew him well. From this man, with some difficulty, I learnt the foregoing truths, which were afterwards literally verified by the inquiries of my friend Mr. Charles Gaselee, made in a totally different quarter."

After this, what right have we to say that no communication with infected persons or things has existed in the cases in which *we* could not trace it? What, too, becomes of this species of argument in favour of epidemic influence, if it be sifted as rigorously as common induction demands?

As to the proof afforded by the immunity enjoyed by the numerous exposed persons, we confess that there is no kind of reasoning more open to fallacy. We have over and over again recorded the occurrence of similar events in typhus, small-pox, measles, and plague.

In short, of all the reasons supposed to indicate the prevalence of epidemic influence, the only one which weighs with us, is, the history of the geographical statistics of the disease, for the past two months. This as strongly points to some other agency besides that of contagion, as the previous continuous route and moderate mortality of the disease pointed to contagion alone.

In conclusion, we have only to remark, that with respect to the hint given in the last of our numbers but one, as to the prevalence of the unknown cause of epidemic diseases, being, to a certain extent, a local protection against contagion, it was merely thrown out as a topic for discussion. It was not advanced as an adopted opinion; it may be accepted or rejected. There are powerful facts to uphold it in the history of cholera, and all the confessedly contagious maladies. There are other facts equally opposing the admission. At any rate, the subject deserves consideration.

UNIVERSITY OF LONDON.

To the Editor of The Lancet.

Sir,—Knowing the interest you have always taken in the prosperity of the University of London, I feel assured that you will insert in the next number of The Lancet the enclosed copy of the questions proposed by our highly and justly-esteemed Professor, Dr. Elliotson, to those gentlemen who have contended for honours in his class. Their great merit will I am sure alone be sufficient apology for my obtruding them on your notice.

A brief account of the manner of conducting the examination may not be unacceptable to your readers, the same plan being adopted in all the classes. It took place on the 5th of May. At 12 o'clock a number of copies of the questions were brought into the theatre in a sealed packet, and the students were requested to seat themselves at such distances from each other as to preclude any communication, when a copy of the questions was delivered to each, and six hours allowed for the completion of the answers; but in this instance, another hour was given at the unanimous request of the students. At the expiration of that time the answers were all placed in a box appropriated for that purpose, each being signed with a motto adopted by the pupil, so that a Professor has no means of knowing the name of the persons to whom he has awarded honours until the day of their distribution, when they are publicly declared. This will take place on the 16th of May. The students have been highly gratified to learn that their Professors have fixed on a plan for a hospital (being one of four submitted to them), which they have agreed to recommend to the Council, and which is intended to hold between three and four hundred patients, a part of which, at any rate, will be completed by the commencement of our next session.

It is reported that the Professors at the same meeting were pleased to say, that they thought it their duty to state to the Council the high sense they entertain of the great attention, orderly and gentlemanly conduct, evinced by the whole body of the students during the past session, and from which they have derived great satisfaction.

By inserting the inclosed question, together with the above, you will much oblige a

STUDENT OF THE UNIVERSITY.
London University, May 7, 1832.

QUESTIONS *proposed to the Class attending* DR. ELLIOTSON'S *Lectures on the Principles and Practice of Medicine.*

1. What are the series of symptoms and anatomical changes in the progress and decline of inflammation of the lungs—in peripneumonia?

2. What are the series of symptoms and anatomical changes in phthisis properly so called—in tubercular phthisis?

3. What are the circumstances which favour or oppose the production of malaria, which favour or oppose its presence in any place after its production, and which favour or oppose its operation in any particular person?

4. What are the contagious diseases of the human body, and in what points do contagions differ?

5. What diseases in this country attack chiefly children, what youth, what adults, and what those in the decline of life?

6. What are the principal ectozoa and entozoa of the human race, and what are their ordinary seats respectively?

7. In what canals and receptacles are concretions usually found, and what is the nature of these concretions?

8. What is the diagnosis of phrenitis and delirium tremens, the causes, anatomical appearances, and treatment, of each?

9. What are the varieties of paralysis as to situation, symptoms, anatomical appearances, and mode of treatment required?

10. What are the causes, symptoms, and diagnosis, of tetanus and hydrophobia?

11. What is the diagnosis between cholera, dysentery, enteritis, colic, and diarrhœa?

12. What are the symptoms of scurvy, its causes, and treatment?

13. What are the varieties in the symptoms, and what the anatomical appearances of epilepsy?

14. What is the diagnosis between solidification of the lungs, hydro-thorax, pneumato-thorax, and hydro-pneumato-thorax?

15. What is the series of symptoms in small-pox, modified small-pox, and chicken-pox, and what the anatomical appearances after death from the first?

VALERIAN.

To the Editor of THE LANCET.

SIR,—As the time for collecting the roots of *valerian* for medicinal use has *now* arrived, the following lecture will I trust be kindly inserted, as containing information of importance in regard to this article of the materia medica. Yours truly,

JOHN EPPS.
11, Berners Street, May 7.

Virtues of Valerian.—Different Varieties of this Root sold in the Shops.—Heath Valerian the truly Medicinal.—Differences of Opinion regarding the Virtues of this Plant explained.

"SCIRE vere est scire per causas" is a dogma, Gentlemen, to which we cannot pay too much attention. The truth embodied in this, is one in which the scientific mind delights, because the knowledge thus founded has an accuracy and a certainty which no empirical knowledge can ever possess. The state of mind, connected with a knowledge of causes, throws a light where formerly darkness prevailed, and imparts for vacillation a fixedness of purpose. To illustrate these remarks, we may refer to *valerian*. This article has had its day of glory, and its day of dishonour. Its virtues have been exalted by many of the fathers of the medical art; the moderns, for the most part, despise it. How can this diversity of opinion be explained? Only by putting in force, in an examination of the plant, the adage—"Scire vere est scire per causas."

Well, Gentlemen, let us proceed to this examination, before which, however, it may be useful to recal your attention to an article called *extractum taraxaci*. You will remember that, in treating on this, similar differences of opinion in regard to its virtues existed; and you will also remember that Mr. Houlton has solved the problem, by proving that the extract of dandelion, as prepared by the *common* process, is inert, whereas, as prepared by the process he recommends (and dandelion extract as prepared by this process may be had at Smith's, 10, Nutford Place, and Rose's, 211, Tottenham Court Road), it possesses all the virtues which were ascribed to it. Now these circumstances, in regard to dandelion, have always acted upon my mind as an impulse to inquiry, whenever I found a great contrariety of opinion regarding the virtues of any particular medicine. Thus influenced, then, I began to investigate the subject of the virtues of valerian. I found, Gentlemen, but little help from pharmacologias, or works on materia medica; works generally mere compilations. Nature therefore must be examined. I pursued the way in which she led, and found my goal. Previously to mentioning this to you, it may be advisable to enumerate some of the testimonies in *favour* of and *against* valerian.

Valerian is spoken of by Dioscorides, who derives it, a Greek name, from φυ (phu), on account of its smell. This writer, as well as Galen, mentions it as an *aromatic*; and this, Gentlemen, I beg you particularly to bear in mind. Dominicus Panorolus published some cases exhibiting its use in *epilepsy*. Haller recommends it in hysterical affections, and considers it as useful in epilepsy. Fordyce, in his Treatise on Hemicrania, p. 91, praises it highly. Whytt on Nervous Diseases, p. 513, states that he used it with success in epilepsy. He combined it with manna. Columna reports that he was cured of epilepsy by this remedy, after other remedies had failed. M. Marchant, in the Memoirs of the Academy of Sciences, relate two cases of epilepsy where it was useful. The patients voided great quantities of worms. He always purged before using it. Dr. Withering recommends it as a laxative, and states that it acts when other means more violent have failed. *Catalepsy* was cured with it by Mr. Mudge; he gave ʒss doses of the powder. Fordyce recommends it in *dulness of vision*. Dr. Home, of Edinburgh, who, though not esteemed an authority in matters relating to the practice of physic, is a very good authority in reference to materia medica, states that he has found benefits from the use of this medicine in nervous affections. I myself have seen many benefits from its use, and I have been much disappointed. Most of the moderns condemn it. Dr. Woodville asserts that he has seldom found it answer the expectation of the prescriber.

Such, Gentlemen, is the contrariety of opinion; the solution will interest you.

Three modes of solving the problem occurred.

The *first* was—*May not the valerian root be gathered at the wrong season, when its virtues are nearly gone*? This, though affording in part the explanation, did not satisfy me, because I found that sometimes from the *yearly* supply from the *same* herbalist the utmost benefits, and sometimes no benefits at all, arose.

The *second* was—*May not the medicine have been given in too small doses*? This seemed to be a fair presumption, but, upon examination, the same irregularity in effect occurred during the use of large doses as of small.

The *third* mode, which I found to be the true one, was—*May not other roots, varieties of valerian, be mixed with the true root*? I entered upon the investigation of this, and while thus engaged, happened to fall in at a book-stall with a pamphlet written by John Hill, M.D., on this plant, establishing my views on this plant. This was published in 1758.

The species of valerian referred to in the Pharmacopœia, as producing the valerian root, is the *valeriana officinalis*. This is thus described by Sir Edward Smith—"floribus triandris, foliis omnibus pinatis foliolis lancialatis subuniformibus." This description is not sufficient to characterise the true valerian. A plant may have all these characters, without possessing the virtues of valerian. The real plant was called *valeriana sylvestria major* by the older botanists.

There are three varieties of this species of valerian—*the heath, the wood*, and *the water valerian*.

The heath valerian is the one possessing the medicinal virtues in the highest degree.

The wood valerian is considerably inferior, and the water valerian is still more so. Unfortunately the last is most easily obtained and most abundant, and hence there are 50 roots of this, to one of the heath. Hence originates the uncertainty of effect.

Let us now, Gentlemen, point out the distinctions between these varieties. These distinctions refer to the plants themselves, and to the roots. And first in regard to the *plants*.

Character of Heath Valerian.	*Character of Water Valerian.*
Height.—Two feet and two feet and a half.	Four feet in height.
Stalk.—Dusky green.	*Stalk.*—Pale green.
Lightly hairy.	Not so; thick.
Leaves.—Smaller than the water.	
Foliola.—The little leaves, composing each of the larger leaves, of a *deeper* colour, *narrower*, and covered with *white hairs.*	Larger—*fresh pale* green, and *smooth.*
Flowers.—Brighter red.	*Flowers.*—Pale red.
Clusters.—Smaller than in the water.	
Seed.—Small.	*Seed.*—Large and soft.

The wood valerian has characters intermediate.

The distinctions in regard to the *root* may be arranged thus:—

Heath Valerian.	*Wood Valerian.*	*Water Valerian.*
Colour.—Brown, nearly olive	Tawny or deep brown ..	Pale colour, nearly yellow.
Radicles.—Long, slender, with numerous hairy threads	....	Thicker fibres—more naked.
Roots.—Firm, tough	....	Tender, easily broken.
Smell.—Fresh, aromatic	....	No freshness, little aroma.
Slightly fetid	....	Heavy fetid smell. Cats busy themselves about this more than about the other roots.

The characters refer more exclusively to the *recent* roots. The *dry* roots may be distinguished also by the colour, as above described, the hairy threads, and the toughness. The heath valerian root, when dry, has no *cavity* in the *centre*, as the water valerian has. In the former, a circle of a greenish or pale hue, *surrounding the pith*, is observed: in the water, this circle is often black. The taste of the heath is spicy and pleasant, and after a little chewing, bitter and astringent, which is not the case with the water. If the root be dirty and dry, macerating the same in water will soon bring out the colour.

I hope, Gentlemen, that this statement will put you on your guard in purchasing the roots; but with the view of still more deeply impressing on your mind the necessity of caution, it may be as well to mention who are the collectors, and some circumstances connected with the collection of the roots, which tend to the introduction of the water instead of the heath valerian.

The collectors are generally very unfit for the employment; they are very imperfectly acquainted with botany, possessing but a very general knowledge of the external characters of the plant. Their object is *quantity* not *quality*. They take the roots which are most easily obtained, and most easily drawn up. Now the water valerian growing in clusters and in moist soil, is both most easily and abundantly obtained, and most easily drawn up. The heath valerian is more scattered in its growth; and growing in a closer soil, the roots are more difficultly obtained. The collectors often draw up other roots. Dr. Hill states, that he raised a plant of the *smooth water crowfoot* (poisonous) from a root sold as valerian.

Another circumstance regulating the collection is, that the gatherers pull up the plant when it has shot up the stalk, which enables them to draw it up more easily. Now the plant should be gathered *soon after the stalk has begun to appear above the earth.* After the stalk has fully risen, the plant has lost much of its virtues.

Another circumstance is, that many of the collectors know the plant only by its *flowers.* Hence they gather the roots when the plants are in a state of flowering, a period when *the juices of the plant are exhausted.* For, Gentlemen, I think you must be aware, from your attendance on botany at this theatre, that as the *generation of a new individual* is the grand end of the changes which the plant goes through in passing to the state of flowering, and as all the energies of the plant are directed to effect this end, when the end is obtained, the roots can be of little or no use. And, to establish this more fully, let me remind you of the fact, that an *annual* may, by its flowering being prevented, be made to exist for *another year*; in other words, a plant, which exhausting itself by flowering would die, is perpetuated by this exhaustion being prevented. Need I add any more to show the utter uselessness of the roots of valerian obtained from plants that have *flowered*?

Such, Gentlemen, being the difficulties connected with obtaining true valerian root, it seems not at all astonishing that such differences of opinion should exist in regard to the medicinal powers of this article of the materia medica.

The virtues of this medicine we shall enter upon on another occasion.—*Part of a Lecture at the Westminster Dispensary, Gerrard-street.*

CASE OF

SPINAL IRRITATION.

To the Editor of The Lancet.

Sir,—In some of the last Numbers of your valuable periodical, I have read several cases of affection of the spine, &c., which have brought to my recollection a case that came under my observation some time since, after reading an interesting clinical lecture on spinal irritation, by Dr. Corrigan, of the Jervis Street Hospital, Dublin. Should you deem it of sufficient interest, you will be good enough to insert it in your pages. I am, Sir, yours, &c.,

L. O. Fox.

Richard Hendley, a labourer, ætat. 36, was attacked on July 6, 1831, with inability to use the right arm and leg whilst walking to his work. Had felt a sensation of numbness in the affected limbs for two days previously. Complained of pain in the back. The arm and leg were painful, excepting when he lay down in bed, when he felt perfectly easy. Had, during the day, palpitation, darting pains about the chest, particularly on the right side. Pressure over the superior dorsal vertebræ brought them on immediately, and occasioned also severe pain in the right arm. Pulse 80; tongue slightly furred; bowels confined; slight headach.

Mitt. sanguin. a verteb. dors. ad ℥xij;
℞ *Hydrarg. submur.*, grs. x, *statim sumend.*
℞ *Ol. ricini*, ℥vj, *post horas duas sumend.*

July 7. Felt better to-day; had less palpitation; pressure over the dorsal vertebræ occasioned less pain in the arm. Bowels slightly acted upon.

Applicentur hirudines xx *verteb. dors.*
Repetatur medicinam.

9. Leeches bled so profusely as to occasion syncope. Expressed himself greatly relieved; scarcely any pain from pressure on the spine, excepting tenderness from the leech-bites; bowels freely opened; pulse 76, and soft.

I need not trouble you with any further account of the case, otherwise than that the man rapidly recovered, and at the week's end was enabled to follow his usual employment.

Remarks.—There is a striking analogy between the foregoing case and some of those related by Dr. Corrigan in The Lancet of May 7, 1831, to which the name of spinal irritation has been applied.

I was induced to follow the simple method of treatment recommended in the lecture referred to, which succeeded most satisfactorily.

Broughton, Stockbridge, Hants,
May 4, 1832.

ROYAL COLLEGE OF PHYSICIANS.

Monday, April 30, 1832.

Sir H. Halford in the Chair.

PAPER ON CHOLERA.

A large assembly of Doctors, clerical, legal, and medical, took place in the library of this College. Amongst the most prominent were the celebrated linguist Dr. Ireland, Dean of Westminster, Baron Vaughan, the Dean of Chichester, and Sir Charles Mansfield Clarke, Bart. and M.D. *jure divino*. A paper on the threadbare subject of cholera was presented from Dr. Heberden, and read with feminine grace by the President's nephew, Dr. Hawkins. Dr. Heberden took a historical view of the various contagious maladies that have prevailed at different epochs, and compared them with recent epidemics, and with the rise and progress of the cholera, "in its regular movement from the rising to the setting sun, and from the torrid zone to the arctic circle." From all his researches, the writer came to the conclusion, that two conditions were necessary to the communication of the disease,—a predisposing state of the body, and the presence of the morbid principle. The rule according to which these circumstances acted, might mathematically be described as follows:—the relative power of the two causes did not matter, provided the resulting influence attained a certain amount. It signified not what relation A, the predisposing cause, might bear to B, the morbific matter, if C, the combined exciting cause, was of a given value. The author surveyed the whole field of controversy, and analysed with considerable ability the arguments employed, both by the contagionists and the anti-contagionists. He related numerous cases which seemed conclusive on both sides, and summed up by a statement of the above-quoted creed. In contradiction to the assertion that the cholera was not considered contagious in India, he alluded to the opinion of the late Sir Thomas Monro, a distinguished officer, and one of course quite free from any party bias on the subject. Sir Thomas died of cholera, and always conjured his friends not to stay in his room, lest they should become affected. In illustration of the influence of the morbific principle on constitutions so disposed to disease, he narrated the history of his own child, who caught the measles from his brothers and sisters. The disease set in with an affection resembling croup. He consulted his apothecary, whose experience was more extensive than his own on the subject, but that gentleman had never known an instance of croup thus preceding the measles. Dr. Heberden was impressed with a sense of the immediate danger of his child, and sat on the bed all night ready to administer to its wants. The antiphlogistic treatment was adopted, and by the morning the little patient was much better, but the measles continued their course. The Doctor himself was now conscious of a sense of constriction almost amounting to suffocation in the throat, and especially in the larynx. This was intermittent, and returned more severely every quarter of an hour. In the course of a few hours, however, it went off entirely, but left considerable debility of the whole system. This the doctor considered a good illustration of the resistance offered by an unwilling constitution to the reception of the virus. The paper was interspersed with pious ejaculations, and concluded with a suitable peroration.

DISSERTATION ON PHLEGMASIA DOLENS IN MALES.—CASE OF THE EARL OF LIVERPOOL.

At the conclusion of Dr. Heberden's paper, the President begged leave to read a few remarks he had drawn up on the subject of phlegmasia dolens. Until lately, this disease had been supposed to affect women exclusively, and had accordingly been named "Depôt du Lait," but the more correct pathology now advocated, coincided with the true history of the disease. It was true that females suffered it after parturition, from the pressure of the gravid uterus on the iliac veins, and the violent changes incident to that state. The disease, however, is occasionally met with in men. The late Earl of Liverpool was affected with it many years; he was attended by the late Dr. Pemberton and Sir Astley Cooper, and was treated with leeches, and the usual antiphlogistic means. After Dr. Pemberton's death, Sir Henry Halford was consulted. He found the disease affecting the left groin and thigh, and extending into the leg. Upon examining his Lordship, Sir Henry found nothing remarkable, except a remarkably slow pulse, making about forty-four beats in a minute. This struck his attention, for he was informed the usual habit of the pulse was seventy-four beats in a minute. On communicating the fact to Sir Astley Cooper, that eminent surgeon ingeniously conjectured that this anomaly was due to the obliteration of the left iliac vein, a circumstance which would occasion a retardation of the blood from the inferior members to the heart, and thus occasion a delay in the systole of that organ. His Lordship subsequently found the sight of the left eye affected, and soon after had an attack of apoplexy. This was followed by a series of epileptic fits, one of which proved fatal. The post-mortem examination afforded a curious confirmation of Sir Astley Cooper's conjecture, for the left iliac vein was found completely impervious (as was shown by a drawing). On examining the brain, a large cavity filled with serum was found over the right ventricle, and extending upwards as far as the pia mater, vaulting that part of the hemisphere. This case Sir Henry thought particularly instructive, and suggested to him the idea, that phlegmasia dolens was not an unfrequent cause of paralytic and apoplectic disorders; for, consistently with the ingenious hypothesis of Sir A. Cooper, the obliteration of the iliac veins clogs the action of the heart, and destroys the balance of celerity between the upward current from the inferior parts, and the downward current of blood from the head. An unwonted accumulation of blood in the veins and sinuses is the consequence, and, increasing with the increase of the cause, gradually leads on to an apoplectic paroxysm. Sir Henry then related two other cases of phlegmasia dolens in men. They were very similar to the case of the Earl of Liverpool, and were succeeded by a marked tendency to head affections. The patients were still living, and under the care of Sir Henry, who stated he should regard with suspicion any tendency to cerebral derangement. Sir Henry concluded by stating, that the object of his short paper was to contribute a few facts in support of the new views entertained of the pathology of phlegmasia dolens, and towards the establishment of the coincidence of obliterated iliac veins, and cerebral nervous affections, as a principle of pathology.

On the table were displayed the splendid plates of Alibert, numerous publications, chiefly German, and a curious anatomical work, published at Ulm in the year 1639. This was entitled "Catoptrum Microcosmicum," the author, Johannes Rammelini; the plates were arranged so as that the different parts were shown in layers, a plan which has been claimed as original by Mr. Tuson.

ROYAL INSTITUTION.

April 13th, 1832.

EXPERIMENTS RELATING TO THE LAWS WHICH REGULATE ANIMAL LIFE.

Relation between Irritability and Respiration.

Dr. Hall has pursued in this province of physiology, the experiments instituted originally by Dr. W. F. Edwards, to determine the laws which regulate the influence of physical agents upon the vital principle. Dr. Edwards made an elaborate series of experiments to ascertain these laws. The physical agents referred to, comprehend atmospheric air, water, and temperature; the two first constituting the media in which all animals exist, and the last affecting in common the inhabitants of both media. The agency of the air around us, water, and heat and cold, have often been the objects of *chemical* inquiry, from their known great influence upon the animal economy. The changes effected by the phenomena of animal life upon these agents have been accurately examined, and partly reduced to a mathematical precision of calculation.

Spallanzani and others have viewed the subject as it regards physiology, but with such results as left the field open to subsequent investigation. Dr. Edwards seems to have seized upon the deficiencies of his predecessors, and by going over their ground and extending his own inquiries, he has arrived at most interesting and important results. These he has divided into four parts, as they relate to the different orders of the animal creation. The first includes some of the lower animals particularly tenacious of life, and of cold blood, such as frogs, toads, and salamanders. The second part is devoted to other animals of cold blood, and of the vertebrated order, as fish, and those reptiles which include lizards, snakes, and turtles. The third part refers to the warm-blooded animals. The last division of the work is dedicated to the influence of the physical agents alluded to, upon the human race, and upon vertebrated animals. Dr. Edwards proceeds to examine the action of physical agents upon the simplest forms of organised beings, extending his inquiries upwards in the scale of the animal world.

The air exercises its influence uniformly upon the four classes of vertebrated animals, and their different families are similarly exposed to the action of the atmosphere by respiration. Dr. Priestley laid the foundation of our chemical knowledge of gases in their relation to respiration. Oxygen gas was shown by that philosopher to change the colour of blood from dark purple to bright crimson. Lavoisier founded a chemical theory upon this discovery of the agency of the air, which was afterwards applied by Goodwin to physiology. The latter author demonstrated, that the exclusion of atmospheric air produces death in animals, by preventing the dark-coloured blood usually circulating in the veins from becoming crimson. The state in which any animal may be thus placed, is known by the term asphyxia, by which is meant, a deficient aërification of the blood, from whatever cause.

Bichat, after several experiments, arrived at the important conclusion, that *the venous blood circulating through the brain, causes a cessation of the functions of that organ, and that, in consequence, the heart loses its action.*

Le Gallois further found, that *when dark-coloured blood circulated through the spinal marrow, the motions of the heart ceased*, and thus determined not only the relations of the nervous system to atmospheric air, but also those of the respiration and the circulation.

Impressed with a conviction that the phenomena of cold-blooded animals were too extraordinary to be noticed cursorily, and required extensive observation, Dr. Edwards proceeded to form an estimate of the comparative influence of the air and water upon the nervous and muscular systems of cold-blooded animals, which the singular modifications of life among reptiles in particular, afford ample means of ascertaining.

After removing the heart and bulb of the aorta from a certain number of salamanders, and immersing them in some common aërated water, and some in water deprived of the air by boiling, he came to the conclusion, *that the air is the most proper medium for the action of their nervous and muscular systems in their insulated state, the respiration and circulation of the blood being both suspended.* Dr. Edwards also confirmed the observation of Goodwin as to the effect produced on the colour of the blood.

The next point to determine was, the influence of the air upon the same animals exercising the respiratory function, and retaining their circulation, compared with those deprived of these functions. Having placed a number of frogs circumstanced as in the first experiment, and an equal number left entire, in dis-aërated water, it was found the entire animals lived longest; and it is deduced from this, that although dis-aërated blood furnishes but an ephemeral sort of existence, it exercises nevertheless a comparatively favourable influence upon the nervous and muscular systems, since it tends to the prolongation of these functions.

Dr. Edwards also investigated the phenomena of *asphyxy* produced by *strangulation* in the same animals. As a comparative illustration, an equal number of frogs were submersed in water, all of which died in about *ten or eleven hours*, while those which were strangled lived from *one* to *five days*.

In comparing the effects of strangulation by drowning, it was to be supposed either that these animals exist a limited period without the necessity of the nervous system being in contact with the atmospheric air, or that the air influences their blood through the integuments of the body. This led to experiments on *cutaneous transpiration*, and after a suite of interesting experiments, this extraordinary fact was established, that toads can live far longer in a state of asphyxy, confined in solid substances, than when submersed in water, a fact remarkable, inasmuch as it affords an exception to the general rule as regards reptiles, that all animals require a *constant* supply of fresh air for the maintenance of their existence.

Dr. Edwards found, that although a certain quantity of air enters the boxes in which the animals were placed, yet it is far too little to maintain life. His conclusion, therefore, is correct, that these animals can live longer in *solid substances* than in a limited quantity of *dry air*. It is supposed that a rapid and abundant transpiration from the body, united with deficiency of air, seems to be a greater cause of dissolution, than confinement in solid substances, wherein there is no waste by transpiration.

These experiments, on being repeated with reference to the influence of temperature, proved that the animals submersed in aërated water lived longest, the lower the temperature of the surrounding medium was. It was found, too, that the seasons exerted a peculiar influence on the constitution of these animals, and that the autumn was more favourable to the duration of life than summer, and winter more than autumn. Dr. Edwards found that frogs did not remain submersed in stagnant water more than two days and a half. Frogs are less active during winter than at other seasons, but they never lose their motion. By a final series of experiments, Dr. Edwards ascertained that frogs pass the winter in an animated state in water, not becoming stiffened as in ice, and that they need not approach the surface of the water in order to respire, provided the water they inhabit be renewed at intervals; but if the water be not renewed, the frogs perish. Thus showing, that these animals are able to respire the air contained in the dense medium of water for an indefinite period, and just as easy as the finer medium on land.

Respecting the action of aërated water on the skin, the conclusion drawn is, that the continuance of life, in the experiments made upon this point, was due to cutaneous absorption, since the animals were in a state of *asphyxy* as regards the lungs, for no water was ever seen in the lungs. It was shown also, that *running* water was most favourable to these amphibious creatures, even at a higher temperature than ordinary, and this was due apparently to its constant and unceasing renewal.

The general result of the experimental investigation appears to be this,—that these animals when submersed in water, are influenced by three circumstances:—1. The presence of *air* in water; 2. The *quantity* of its renewal; 3. The *temperature* of the medium.

Dr. Marshall Hall repeated with great industry and ingenuity the experiments of Dr. Edwards, and in some respects refined upon them. These shall be more particularly alluded to in a subsequent number. He ultimately arrived at this important corollary, that the *irritability* of animals, of whatever class, is in the inverse ratio of the perfection of the respiratory function.

ELASTICITY AND BEARING PROPERTIES OF CAST IRON.

May 4th.

Mr. COTTAM delivered a very interesting and animated lecture on the above subject. He said that many engineers were guided in their investigations into the tenacity of this pre-eminently useful metal, by taking as a datum its breaking point, when submitted in various forms to the influence of weight or pressure. Having ascertained this, the class of inquirers alluded to considered one-sixth of the force necessary to destroy its tenacity, as the proper weight to be habitually imposed upon it. Mr. Cottam showed clearly, however, that the breaking force was by no means regular in its quantity, for the resistance varied constantly according to the manner or direction in which the opposing force was applied. It could not, therefore, be safely adopted as a datum in any strict or important calculation. This gentleman also stated that the extreme point of elasticity was a much safer basis to reckon upon, for it bore strict relation to the length, cubic size, and toughness of the materiel. Mr. Cottam made several conclusive experiments to determine the point of deflection at which any given bar of cast iron could recover its form, a greater weight producing a permanent curvature. The measurement was effected by means of a graduated line suspended at a right angle with the bar subjected to examination, and which was placed horizontally. The pressure was applied in some instances through the medium of weights, in others by means of a Bramah hydrostatic press. A small but beautifully constructed machine of this kind was employed for the purpose. After numerous experiments, Mr. Cottam adopted as a starting point in his calculations, the fact that a bar of cast iron of ordinary quality, a foot long and an inch square, had elasticity sufficient to recover from a weight of 850 pounds. Thus a medium is discovered for ascertaining the resisting force of a bar of iron of any size, this force being always in the inverse ratio of the length of the bar, in the direct ratio of its width and of the square of its depth. The lecturer enumerated instances of errors which had appeared in the calculations even of eminent artists on this point, and stated it was a rule in his practice to take as a datum for his calculation a force somewhat less than sufficient to overcome the elasticity. He also alluded to the injurious effects induced in the *grain* of the iron by the fantastic forms occasionally prescribed by architects; for every complication of shape adds to the risk of producing flaws in the metal through the intervention of bubbles of air, and particles of sand, or other extraneous matters. As an illustration of the safety ensured by cast-iron supports and joists to floors, Mr. Cottam stated, that in estimating the resisting force to be given to a dancing-room floor, he had allowed a square foot of space for every individual present, and consequently supposed the room packed, and every person in contact breast and back, (a degree of crowding, by-the-bye, that would inevitably produce suffocation,) that each person weighed twelve stone, and were engaged simultaneously in vaulting. The impulse thus imparted would amount to 60lbs per square foot. To meet this supposed weight, he supplied a counter-force of 150lbs. per square foot in the form of square iron joists.

On the library table was exhibited an ingenious automatic drawing machine, called the "Perspective Tripod," and also a curious prehensile elastic hook, adopted as a substitute in case of amputated fore-arm.

HUNTERIAN MEDICAL SOCIETY OF EDINBURGH.

At the last meeting of this Society, for the session which has just terminated, Mr. Harrison read a paper on Injuries of the Head; and Mr. N. Rogers, Senior President, delivered, as is usual, a valedictory address, in the course of which he took a retrospective view of the proceedings during the session, which he regarded as one of the most interesting which had been passed since the institution of the Society. He enlarged particularly on the all-engrossing subject of cholera, alluded to the prompt and efficient attention which it had received both in that Society and the City, and quoted the high eulogium of Dr. Chalmers on the medical arrangements of Edinburgh; arrangements which had been the universal and unqualified theme of admiration. This led to some observations on the reception which the disease had met with in London, and the earnest but unheeded warnings of some of the medical journals. A short review was then taken of the progress of medical literature during the past year, adverting particularly to the Cyclopædia of Practical Medicine, and the Dublin Medical Journal, which latter subject led to some remarks on the eminence of the medical corps, and on the rising medical school of that city. The address of Mr. Rogers was delivered to a very numerous audience, and was received with marked applause.

WESTMINSTER HOSPITAL.

HYPERTROPHIA AND ENLARGED CAVITIES OF THE HEART.

Sarah Barlow, aged 25, with a pale skin and fine complexion, light hair and eyes, and thick lips, was admitted on 7th March ultimo, with excessive action of the heart. She has been subject to palpitation for these five years, and has in consequence been several times in this hospital. She now complains of acute pain in the cardiac region, dyspnœa, and violent beating of the heart, which are excited even on walking across a room. The pain is most severe down the arms and between the shoulder blades, and there is headach in the occipital region. The heart's action is heard over an unusual extent, and its impulse is stronger than natural, and an occasional irregularity is perceptible in the contraction both of auricles and ventricles. Pulse equal at both wrists, weak, and 102. The respiration is easy, but louder than common, and interrupted by deep-drawn sighs, which relieve the feeling of oppression at the base of the sternum. She has slight cough, but no expectoration. The chest sounds well on percussion; surface rather cold, especially at the extremities; tongue exsangueous, clean, and flabby; habit of bowels very loose. The appetite is good, and the catamenia are regular; she sleeps but little. and even then often starts, imagining she is falling. Fish diet; and the following powder to be taken three times a day:—

Dover's powder 5 grains;
Tartrate of iron 10 grains.
Mix into a powder.

March 14. She has gradually improved since admission until last night, when she was suddenly affected with the greatest pain in the chest for three hours. The heart beat with great force, frequency, and irregularity; the countenance became anxious, and the skin was bathed in perspiration. Assisted by an antispasmodic draught, the paroxysm went off. To-day she is better; pulse 80; remedies continued.

23. She had been put on full diet for a day or two, when a violent attack of palpitation, such as has been described, came on, attended with acute pain under the left breast. The heart's beats could not be counted. She was relieved by the administration of a *henbane* draught; but in the evening the attack returned with increased violence. These attacks are considered due to the "full diet" to which she has been subjected; she is therefore put upon low diet, and discontinues the use of the iron powders.

24. The patient is much quieter this morning; the face is pallid; tongue chopped, and the bowels open. The following pill is administered:—Take of *opium* gr. ss, *sulphate of iron* gr. ij. Mix into a pill, to be taken twice a day.

April 1. The patient is much improved; she sits up, can exert herself moderately without inconvenience; colour heightened, and appetite strengthened.

9. Another paroxysm came on yesterday after slight exertion. The pain in the region of the heart is so violent, that she cannot turn without assistance. Each systole of that organ is as a blow from a sledge hammer, and the accompanying dyspnœa is consequently hideous; pulse 114. To be cupped on the affected part to twelve ounces and an anodyne draught to be administered.

10. Much relieved by the cupping; pulse 90; considerable tremor.

24. From the last date the patient continued to improve; her appetite soon recovered, and under the influence of tartrate of iron she soon gained flesh and colour. The catamenia are right in all respects; the bowels are regular, and the sanguiferous and nervous systems are perfectly tranquil, except when excited by any slight shock, which always produces a temporary increase of action. She was discharged yesterday as convalescent.

PLEURITIS ATTENDED BY EFFUSION BETWEEN THE SURFACES OF THE PLEURÆ.

Jane Hambling, 24 years of age, a servant, was admitted the 20th March, having acute pain of the left side. She had had tolerable health all her life-time, until the last month, when she was affected with the symptom first mentioned, which was attended by cough. The pain has continued increasing up to the present period; and the cough was provoked by every attempt to make a full inspiration. She had headach also, and pain in the back.

On admission she presented the following particulars:—The patient tall and well-formed, and has a remarkably fine skin, which however is at present dotted with a few papular points. The thorax is of proper symmetry and dimensions, except perhaps a slight fulness perceptible in the left side. Respiration 34, painful, and producing cough. Its murmur is not audible in any part of the left side of thorax, but it is puerile in the right. Percussion produces a dull sound over the whole of the left side, and laudable responses in the corresponding points of the right side. Pulse 112, full and soft; tongue furred on the *left half only*. Bowels open. On applying the stethoscope, with the stopper in, on the posterior phase of the left thoracic region, and in the convexity of the 5th and 6th ribs, a very distinct œgophony is perceived, corresponding to the patient's voice. On the opposite side no such phenomenon is discovered. Face flushed, and countenance anxious. Venesection to ℥viij; half a drachm of ipecacuanha wine to be taken in camphor julep, every two hours.

22. Blood slightly buffed. The patient breathes more freely and less frequently; the same stethoscopic signs are extant, and the œgophony is remarkably distinct. Pulse 112, same character; tongue furred on the same side. Bowels open. Venesection to ℥viij; remedies continued.

24. The pain of side relieved by the last bleeding; pyrexia diminished; face flushed notwithstanding; dilates the chest a little more. A very feeble respiratory sound is detected by the stethoscope. Œgophony still evident, though less distinct; tongue still furred on one side; bowels not open since yesterday; pulse full, and about 90.

25. A return of pain in the affected side, with aggravation of symptoms. Venesec-

tion to ℥xij. Half a grain of tartrate of antimony every two hours.

27. Feels better; can dilate the chest with more freedom and without pain. The respiration on the left side is now distinguishable by the naked ear. The *puerility* of that on the opposite side is lessened. The cough is less. Percussion now evokes a tolerable sound in the affected side. The œgophonism is still perceptible in the same place; the tongue presents the same curious features; the pulse is 92, and soft. A nausea is kept up by the antimony. A warm bath.

29. A return of pain and dyspnœa; a blister to the side.

April 1. The patient was relieved by the blister, and is much improved; an occasional pain, however, annoys her under the left clavicle. The respiratory murmur is equally distinct on both sides. Percussion educes natural sounds, and œgophonism is distinguishable. Tongue clean; bowels open; pulse 92, and full. *Eight leeches to the pained part.*

5. The patient is much better, but she still has pain in the left side upon coughing, which she does frequently; the respiration nearly natural; the countenance is slightly anxious; a blush mantles each cheek, and forms a striking contrast with the clear white of the skin generally; the tongue is again furred *on the left side only;* bowels open; pulse feeble; 130 on sitting up, somewhat less in the recumbent posture. In the evening she has febrile exacerbations. She sleeps well, and has no night perspirations. Fish diet; and the following pills three times a day: *Foxglove powdered, squills, and ipecacuanha, of each a grain;* to be made into a pill with a proper medium.

9. The patient has been materially benefited by the last medicine. The pulse is 100, full. The cough is less urgent. The tongue is furred, bowels confined. The respiration easy, but a little accelerated. To omit the pills, and resume her original ipecacuanha-mixture. The phthisical diathesis is less evident than formerly.

May 1. The patient has continued to improve since the last date, though with an occasional disposition to relapse. This was obviated by leeches, &c. The patient's appetite is now good; her bowels regular. She sleeps well. The pulse is 80, full, and soft, and she appears perfectly free from pleuritis and from hydrothorax. The delicacy of her constitution being great, she is detained in the hospital until finer weather and her more complete convalescence shall render her on departure secure from the risk of relapse.

JAGGED WOUND OF THE HAND.—HEMORRHAGE FROM THE PALMAR ARCH.

March 27, 1832. James Stanson, 30 years old, a mechanic in the employment of Mr. Southam in his saw-mills at Pimlico, whilst applying a block to the saw, slipped, when his right-hand became entangled and severely lacerated. A copious hemorrhage immediately took place, and was evidently arterial. It was however promptly stopped by Mr. Knox, the cupper to the hospital, who happened to be present, and who made pressure on the brachial artery. The man was brought to the hospital immediately, and on being inspected by the surgeon on duty, it was found that the palm of the hand had been actually *minced* by the teeth of the saws. Both palmar arches were wounded, and the hemorrhage was profuse. The capsules of the first joint of the index and ring fingers were also cut open. It was determined, after a careful examination, to take up the several bleeding points that should present themselves rather than tie the ulnar or radial, or both, above the wrist. Several ligatures were accordingly applied, and the hemorrhage was arrested. The sides of the wounded chasm were then approximated, and the ends of the incised tendons brought as near each other as possible by closing the hand. He was carefully placed in bed, cold lotions were applied over the hand and arm, and aperient medicine was administered.

28. The patient passed a restless night; he is feverish; there is a good deal of pain and heat in the wounded hand. The bowels have been well opened; cold lotion continued.

29. Much inflammation of the affected arm; pulse 90, hard; headach; furred tongue; fever; bowels opened by the medicine. Venesection to ℥xvj. He was relieved by the bleeding.

30. Considerable phlegmasia still remains. The absorbents are becoming red and painful. Tongue furred; great pain of the hand, reaching up to the shoulder; bowels open. A bread poultice applied to the hand.

April 1. The febrile symptoms are more urgent than ever. There is heat, restlessness, rigors, furred tongue, headach, and torpid bowels. The pain and inflammation of the hand are very high. The absorbents, too, are decidedly inflamed, and the axillary glands are tumefied. Venesection to ℥xvj. Calomel bolus to be swallowed at bed-time; saline medicines at proper intervals; the hand to be involved in a poultice, and the track of the morbid absorbents to be washed with a strong solution of argentum nitratum.

April 6. The remedies last noted were attended with the happiest effects. The fever and inflammation subsided considerably. The lunar caustic proved perfectly effective in stopping the herpetic inflammation of the lymphatics. The general appearance of the patient is consequently much improved, but a collection of pus is evidently forming above the wrist, apparently in the theca of the tendons.

24. Mr. Taylor opened the collection of matter alluded to. It was situated in the theca of one of the tendons of the flexor sublimis. The ligatures came away on the 20th day. The wound has nearly healed, and will have comparatively a narrow line of cicatrix. All the fingers are curved naturally towards the palm. He has, however, as yet but very imperfect command over them.

May 1. A few granulations have sprung up above the level of the adjacent integuments, otherwise the hand is perfectly healed. Some of the tendons appear to have united, for he has acquired some power of flexion over the fingers. Their lateral movements through the medium of the inter ossei remain unhurt. His general health is quite re-established, and his spirits are good. The hand will experience very little injury in an operative point of view, for the functions of the thumb are perfect, and he has consequently sufficient prehensile power for his peculiar art.

LONDON HOSPITAL.

CURIOUS INJURY.—FRACTURE OF THE SKULL.

April 12.—A man was brought into the accident ward of this hospital with a lacerated wound of the right upper eyelid; he stated, that while working on board a ship discharging coals, a hook which they use for raising the coals caught his eye, and he was elevated to the height of several feet before he could be extricated, when he withdrew the hook himself. On his admission here, he did not appear to be suffering from any serious accident; the eye was found to have escaped injury, and no fracture could be detected; his respiration natural, pulse 76, full, but not more than might be expected in a robust man, pupils obedient to the stimulus of light; has no pain in the head. *V. S. ad* ℥xx ; *Cold lotion to the forehead ; Hydr. submur. gr.* ij *om. 2nd hor.*

13th. Passed a quiet night; pulse 74, full, free from hardness, has very little pain in the head ; bowels opened three times :—*Hirudines* xij *fronti. To continue the calomel.*

Evening, six o'clock. Symptoms of compression of the brain very suddenly came on ; his breathing became stertorous, pupils contracted, and insensible to the stimulus of light ; pulse 52 and labouring ; cannot be roused by any noise ; at this time a quantity of blood mixed apparently with cerebral substance, to the amount of about two ounces, escaped from the wound.

V. S. ad xx oz. ; *blood buffed and cupped ; Hirudines* xx *temporibus.*

He lingered on in this state till two o'clock the following morning, when he died. On an examination post mortem, the orbitar plate of the os frontis was found to be completely smashed, and a considerable portion of the anterior lobe of the right hemisphere of the brain wanting, it having escaped through the wound.

When the companions of this man found that they had elevated him by the chain and hook, they suddenly let go the rope in alarm, and the poor fellow was thus let fall heavily on the deck.

COMPOUND FRACTURE OF THE TIBIA AND FIBULA.

W. F., ætat. 50, was admitted on the night of the 18th April, with compound fracture of right leg.

The seat of the fracture was in the lower third of the leg ; about two-thirds down the leg was a small wound through which the upper fractured portion of the tibia protruded ; this bone was also fractured about one inch and a half lower down. The fibula also was fractured.

The account he gave of the accident, which occurred but a short time before his admission, was, that while walking on the side of the road his foot slipped, and he fell with his leg under him, partly resting on the curb stone.

The portion of bone that protruded was small, and by means of moderate extension it was reduced, and the ends of the bone brought into opposition. The limb was placed in the straight position, with splints, and cold lotion ordered to be kept constantly applied. *Tinct. opii*, M. xl, *statim sumend.*

19. Slept but little ; pulse 86, full ; complains of pain in the leg, which is hot and swollen ; to have twenty-five leeches applied immediately, and afterwards the cold lotion. *Tincti. opii.* M. xxx *h. s. s.*

20. Passed a restless night ; pulse 84 ; tongue moist and tremulous ; skin cool and moist ; bowels confined ; has no appetite ; limb feels hot and painful. *Ol. ricini* ℥j, *statim s.* ; and the following draught :—℞ *Mist. salinæ ; mist. camphora* aa ʒvj ; *Tinct. opii* M. x, *ft haustus, 4tis hor. s. ; Tinct. opii* M. xxx *hora somni sumendus.*

21. Passed a tolerable night. Bowels open three times, skin cool, tongue clean,

has no appetite, is rather thirsty, pulse 88; the limb is swollen and painful. To continue the medicine and anodyne.

22. Slept four hours during the night; pulse 94, soft; tongue clean, bowels open, skin hot, but moist, appetite bad, takes nothing but a little tea: to have twelve leeches to the leg, and to continue the medicines.

23. Pulse 104, moderately full; tongue slightly furred, bowels open, countenance anxious, skin cool, appetite none; has considerable tremor of the hands. To continue the draught as before ordered, with the addition of *Tinct. opii*, M. x.

24. Pulse 110; slept for four hours during the night, skin warm, but moist; bowels open, tongue slightly furred and tremulous, with considerable tremor of the hands. To continue the medicine as last ordered.

In the evening violent delirium came on, so much so as to require the restraint of the strait-waistcoat.

25. The delirium continued throughout the night; pulse 104, weak; skin cool, and covered with a clammy perspiration:—℞ *Mist. salinæ* ℥iss; *Tinct. opii*, M. xx. *ft. haust. omni hora s.*; *Hirudines* xx *stat. applicand.* He continued in this state until nine the following morning, when he died.

We could not learn if any post-mortem examination was made.

INJURY TO THE SIDE.

J. N., ætat. 78, was admitted on the 18th of March, with injury to the left side. This man was employed by the water-side, when a log of timber slipped, threw him down, and fell upon his side. On his admission here, a fracture of the left seventh rib near its angle was readily detected; the breathing was considerably affected; severe pain on coughing, pulse full and frequent, tongue foul; he was bled to ℥xvj, and a broad flannel roller applied round the chest. He was ordered—*Hydr. submur.* gr. ijss; *Pil. scillæ Comp.* gr. iv; *quarta quaque hora sumend.*

19. Slept but little, his cough being very troublesome; feels great pain on pressing the injured part; pulse full and strong; bowels open, tongue white, expectorates with difficulty: some viscid mucus:

20. Passed a very indifferent night; complains of severe pain in the chest on coughing, or making a deep inspiration. To be bled to ℥xvj, and continue the pills.

21. Blood drawn yesterday buffed and cupped; feels greatly relieved in his chest; pulse considerably reduced, skin cool, tongue clean; complains of his mouth being sore.

22. Passed a good night, but complains of his cough; pulse full, but soft; appetite bad, tongue clean, bowels confined:—℞ *Ol. ricini* ℥ss *stat. sumend*; *Mist. salinæ* ℥iss; *Tinct. scillæ* M. xv.; *4tis hor. sumend.*; *Emplast. lyttæ sterno.*

24. Feels much better; cough much less troublesome; has very little pain in his side; pulse moderately full, tongue clean, mouth very sore, bowels not relieved yesterday: to take a dose of house medicine.

28. From this time he gradually recovered, and was discharged quite well on the 19th of April.

Note.—The correspondent "A London Hospital Pupil," who pretends to reply to the report of the 31st March, of an hydrocele case, seems to imply that the notice of this case was incorrect. I can only state, that up to the 10th of March, the last date of the report (not the 25th as the "Pupil" says), the case presented every appearance of failure of the operation. I am happy to say the conjecture which I then formed as to the result has proved premature, and that it is true that the patient has since been discharged cured.—REP.

SUPPOSED CHOLERA IN CUMBERLAND.

To the Editor of THE LANCET.

SIR,—I perceive from a constant and regular perusal of your excellent journal, THE LANCET, that this all-engaging disease, the cholera, constitutes at present the chief topic of discussion in the medical societies in the metropolis.

While this disease arrests the attention of almost every thinking person in and about the great city, it does no less so here in this remote part of the country; for no sooner is a person seized with vomiting and purging, than it is at once pronounced to be a decided case of cholera, (and by some professional men it is called *Asiatic* too), notwithstanding the absence of the leading features of that formidable disease, viz.—spasms of various parts of the body, especially the extremities, rice-coloured dejections and ejections, suppression of urine, &c. &c.

On reading over your Number of March the 24th, my attention was particularly attracted by Mr. Dobson's account of the epidemic cholera of Leeds in 1825, as a disease bearing some resemblance to it, which is and has been prevailing here for some time past. I have attended during the last eight months several cases of a disease here, which invariably commenced with vomiting and purging, or with vomiting and dysentery, but always unaccompanied (as far as my observations go at least) with spasms of the extremities, or abdominal muscles, the matter vomited and dejected being also almost always either of a dark-green or a dark-brown colour, and of a bilious nature.

The urine, in many cases, was also of a deep-brown colour, and I could never observe any suppression of it. In addition to these symptoms, there were, great prostration of strength, depression of spirits, and a sense of sinking or fainting. The face was sometimes pale, sometimes flushed; great thirst; tongue sometimes moist and white, at other times white and dry; a small quick pulse; fever, for the most part of the continued typhoid kind. It is rather remarkable that no blood was ever observed in the stools either by myself, the patients themselves, or their attendants. The pulse was generally fuller and stronger in cases attended with dysentery, than in those accompanied with diarrhœa, but the other symptoms were mostly the same.

I have often observed too that, in the course of a few days after the onset of the complaint, patients complained of more or less pain in the region of the liver, both in cases of purging and dysentery; that the tunica conjunctiva became more or less yellow, while there was no such appearance in any other part of the body.

The cases I mention bear a strong resemblance to those mentioned by Mr. Dobson, in his report of the epidemic cholera of Leeds in 1825. I think all practitioners ought to be very careful in deciding and giving their opinions on these cases at first sight, since the public mind has become so much excited and alarmed by the fatal and destructive tendency of this very formidable disease. Besides, too, it would often save their own reputations, as I have heard of many cases occurring to neighbouring practitioners of a similar nature to those I have described, which by them were pronounced to be decided cases of *Asiatic cholera*.

Now, Sir, I have been in the *East Indies* myself, but the symptoms of the epidemic prevailing here are very different from those attending the formidable and *gigantic* disease of the east. I, of course, regard the disease which is now so prevalent here, as fever attended with an unusual degree of functional derangement of the biliary organs, and an increased and diseased action in the mucous membrane of the alimentary canal. I have attended a great number of cases but none have proved fatal, except that of the child, whose case I have already mentioned.

I am, Sir, your obedient servant,
E. WILKINSON,
F.L.C.M., L.R.C.S.E. & A.H.L., &c
Aspatria, Cumberland,
April 27, 1832.

AT a public examination of the pupils attending Dr. Stratten's Lectures on Materia Medica and Pharmaceutic Chemistry, held on two successive days, April 24th and 25th, 1832, Mr. Frederick Row of Plymouth, and Mr. Owen Roberts of Bodedern, Anglesea, obtained premiums for their superior answering in materia medica and chemistry.

Next week, a new edition of Denman's Practice of Midwifery will be published, with additional Modern Information on the various Subjects treated of in the work. To which will be appended a Dissertation on the Transfusion of Blood in the more dangerous varieties of Uterine Hæmorrhage. By Dr. Charles Waller.

Received. Dissertatio inauguralis medica de Sanguinis in Periculosa Hæmorrhagi Uterina Transfusione. Auctore Carolo Waller, M.D., &c. Erlangæ. 1832. pp. 29.

CORRESPONDENTS, &c.

Visitor. — *R. X.* — *Edinburghensis,* and others. Our pressing engagements render it impossible for us to comply with the numerous requests for personal interviews.

Castigator. JOE BURNS is not dead; "he only sleepeth."

M. P. It is impossible to answer the *first* question proposed, without a previous inspection of the indentures. With regard to the *second*, the paltry demand made by the not less paltry friends of the apprentice may be sustained, unless it can be *proved* that the youth was removed at *their* request, as well as in compliance with his own wishes.

Chirurgus draws an inference from insufficient grounds. It is no proof that a letter will not appear, because it is not immediately inserted. A communication may be intended by the Editor for publication during the current week, and yet the number not afford room for it. This it is usually very inconvenient to announce. Two or three weeks may elapse after a letter is in type, before arrangements which *must* be attended to will allow of its appearing.

Our Correspondents are requested to address all their letters to THE LANCET OFFICE.

The letter of a *Well-wisher, C.*, did not reach us until after the dinner had taken place.

The article in the Scotch newspaper, forwarded to us last week, is abundantly long, and yet we have failed to arrive at its precise purport. We certainly do not coincide in feeling with the writer, when he speaks of the raising of the infirmary salaries from 25*l.* to 40*l.* per annum, as an "indefensible" measure; nor can we admire the economy which would have saved the charity "fourteen or fifteen hundred pounds," by, amongst other things, wholly withholding remuneration from the medical officers! In all other matters the efforts of the "agitators" on the present occasion have our best wishes.

A Naval Surgeon. The diploma of the London College of Physicians is recognised by the medical department of the navy, but it is not usual for the "M.D." to be attached to the name in the navy list, unless the possessor be appointed to a physician's office.

"*An Eye Witness*" of the operation in the S——d G——l Infirmary is requested to send us his name and address confidentially.

1. The party so practising is liable to prosecution. 2. Yes; in any.

It is not customary with us to insert such articles from correspondents as the review of "Hints," &c.

A theory very similar to that of our ingenious correspondent *J. F.* has already been made public, and nothing will be gained by the insertion of his essay.

THE LANCET.

Vol. II.] LONDON, SATURDAY, MAY 19. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXX.

PHTHISIS PULMONALIS, OR CONSUMPTION, IN THE HORSE AND CATTLE.

Gentlemen,—It is my fate to have to treat to-night of another disease of our domesticated animals not yet admitted into our imperfect nosology, but which is occasionally observed in the horse, and destroys thousands of cattle; I mean phthisis pulmonalis—consumption or decline: usually the consequence of pneumonia or pleurisy, or both combined, but often running its course without any indication of acute inflammation of the thoracic viscera or membranes. I will suppose a case, fortunately not of frequent occurrence. I have a narrow-chested, flat-sided horse. It is attacked by pneumonia or severe catarrhal fever. My experience tells me, that I shall have a great deal more difficulty in subduing the disease in him, than in one deeper in the girth, or rounder in the barrel. The lungs, deficient in bulk to a degree proportionate to the diminished capacity of the chest, have been overworked in supplying the quantity of arterial blood usually expended in the various purposes of secretion, nutrition, and motion; and the animal has evidently been distressed and blown, when an increased current of blood has been required under unusual and violent exertion. The lungs have, therefore, been predisposed to take on inflammatory action; and that inflammation has acquired an intense character under circumstances by which another horse would be scarcely affected.

Symptoms of Consumption.—I will, however, suppose that I have apparently subdued the disease. Do I dismiss him so quickly and so summarily to his work as I should another horse? No, far from it. He has been much emaciated by the disease; he often continues so; his coat stares, and his skin clings to his ribs, and his belly remains tucked up in spite of all my mashes, and carrots, and green meat, and medicine; his life and spirit do not return to him; he is listless; or if he is full of spirit and willing to work, he is easily tired, sweating on the least exertion, and the sweat most profuse about the chest and sides; his appetite does not return, or perhaps it was never good, and the slightest extra-exertion puts him entirely off his feed. I observe him more attentively, and even as he stands quiet in his stall, the flanks heave a little more laboriously than usual, and that heaving is more decidedly quickened when he is in exercise. He coughs now and then—sorely,—and discharges from the nose a mucus tinged with blood, or a fluid decidedly purulent; the breath becomes a little strong, and, by-and-by, very offensive, while the pulse is always above forty, and strangely increased by the slightest exertion.

When many of these symptoms are developed, the animal will exhibit considerable pain on being tapped, or gently struck, on some part of the chest; and then, all at once, the cough will be more frequent and painful—the discharge from the nose more abundant and fœtid—the emaciation, and consequent debility, more rapid, until death closes the scene.

Post-mortem Appearances.—The lesions which are presented after death are very uncertain. Generally there are tubercles; sometimes very minute, at other times large; in different states of softening, some of them having burst into the bronchial passages, and exhibiting abscesses of enormous size. Other portions of the lungs are shrunk, flaccid, or indurated,—hepatised,—of a pale or a red-brown colour; and there are occasional adhesions between the pulmonary costal pleuræ.

Symptoms when not connected with Acute Inflammation.—At other times there is no previous disease deserving of notice; but there is cough more or less frequent, dry, or accompanied by nasal discharge; slight diminution of appetite, and that only apparent after unusual work; want of perfect condition; a degree of listlessness when not at work, but which immediately yields to ardour, scarcely perhaps to be restrained, when the animal is exercised; a little heaving of the flanks, and this only perceptible after exertion: and this will continue for months or even years, without any disease or apparent derangement that interferes with ordinary service and work. At times the horse will rally, the appetite will return, and the condition will improve; but this will last only a little while,—the cough will return from slight or unknown cause, and the listlessness, and want of appetite. Next will appear *hectic fever*, and the malady will run its fatal course.

This is a meagre account, Gentlemen, but it is the best I can give you. Our veterinary authors either mention it not, or bestow only a few lines on the consideration of it. Phthisis is exceedingly difficult to detect at its commencement; it may be confounded with almost every chronic affection of the chest; but in its later stage it is plain enough; and when its symptoms become too manifest to be mistaken, it bids defiance to all medical treatment.

Causes.—Is it *hereditary*? I have not sufficient experience to decide this point which has not yet been mooted among us. One thing only I know, that in the few cases in which I have seen it, the horse has a great deal of daylight under him, and the side has been flat, and the belly tucked up, and the animal has had a great deal more ardour and willingness than physical strength. These conformations, and this disposition, I know to be hereditary, and thus far phthisis may be said to be so too. Of the influence of *climate* I cannot speak. I have no experience on the point. I can satisfactorily trace phthisis to inflammation of the lungs, chronic or acute, and I know that this conformation of which I have been speaking, disposes the horse to inflammation of the chest of the intensest character. I can believe that low and damp situations, or a variable and ungenial climate, may render horses peculiarly susceptible of chest affections; and I do know that all the absurd, or cruel, or accidental causes of pneumonia, may lay the foundation for phthisis, and particularly I know that these causes which tend to debilitate the frame generally render the horse more liable to chest affections, and less able to ward off their fatal consequences. The most numerous instances of phthisis occur in those poor persecuted beasts that are worn out before their time and they are frequent enough among cavalry horses after the deprivations and fatigues of a long campaign.

Treatment.—I quit this unsatisfactory division of my subject for one yet more so, the medical treatment of confirmed phthisis. I have no case of successful treatment in my own experience in confirmed phthisis, and I cannot find one on record. You must be guided by circumstances. If the horse is not very bad, and it is the spring of the year, *a run at grass* will always be useful. Sometimes it will seem to renovate the animal; but the apparent amelioration is too often treacherous. It should always be tried, and the disease may be so far subdued, as more readily to yield to after treatment. The summer having set in, the medicinal effect of the grass ceases, and the effect of the grass ceases, and the flies tease and irritate the horse, and increase the too general irritability of the system. If the patient is taken partially or entirely from work, and submitted to your care, the course you have to pursue is plain enough. Consult the pulse and the membrane of the nose. Is the first quick and hard, and the second too much streaked with red? —*bleed*, but remember that you have not to do with an acute disease, or with a constitution the strength of which is not sapped—your chief dependence here will be on that mode of depletion which I reprobated when speaking of acute disease. The exacerbation of inflammation you must endeavour to subdue, but you must not exhaust a constitution already debilitated. Small bleedings of one or two quarts, cautiously regulated by the pulse and the nasal membrane, and omitted when the one is quieted and the other becomes pale, will be exceeding useful. *Counter-irritants* can never do harm, and these should be in the form of blisters, and not of rowels or setons, for you probably are combatting a disease or disorganisation of the whole or the greater part of the lung, and the counter-irritant should spread as much as possible over the affected part. *Sedative medicines* should be perseveringly administered, and here, as in acute inflammation, our chief dependence will be placed on *digitalis*. Its action on the circulatory system in the horse cannot for a moment be disputed. It should be given in doses of one or two scruples, until a slightly intermittent pulse is produced, and that state of the circulation should be maintained by minuter doses: as a diuretic *nitre* may be added, and the *pulvis antimonialis* as a diaphoretic.

Any *tonics* here? Yes, the tonic effect of mild and nutritious food—green meat of almost every kind—carrots particularly—mashes—now and then a malt-mash. Nothing further than this? You may try, but very cautiously, those tonics, which stimulate the digestive system, but comparatively little affect the circulatory one. Small doses of camomile and gentian may be given, but carefully watched and omitted, if the flanks should heave more, or the cough be aggravated. Iron is not admissible in any stage of chest affection, or at least I can truly say that I have rarely given it without repenting that I had done so.

Conclusion.—After all, the treatment of phthisis is a most unsatisfactory subject of consideration as it regards the practice of the veterinarian. If, after the human being has been subjected to medical treatment for a long course of time and at very considerable expense, he so far recovers that life is rendered tolerably comfortable to him, he and his connexions are thankful and satisfied, and he will submit to many a privation to ward off the return of a disease, to which he is conscious there will ever be a strong predisposition; but the case is different with the horse; this, the scope and bound of the human practitioner's hope, is worthless to the veterinarian. His patient must not only live, but must be *sound* again; every energy—every capability must be restored. Can we cause the tubercles of the lungs to be absorbed? Can we disperse or dispel their hepatization? Can we remodel their disorganised structure? Our consideration then will be chiefly directed to the detection of the disease in its earliest state—to the allaying of the irritation which causes or accompanies the growth of the tubercle—the mingled cause and effect—and thus arresting their progress, and by degrees causing them to become foreign, and in some measure harmless bodies, and which the absorbents will in due time remove. This must be the scope and bound of *our* practice—always remembering that the owner should be forewarned of the general hopelessness of the case, and that the continuance of our efforts should be regulated by the wish of the proprietor and the value of the patient.

CONSUMPTION IN COWS.

I now turn to a subject sufficiently unsatisfactory, but connected with which there are some redeeming considerations, arising from the easier and earlier detection of the disease; the removal of some of its exciting causes; and, what is most important of all, the necessity not existing that the animal should be restored to the possession of every pristine energy in order to render her useful and valuable.

Symptoms.—The first symptom is that of which the farmer and the herdsman are so regardless, *cough*. One would think, that dearly-bought experience would convince these people that there are few things more to be dreaded than *hoose* in cattle. I have treated of this in a former lecture. The cough is at first sonorous and clear, and while it continues thus no great harm is done; while the cough is loud and clear the lung is sound enough. But the inflammation, chronic or acute, that produces this cough, is that harmless? It is silently, if not rapidly disorganising the lungs, and the change of the cough will tell the extent of the disorganization. That farmer is wisest who attacks the cough while it is loud and clear; and he next deserves the character of a prudent man, who recognises the first symptom of disorganization; and that is as plain to him who is accustomed to cattle as is the variety of cough in the horse indicating the state of the lungs in that animal. The cough of incipient phthisis is *inward, feeble, painful, hoarse, rattling, gurgling,* and that probably for a considerable period before there begins to be any discharge from the nose or mouth.

Gentlemen, I would urge you to study this peculiar cough of incipient consumption in cattle. There is something almost as peculiar about it as there is in the howl of the rabid dog. There is little or nothing of spasm. It is a painful and not violent pressing out of the air through passages half obstructed. Some good may be done now. The lungs of the cow have not been so sadly used as those of the horse, nor is the process of disorganization so rapid. The golden time is, however, suffered to pass by, because the animal does not seem to lose much flesh, and gives milk as abundantly as before. She was designed to accumulate flesh, and to give milk; and she will continue so to do, and the appetite, by which alone the means of doing this can be accomplished, will continue unimpaired long after local disorganization and mischiet have commenced. Besides, she is not hurried beyond her will and her strength by the caprice or cruelty of her owner, and thus the progress of the mischief unnecessarily and rapidly increased. Weeks and months, shall I say years, will pass on, and there will be nothing to indicate the formation of tubercles in the lungs, but the inward, feeble, painful, hoarse, gurgling, cough. Study, it, gentlemen, carefully. It will advise you of the existence of the mischief, while, perhaps, it is remediable; and the accuracy of your prognosis, even although the farmer may not at the moment listen to you, will afterwards materially add to your reputation.

Treatment of early Stage.—You have two ways of saving the animal here. Your bleeding and digitalis may quiet the irritation of the lungs, or at least prevent an exacerbation of the disease; for although in some cases the next steps are, a staring coat, hide-bound, loss of flesh, and diminution of milk, yet much oftener the progress of the disease is hastened by carelessness or accident. The cow is exposed too soon after calving, or at other times unnecessarily exposed to the vicissitudes of the weather, while she is neglected and half-starved, and she *gets cold;* and the lungs, already irritatable by disease and disorganization, take on inflammation; and that becomes highly intense, and either kills the animal at once or increases and hastens the change of structure in which phthisis consists.

Deceptive Nature of the Disease.—Here let me press on your minds a lesson of caution. You attack this new inflammation promptly and skilfully, and you apparently subdue it. Appetite, condition, a plentiful secretion of milk, return. *Is the cough gone?* the painful, inward, feeble gurgling cough? If that remains there is mischief still, — irretrievable mischief. All you have done stands for nothing, unless you follow up your blow, and drive away the cough as well as the inflammation.

Slow Progress of the Disease.—If you are satisfied with what you have done, or the penurious and illiberal proprietor forbids you to proceed, weeks and months may again pass, and no cause for alarm may appear, because, as I have already said, the lungs of the cow, although irritable enough from the disease, will not be rendered more so by ill-timed or cruel exertion, and the acute diseases of the lungs here are more those of the investing membrane than of the substance; they partake more of pleurisy than of pneumonia. Another attack of acute inflammation may follow, and that again be subdued, until at length symptoms, of which no man can mistake, irretrievable mischief begin to appear. She wastes, more or less rapidly; her hide sticks to her bones; her milk gradually diminishes she becomes evidently weak; yet still she eats, *eats with almost undiminished appetite:* but the process of rumination, requiring long, and now fatiguing action of the jaw, is more slowly performed. Then appears that which accompanies almost every disease of the ox, inflammation of the subcutaneous tissue. On whatever part of the animal you press, she shrinks; and if you press upon the loins, where this circumstance is always most evident, she will not only shrink under your hand, but probably moan with intensity of pain. The cough now becomes even more painful and frequent, and is accompanied by discharge from the mouth or nostrils, of a purulent, or bloody, or fetid character. The offensive smell increases to an almost insupportable degree as the termination of the disease approaches.

Later Symptoms.—When this stage of the disease commences, or through the whole of its progress, diarrhœa often prevails, and that to a degree which the most powerful astringents cannot arrest, and the fæces are, like the other discharges, indicative of the putrid form which the disease is now exhibiting. The end is even not yet, but the animal clings to life with singular tenacity. Hectic fever is now established, distinguished by every symptom except profuse sweating. There is no cutaneous perspiration even now. The skin clings dry and scaly, *it strangely creaks* as the cow crawls staggering along. The discharge from the mouth and nostrils increases and becomes even more fetid, the animal weaker, and, if possible, thinner; and yet she bears up, and eats, and slowly ruminates, and then when the vital power is quite exhausted, and even with the food in her mouth she falls and dies. One circumstance is very remarkable and characteristic. The mind and animal desires even of this comparatively dull and insensible being are roused to an extreme degree of intensity during the progress of this disease. The cow is frequently *almost continually at heat.* When she is impregnated the œstrum goes not off, and the consequence of this continuance of excitement is, that she is exceedingly subject to abortion.

Long before the farmer suspects it, there is a deterioration in the character of the milk, even although the quantity should not be materially diminished. It has an unpleasant taste—it quickly becomes sour—it spoils that with which it is mixed. The butter made from it is ill-flavoured; the cheese will not acquire the proper consistence. Some have said that the milk is of a bluer colour, and that it has abundantly more serum in its composition than ordinary and healthy milk. Some of the puzzling disappointments of the dairyman may be accounted for by the deterioration of the milk from a cow labouring under phthisis.

Post-mortem Appearances.—The first remarkable difference between the lesions presented in the lungs of the ox and the horse after death is the more frequent and complete adhesions, farther illustrative of the fact that in the ox inflammations of the chest are in the great majority of cases those of the pleuræ, and in the horse those of the substance of the lungs. The substance of the lungs, however, is soon affected in the ox, as is the investing membrane in the horse; for there is nearly the same tubercular appearance, but generally less induration or hepatization; if there is any difference, it is that the tubercles in their state of hardness are more concrete than in the horse, and sometimes can scarcely be cut through. The tubercles here, however, are not confined to the substance of the lungs, but they are scattered over the pleura, both costal and diaphragmatic, and are even found within the duplicatures of the mediastinum. This is still farther illustrative of the difference in the origin of the disease in the two animals. Hydatids are also usually found about the lining membrane of the chest, or numerously and deeply embedded in the substance of the lungs.

Causes. Hereditary Predisposition. — Of one and a very frequent cause of consumption in the cow I can speak with confidence, for I have had opportunities of observing and tracing it, I mean hereditary predisposition. I have rarely known the offspring of a consumptive cow that was not also consumptive. I recollect two dairies that were almost destroyed by this hereditary taint. If it is a heifer-calf, she may possibly live a little after her first calving, but then she usually sickens, and the disease proceeds with a rapidity unknown in the mother. This will be a very powerful reason for our having as little to do with these cases as we can, lest we entail a kind of pest upon our employers. The cow that exhibits the slightest symptom of consumption should be sent to the butcher immediately if she is in tolerable condition, or got into condition as quickly as possible and sold.

Epidemic.—It is often epidemic; or, although the cow may have previously had the seeds of the disease within her, there are certain seasons when they are more rapidly and fatally developed. This is a fact well known to dairymen. While it is especially prevalent in low, moist, woody pastures, it is, even there, of more frequent occurrence, and more fatal in spring and autumn, and when the weather is more than usually cold and changeable.

Change of Climate.—There is a vast deal, likewise, in the cow being suited to the climate, or rather being in her native climate, or one that closely resembles it. This explains the strange difference of opinion with regard to breeds. Every farmer is partial to his own breed, and undervalues those of other districts; and to a very great degree he is right. His cattle breathe their native air; they are in the climate to which, by a slow and sanative process, and extending for many a generation back, their constitution has been in a manner moulded; and it is only after a long seasoning, and a dangerous one too, that the stranger becomes adapted to the temperature and degree of dryness or moisture, and more particularly the soil and difference of herbage in which she is placed. There is more in this than is often dreamed of in the farmer's philosophy.

While there is derangement in every system, the respiratory one seems to suffer most, and a slow, insidious, yet fatal change is there oftenest effected. If a dairy is removed from a moist situation to a dry and colder one, consumption will often appear among the cows, although a dry air is otherwise esteemed a specific against the complaint; but if a dairy of cows are removed from a dry situation to a woody and damp one, phthisis is sure to break out before the first season is past. There is no animal with so much difficulty naturalized in a foreign soil as the cow. I knew a lady who long tried in vain to naturalize the Durham cow in one of the northern counties of Scotland, and she could at last only effect her purpose by admitting one cross from the native breed. They all died of hoose or phthisis.

Mismanagement.—I will not repeat what I have said of the causes of hoose, pneumonia, and pleurisy in cattle. All these diseases will degenerate into phthisis; and the causes which produce them will often produce, or lay the foundation for, consumption, without the intervention of these diseases. Consumption, like these maladies, is principally to be traced to our absurd management of our domesticated quadrupeds, and equally to the strange extremes into which we fall of over-nursing or cruel abandonment.

Swiss Cattle.—Hurtrel d'Arboval, to whom we are indebted for one of the best accounts of this disease that I am acquainted with, and from whom I have derived some useful hints, states a curious fact, that the Swiss cows are not subject to phthisis, although they pass the spring and summer on the mountains, unsheltered, breathing the coolest and purest air; and in the autumn and winter are shut up in close and hot stables, where not a breath of pure air can reach them, except when they are driven daily and far through the snow to water. They, however, who have no upland pasture to which their cattle can be removed, and whose beasts rarely escape from the miserable huts in which they are confined and fed, lose many a cow from phthisis. Habit, and a constitution gradually formed by the influences of these changes on many a generation, had prepared the first for them, or had even rendered them in a manner necessary; but habit could not secure the others from the deleterious effect of empoisoned air, and unwholesome or insufficient food.

Want of Ventilation.—There is one striking fact showing the injurious effect of heated and empoisoned air on the pulmonary system. There are some cow-houses in which the heat is intense, and the animals are in one constant state of perspiration, The door and the windows must however be sometimes opened, and then the air blows in cold enough upon those who are close to them, and one would think could not fail of being injurious. No such thing; these are the animals that escape; but the others at the farther end, on whom no wind blows, and where no perspiration is checked, are the first to have hoose, inflammation of the lungs, and phthisis.

Treatment.—On the treatment of phthisis in cattle I have little to say, or rather it is comprehended in what I have said of this disease in the horse, with some slight difference, principally confined to the quantity of medicine. Here even more than in the horse appears the necessity of early treatment, and we have here fairer and better scope. We have the slower progress of disorganization, and we have the peculiarity of cough, pointing out the very commencement of it: but then, on the other hand, if we neglect our duty to our employers, or are not permitted to act in time, we have the increased difficulty, or absolute impossibility, of removing a disorganization so slowly formed, and becoming as it were an integrant and inseparable part of the animal. The same medicines, but in smaller doses, afford the best chance of success; but they must be long and perseveringly administered—much more so than in the horse, for the process of morbid growth was slower. Your attention, however, will be most of all directed to the removal of the cause of the disease whatever it may have been. If that which has a tendency to prolong and to aggravate it is taken away, you may have a little better chance; but the original cause, or the deleterious adjuvants continuing to act, you have no chance at all. I cannot point out what steps you must take—you must make yourselves masters of the history of the case, and be guided by circumstances. Let the overfilled cow-houses be enlarged—the close and hot ones better ventilated—let cruel neglect, and exposure, and starvation, yield to more humane and judicious treatment—when cattle are fed on dry meat, let them have sufficient to drink two or three times every day—let those that exhibit decided symptoms of phthisis be removed from the dairy, not that the disease is contagious, but that it is undeniably hereditary—in fine, in a case where generally so little can be done in the way of cure, let nothing be omitted in the way of prevention.

ON THE
AUSCULTATION AND TREATMENT
OF
AFFECTIONS OF THE HEART.

From an Essay read at the late Anniversary Meeting of the Hunterian Society.

By Dr. Billing,

Physician to the London Hospital.

Causes of the Beat and Sounds of the Heart.

Upon applying the ear, or stethoscope, to the chest of a person in health at that point where the heart may usually be seen and felt to pulsate—that is, between the cartilages of the fifth and sixth ribs on the left side, you feel a beat, accompanied by a sound, as if the sound was produced by the blow against the ribs, and immediately after a sound, rather shorter and weaker, appearing more distant, as if produced by the falling back of the body which gave the blow; there is then a pause, rather longer than the time occupied between the first and second sounds. I have elsewhere stated that the beat, or push of the heart, was between the first and second sound, which will be found to be the case, for though the first sound and blow come together, just as when a book or other hard substance is laid against the head, and struck by the hand, yet the push lasts some time,—nearly until the second occurs. Now these phenomena are caused by the ventricles and the valves, for, contrary to the opinion of the immortal Laennec, the auricles have nothing to do with the production of the sounds; the push is caused by the swelling up of the ventricular muscles, in their systole to expel the blood; the first sound is caused by the tension produced in the shutting of the auriculo-ventricular valves, and the second sound is caused by the tension produced in the shutting of the ventriculo-arterial valves. The cause here assigned might be thought inadequate to the production of the sound heard, but the little instrument used by gamekeepers to call partridges, may be heard at least at the distance of a quarter of a mile, though consisting only of a bit of bladder stretched over a thimble, a membranous expansion, which is five or six times less than the valves which act together in the heart, and would give a much louder sound if not surrounded by soft parts. The first sound is loudest and longest; the second sudden, and terminates instantaneously, because the semilunar valves have a regular, defined, attachment, and are smaller than the auriculo-ventricular valves, the attachment of which to the cordæ tendineæ, and their irregular shapes, render the termination of the sound less abrupt. This is a simple, unsophisticated, explanation of the causes of the beat and sounds of the heart, and you will find that the morbid signs are all explicable as alterations of these.

Hypertrophy, with and without Dilatation, and the resulting Bruits.

First, simple hypertrophy of the ventricles produces diminution of the sounds; simple hypertrophy signifying, that their thickness is increased without any dilatation, consequently encroaching on the cavities, by which the auriculo-ventricular valves have less play, and there is less contained blood to act upon them to produce the first sound. The second sound is diminished likewise, on account of the diminished quantity of blood, and there is, besides, less space for the action of the ventriculo-arterial valves. In each case the sound is diminished by the diminution of tension. Now when the hypertrophy is not simple, but the cavity enlarges at the same time, producing enlargement of the heart, that is hypertrophy with dilatation, it is well ascertained that the sounds remain nearly natural, the space being free, until at length, if the cavity become enormously enlarged, the columnæ and cordæ, from the distance of the parietes, draw the auriculo-ventricular valves, so as to prevent their shutting, and then the sound is lost in that ventricle, and instead of it there is heard a "bruit," caused by the regurgitation of the blood into the auricle, and if this be on the right side of the heart, you may at the same time have a pulse in the jugular vein; for though the right auricle, under ordinary circumstances, is not able to distend the veins, so as to render their valves useless, yet, when the tricuspid valve is imperfect, either permanently, or only temporarily, so, under extra action, the force of the ventricle causes a reflux, visible in the jugular vein, and then the passage being once forced, the auricle also is able to produce a pulsation in the vein; so that you have this jugular pulsation twice for each pulse at the wrist, once for the reflux from the auricle, and once for the regurgitation from the ventricle, as in the case of the old woman alluded to in Spittal (pages 87 and 105).

In this case he appears to think that the auricle pulsated twice for each pulsation of the ventricle, but if the old woman ever dies, it will be found that the auriculo-ventricular valve was rendered imperfect on excitement, either by dilatation of the ventricle, or perhaps by some alteration of its own structure, as there was bruit de soufflet between the sixth and seventh ribs, or both causes combined; and I am pretty certain, that the pulse being exactly doubled by exertion, depended on the aortic valves being ossified, or otherwise imperfect to a certain extent, so that the ventricle not being empty on account of regurgitation, the auricular systole, as well as the ventricular, caused a pulse at the wrist. This I have several times found to be the case, when the aortic valves were not capable of closing, on account of disease; and in a certain stage of the disease, the valves are just in such a state as to prevent regurgitation under gentle action of the heart, but when excitation takes place, they either do not close in time, from rigidity, or give way by retroversion of their edges.

Now when the ventricle is moderately dilated, without being hypertrophied, we find that the sound of the auriculo-ventricular valves is increased, for they are more on the stretch, there being a larger space; and, besides, when there is dilatation, there is a concomitant laxity of the carneæ columnæ, allowing the valves to flap to more suddenly, and the sound becomes short, like that of the semilunar valves. The valves not having been hitherto considered as the cause of the natural sounds, the influence of the carneæ columnæ in modifying them has not usually been estimated, though this will account for one of the unnatural sounds which takes place in the perfectly formed heart, when under the nervous excitation called palpitation, as in hysteria, &c., in which cases the carneæ columnæ, partaking of the convulsive action of the ventricle, keep the auriculo-ventricular valves open, and the consequence is, a regurgitation at each systole, which causes the sound called bruit de soufflet, instead of obscuring the natural valvular sound, and causing distress in the breathing, on account of the regurgitation on the lungs, and a feeling of syncope, on account of there not being a sufficiency of blood sent to the brain, which effect is also dependent upon the too rapid action not allowing time for the ventricle to fill. I have seen a person faint from this cause after running very quickly, though his heart and lungs were sound. This effect is produced even more strongly, when there is hypertrophy and dilatation of the ventricle, in which case, upon the excitation of exercise, &c., the distress of the lungs and bruit de soufflet are augmented, and besides, when the enlargement is great, the disproportion of the ventricular cavity to the aorta causes a ripple and bruit.

Percussion is not available for diagnosis, until enlargement is far advanced, and then there is more or less dulness of sound, as also when there exists much hydrops pericardii, in which case, in addition to the dulness on percussion, there is a joggling sensation when the cheek is applied for the purpose of immediate auscultation.

Bruits from various other Causes.

We thus find that various alterations of the sounds of the valves, dependent on other circumstances, take place, though they remain perfect in their structure, and we have now to consider the *bruit* which occurs in consequence of alteration of the valves themselves, and must recollect, that there may be natural sound of the right side, whilst the left has an unnatural bruit, and *vice versa.* If there be bruit accompanying or masking the first sound, it may be either from the auriculo-ventricular valve being incapable of closing, from ossification, &c., or from rigidity and irregularity of the semilunar valves causing a ripple, though they be still capable of closing; if the fault be in the auriculo-ventricular, the bruit will be heard most in the præcordial region; if in the semilunar valves, in the anterior superior.

If there be bruit accompanying or masking the second sound, it may be from regurgitation from the semilunar valves, in consequence of ossification preventing their closing; or of vegetations which make their edges turn back, so as not to hold the blood; or from rupture from various causes; or from aneurismal dilatation of the aorta, so great, that the valve cannot fill up the opening. If ossification be the cause, it is most likely there will be a bruit accompanying or masking both sounds, the first bruit from roughness, the second from regurgitation. There may be also a bruit rather after, than with, the second sound; and in this case the first sound will be generally weak, as this happens when the auriculo-ventricular opening is so contracted, that a ripple is caused by the filling of the ventricle from the auricle.

There may be bruit accompanying both sounds, from the co-existence of two of the foregoing causes, in which case these causes may be discoverable by the positions in which they are heard, as already stated; or there may be bruit accompanying or masking both sounds. from permanent rigidity and contraction of one opening only, either auriculo-ventricular, or ventriculo-arterial; the particular opening being discoverable according as the bruit is heard most above or below, right or left, besides which, almost always there will be coexistent hypertrophy or dilatation, which must be taken into consideration.

Causes of Hypertrophy and Dilatation.

Hypertrophy arises in various ways,—1st. The simplest, from mere over-action, as the muscles of any part enlarge by extra use, so that mere continued nervous palpitation will induce a degree of hypertrophy; 2dly, Over action, with inflammation, such as the excited pulse, with metastasis of rheumatic inflammation to the pericardium and fibrous tissue of the heart, or from common cold, pericarditis arising alone, or in conjunction with pleurisy. In both cases very frequently lymph is thrown out, and adhesions are formed, productive of most serious consequences, and hitherto most commonly fatal, though now more amenable to remedies from being better understood. Of course, when there is the inflammatory congested state of vessels, or hyperæmia of inflammation, the enlargement will go on much more rapidly than from mere over-exercise. Though the treatment of pleurisy and of pericarditis in the recent acute stage must be so similar, that diagnosis appears of little consequence at that time, yet it is very necessary on account of the after treatment, for a patient who has had pericarditis, must be kept much longer quiet, to avoid the risk of hypertrophy. From what experience I have had of these cases hitherto, I have formed the opinion, that the pulse is a great assistant in the diagnosis; in pleurisy it is hard and tense, whereas in pericarditis it is looser, more throbbing or jerking, more approaching to that of acute rheumatism, as it is also in cases when inflammation is communicated to the heart along the vessels, either after operations, or wounds received in dissecting, &c. 3dly, Hypertrophy arises from imperfection of valves, and, as just remarked, this will be more or less rapid in proportion to the superaddition or absence of inflammation. Thus ossification of valves, by causing obstruction, calls for over-action; also when ossification prevents valves from shutting, the cavity behind the valve is constantly overloaded by regurgitation, and must be over-worked. Now if this non-closure be the consequence of a rupture of the valve from some strain or violent exertion, there will most likely be an inflammatory action set up by the injury, and these are often serious cases and rapidly fatal. The cases, too, connected with vegetations of the valves causing their edges to turn over, are more or less rapid as the accompanying inflammation is acute or chronic. There may be, also, congenital faults of the valves, either as to contraction of the openings or deficiency of shutting.

Dilatation arises from similar causes to hypertrophy, the dilatation taking place in proportion to the debility of constitution, or the degree of inflammation, which latter, when acute, always softens the structures in which it exists; when there is only a little inflammation, so as to produce hyperæmia without much softening, hypertrophy and dilatation go on together; on the contrary, where much debility exists, dilatation almost alone.

The force of the beat, or impulsion of the heart, is a measure of hypertrophy; thus, in the natural state, the heart strikes the end of the stethoscope or the side of the head, but has not power to raise it, but, according to the degree of hypertrophy, pushes the head away from the chest, the word "impulsion" not being used to express the beat until it has a morbid force. Dilatation, on the contrary, gives a broader slap, but weaker. Combining the two, then, when there is proportioned enlargement, viz. hypertrophy with dilatation, the beat is felt, according to the hypertrophy, stronger; according to the dilatation, over a greater extent than natural; and it must be remembered in calculating the hypertrophy, to make allowance for debility from disease; or if the patient have been taking sedative or contra-stimulant medicines, such as antimony, digitalis, &c., which mask the hypertrophy.

Aneurism in the Chest.

With respect to the diagnosis of aneurism in the chest, I do not think I can add any remark, except that the signs are of rather a negative quality, its existence being inferred by the expert auscultator, rather when he finds symptoms which do not belong exclusively to affections of the heart alone. I have to make one observation in defence of Laennec, which is, that he is right in saying there is but single impulsion in simple aneurism of the aorta, for this is the fact, unless the aortic valves be imperfect, in which case the contraction of the auricle may be felt, as before described, and there is really double pulsation, the weakest pulse from the auricle coming first; but that deceptive jarring shock, or back stroke, given sometimes to the aneurismal tumour upon the diastole of the ventricle, follows the impulsion at the moment that the auricle and ventricle are both quiescent.

Treatment.—Rest—Depletion—Tonics—Mercury—Digitalis—Opium—Elaterium.

I cannot be expected in this brief space of time to enter into minute details of practice, but I must give a general outline of what I have proved to be beneficial, in the numerous cases which occur in a large hospital. In the first place it is hardly necessary to state, that absolute rest is indispensable, the only inconvenience of which is, the loss of tone to the system, and injury to the digestive organs which so often ensues, and which must be guarded against by regular attention to the stomach and bowels, without active purging, except whilst there is acute inflammation going on. Depletion is frequently necessary, but all depletion has a tendency to increase nervous excitability, which is in itself a cause of hypertrophy. Depletion must be used with judgment and circumspection, and a free use of tartar emetic will be found a most powerful means of keeping down inflammation, with less expenditure of blood. In order to guard against nervous palpitation, and to preserve the tone of the system under a want of exercise, tonics are necessary in every stage, and this I know to be a safe and useful practice, notwithstanding the common opinion that tonics are heating, which I can fully disprove, though the patient will be heated of course, if whilst inflammation exists the bowels be neglected, or the abstraction of blood, and other antiphlogistic means be omitted. Mercury, though so indiscriminately used, requires caution, as the hardness of the pulse which it sometimes produces, might overbalance the known beneficial effect of it on the capillary system. I find the greatest benefit from the use of digitalis, not in such doses as to oppress the circulation, or stomach, or head, but just enough to make the heart a little lazy, by which the tendency to hypertrophy is combated, and I am satisfied by cases which there is not time now to relate, that the size of the heart may be actually reduced. I scarcely ever use crude opium, or the tincture, in these cases, as being stimulant, but sometimes the liq. opii sedat. as an anodyne and hypnotic, which is at the same time sedative instead of stimulant. In advanced cases when dropsy has come on, and the heart has become languid, as evinced by the irregularity of the pulse, I do not recommend digitalis, well knowing that sudden death occurs in that stage sometimes from syncope, but in these cases elaterium is an invaluable mode of relief, and instead of producing exhaustion, by the great quantity of liquid which it brings away, it takes off the oppression by evacuating the redundant serum, there is no acrimony or irritating property exerted by this medicine, whilst there is fluid to come away; the action of it is specific and peculiar, and produces no more injury by the weeping which it causes from the exhalant capillaries of the intestines, than an onion does when it produces a flow of tears from the eyes, but to do good it must be repeated frequently, and when the patient feels how much benefit he derives from it, in comparison of any inconvenience from nausea, &c. he will willingly persevere in its use.

ESSAYS
ON
ANIMAL ACTION.

By WILLIAM DOBSON, *Esq., Surgeon.*

No. IV.

ON THE DIAPHRAGM.

THE diaphragm is a muscular septum between the chest and the abdomen. It consists of two muscles which arise from the circumference of the base of the thorax, and radiate to a central point,—the cordiform tendon,—leaving perforations in their course, by the separation and decussation of their fibres, for the passage of vessels, the foramina being surrounded by tendinous structures. The vascularity of the diaphragm is high, as is evidenced by its deepred colour. The nerves which supply this organ are the *dorsal* and the *phrenics*. The dorsal nerves are a few branches, given off by the 10th, 11th, and 12th dorsal trunks, which take their origins by two roots, one from the anterior, the other from the posterior, column of the spinal marrow. There can therefore be no difficulty in arriving at the conclusion as to the actual influences of the dorsal nerves on the diaphragm. They will endow that organ with common sensation, and bring it under the influence of volition. By means of these nerves we are made acquainted with any physical aberration in the diaphragm, and they are, in part, the media by which we can retard or accelerate its operations at pleasure. By their agency the action of the diaphragm can be preserved for a short period after both phrenics are divided; but the duration of their effectiveness is very limited.

The physiology of the phrenic nerves is exceedingly imperfect; and we are divested of one important and very essential mean in the inquiry,—that is, strict and positive analogy. Yet demonstration is available.

Let us first investigate the anatomy of these nerves. They are described as arising from the 3rd, 4th, and 5th cervical nerves; and Sir C. Bell has endeavoured to establish that the branches thus derived, are continued onwards to a tract of nervous matter on the side of the spinal marrow, situated between the corpus olivare and corpus restiforme, from which other nerves connected with the respiratory apparatus originate. That this *may* be the case no one will deny, but that it is *not* the case, has been much questioned. I have endeavoured to determine the point, but unsuccessfully; and if the origin of nerves be the *only* basis on which we are to erect theories of function, we shall often be bewildered in our own perplexities. If this be taken as the only foundation, and without the support of correlative evidence, we need not be surprised at the existence of those ephemeral doctrines which are taught but to please.

I am induced to believe that the conjunction of the phrenic nerves with the cervical, exists for the purpose of establishing that kind of communication between the voluntary and involuntary organs which is observed with the sympathetics; and also that the physical connexions of the right phrenic, with the corresponding semilunar ganglion of the sympathetic and the hepatic plexus, are destined to allow of a reciprocal functional influence. So that when the mind is disturbed, either by physical or by moral causes, the organic operations necessarily participate, and *vice versa*. Certain facts might be adduced corroborative of this position, but they will be more appropriately introduced hereafter. The office of the diaphragm is obviously to enlarge the thoracic cavity in a vertical direction, and this it accomplishes efficiently.

During the quiescent state of the diaphragm, we notice that it is concave towards the abdomen, and convex towards the chest; and it is observed that the first change which this organ undergoes, is the descent of the *centres* of the lateral portions, until it has assumed a plane surface. But immediately the organ has arrived at its acme of descension, it returns to its situation in the chest. While the lateral portions of the diaphragm are undergoing this vibratory motion, the cordiform tendon is nearly stationary, for it is so intimately attached by its superior surface to the pericardial bag, as to prohibit any mobility. If the hand be applied to the muscular portion of the diaphragm, when it is descending into the abdomen, in inspiration, it is found to be firm and rigid, and exceedingly resistible; but in its ascent those qualities are lost. In inspiration, the base of the chest is protruded outwards, and as we developed in the preceding essay the total inefficiency of the intercostal muscles to produce this phenomena under ordinary circumstances, we are led to search for the cause elsewhere. It appears to me not improbable, that the expansion of the base of the chest in regular breathing is effected by the attempt of the rigid fibres of the diaphragm to arrive at a plain surface, and that when these fibres become relaxed, as during expiration, the chest recovers its natural state. Full expansion of the chest is observable only in early life. In old age, as we have previously noticed, the osseous boundaries become so consolidated, as almost to preclude this effect. Only the lower part of the chest is moved.

We have adverted to the descension of the diaphragm into the abdominal cavity, and its ascension into the chest, but we have a problem to solve, more difficult than a description of mechanical movements,—a problem beyond the ken of human perception. The eye cannot see, nor can the finger feel, nor can reason demonstrate, that inscrutable agent which occasions the motions of the diaphragm. We may as well search for the immediate cause of the power which propels the earth in its orbit, which directs the varied motions of the planetary bodies, and for the agent which connects universal nature together. He who has reflected on the subject of animal action must confess, that the *agent* which impels the diaphragm to constant motion, is concealed from his senses, and that human research, however extensive, will never discover its nature.

In the continuation of this subject, we shall inquire whether respiration be a voluntary or an involuntary operation, and endeavour to reconcile the conflicting opinions which prevail.

WRITINGS
AND PROFESSIONAL ACQUIREMENTS OF
DR. HOME, OF EDINBURGH.

To the Editor of THE LANCET.

SIR,—On perusing your LANCET of this date, I noticed the following passage, in a letter addressed to you by Dr. Epps, of Berners-street. "Dr. Home of Edinburgh, who, though not esteemed an authority in matters relating to the practice of physic, is a very good authority in reference to materia medica, states that he has found benefits from the use of this medicine in nervous affections."

Sir, I had the advantage of studying the profession under that able man, and had better opportunities of knowing his merits, and the weight due to his opinions in practice, than, probably, Dr. Epps ever had. I have long ago come to conclusions very different from those of Dr. Epps on this point, and know no physician in ancient or modern times, whose opinions may be more confidently relied upon as founded in close and accurate observation of the symptoms of disease, by a mind never influenced by previous theories, and far above the trick and quackery of writing for notoriety and not for the sake of truth.

I should ill, therefore, repay the benefits that I have derived from the writings and instructions of Dr. Home, if I allowed an opinion so unjust and unfounded as that of Dr. Epps to pass unnoticed.

But, fortunately, Sir, the materials to decide this question are abundant, and easily accessible. Let me be permitted to point out to Dr. Epps where they exist. In the first place, Dr. Home was one of the very few physicians who in modern times has had the honour of pointing out to the medical world the existence of a new, a distinct, a dangerous, and common form of disease, which *up to his time* had escaped the observations of *all* other medical men, although it must have existed since the days of Adam. That disease was the CROUP, of which he is the undoubted discoverer, and first describer. Then there are certain works of Dr. Home relating to the practice of physic to be met with in every medical library, with the exception I presume of Dr. Epps's. There is a book called *Medical Facts*, there is another called *Clinical Histories and Experiments*, both of them containing information on practice in disease, of the most important and instructive nature. Then there is another book, even better known than the above, called *Principia Medicina*, not only containing all the information known at the time when it was published, on the subject of practice, but written in more choice and classical Latin than is to be found in *any* medical work since the days of Celsus up to the present time. I suppose that Dr. Epps cannot know that this work is used in many of the foreign universities as the text-book for the practice of physic, and that it will continue as a standard work in the art, long after the names of Dr. Epps and the individual who has the honour of now addressing you are forgotten. When I add, that in addition to the above, Dr. Home's *Analysis of certain mineral waters*, was most ably conducted, considering the state of chemical knowledge at that time—that his *Treatise on bleaching* caused an entire revolution in that art in Scotland, and that his *Treatise on agriculture and its principles*, even now, is a standard work in that department, you cannot feel surprised at my appealing from the judgment of Dr. Epps, to facts, and the opinion of the profession in general.

I cannot conclude without calling your attention to Dr. Epps's paragraphs one of which appears to me not a little inconsistent and ridiculous. Dr. Epps begins the paragraph by appealing from the authority of Dr. Home on point of practice, and finishes by quoting him as an authority on —what do you think, Mr. Editor,—why, on a point of practice. Dr. Epps will hardly maintain seriously, I presume, that the knowledge of the value of certain means of removing disease, is not a point of practice? His own letter is a letter on materia medica, but Dr. Home's opinion, which he quotes, is an opinion upon a point of practice. I have the honour to remain, Sir, your obedient servant, M. D.

May 12th, 1832.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

BY
JOHN ELLIOTSON, M.D., F.R.S.,
PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE
IN THE UNIVERSITY OF LONDON,

Delivered March 19th, 1832.

DISEASES AND SOUNDS OF THE HEART—DR. HOPKINS' CASE.

I WAS speaking, Gentlemen, the other day, of the peculiar sound heard when an obstacle exists at certain openings of the heart, and I mentioned that it was the opinion of Laennec that the ventricles empty themselves first, and that immediately afterwards the auricles empty themselves.

Now Laennec supposes this to arise from a contraction of the auricles as well as of the ventricles, and I mentioned that this statement was at present called in question, for it was said that Haller and others ascertained that the auricles may be observed to contract before the ventricles, and that recent observation has confirmed their statements. But I mentioned that however this might be, it appeared to stand to reason that the moment the ventricles dilate, the blood must rush into their vacuum. Whether that is owing to a *contraction* at all of the auricles or not, is another question; but it does appear that when the left ventricle after contraction is dilated again, the blood must instantly rush in from the auricle. I believe I mentioned that the muscular part of the auricles is small, and forms a sort of appendage only, and therefore I believe that the proper auricles do not influence much the course of the blood—that they merely empty themselves from time to time, and that the great sinus of the auricle is emptied by the vacuum formed by the ventricle. I allow that I have seen the auricle contract before the ventricle. But I have seen it contract after the ventricle, and I have seen it contract several times to one contraction of the ventricle, so that there appeared in the experiment I witnessed very little connexion between the contraction of the auricles and the ventricles, and I am convinced that the auricles are emptied by the vacuum formed when the ventricles are dilated. I have always been able to make an accurate diagnosis by following the opinion of Laennec—not as to whether the auricles empty themselves by contraction or not, but as to the time at which this takes place—as to the time at which he says the blood quits the auricles and enters the ventricles.

There was in THE LANCET of last week (March 17th) an account of the post-mortem examination of Dr. Hopkins, abroad, who was formerly a lecturer at Mr. Grainger's. This gentleman is said to have exhibited, on inspection after death, the following appearances:—"The left auriculo-ventricular opening of the heart,"—that is, the opening between the left ventricle and the left auricle, where the mitral valve is situated—"was ossified in its whole circumference for about three lines in breadth." I presume that means that there was a ring of bone around the commencement or base of the mitral valve, three lines in breadth. "This morbid deviation which had its seat in the internal membrane, extended into the tricuspid valves,"—(That must be a mistake, because the tricuspid valves are on the other side of the house. It must have extended into the curtains of the mitral valve)—"and produced a singular contraction in the osteum." I presume it is meant, that, besides the ring of bone, the whole root of the mitral valve was changed, and a small opening was left in the centre. The rest of the description relative to the heart is unimportant. It is also stated that there was "great degeneration of the substance of the liver. Its body was considerably augmented, and yet its inferior margin hardly extended below the edge of the hypochondrium." There was therefore great disease of the liver and of the heart, and the disease of the heart stated here, was disease of the mitral valve. It is not said whether the left ventricle was hypertrophied or not—dilated or not; neither whether the auricle behind the obstructed opening was dilated. I do not know when Dr. Hopkins died, but the account is dated the 1st of February.

Now Dr. Hopkins applied to me last July, and here is the note which I then made of his case. I saw him on the 1st of July, and the symptoms were these:—"Dyspepsia; lies badly on the left side, and best on his back; pulse 88; dull sound, which is natural over the region of the heart, very extensive." (By this I mean, more extensive than it should be), "very loud sound in the region of, and at the action of, the left auricle." By that I mean a very loud sound, not on percussion, but on the left half of the cardiac region, that is to say, the region of the left auricle. "This sound is heard after the action of the left ventricle." Immediately after the ventricle has acted, and at the same moment as the loud sound above, in the lower region of the left ventricle, and the lower part of that region, was a sound loud like a drum, in double or triple succession. It was a very curious sound, and I never met with such an instance before. It was just like the roll of a drum. After the left ventricle had struck the side, there was a second heard, which consisted of three or four hollow sounds, all distinct, exactly like a roll on a drum. Sometimes I heard two and sometimes three sounds, just like the roll of a drum; and if I listened with the naked ear, not the stethoscope, then the sound was heard, only like a double sawing sound. Then came a pause, and then the ventricle beat again.

Now this took place immediately after the pulse, immediately after the action of the ventricles, and after death there was found disease of the mitral valve. The diagnosis I gave was, "disease of the mitral valve or," I added, "retrocession of the blood through the aortic valve;" because the blood will sometimes rush back into the ventricle. If the valves do not perform their duty thoroughly, then a part of the blood rushes back, and of course it rushes back the moment the ventricle dilates, and therefore it is often not easy to say whether the mitral valve is forming an obstruction to the blood rushing into the ventricles or an aortic valve is so diseased that the blood rushes back through it. It appears from the dissection that the noise I heard arose from disease and obstruction of the mitral valve, therefore it is clear, that the blood rushes through the mitral valve immediately after the ventricle dilates; therefore that the blood of the auricle runs into the ventricle immediately after the pulse at the wrist; therefore that Laennec is correct as to the time at which this takes place, only he is wrong in saying, that it arises from a contraction of the auricle; for it appears most probable that it arises from merely the passage of the blood from the auricle into the ventricle. I also added, "I presume there is great dilatation of the left auricle and hypertrophy of the left ventricle." But nothing more is said in the account of the autopsy. What, however, is said is this:—"great disease of the mitral valve and the liver." On seeing him a month afterwards, I found the same symptoms, and therefore I said the diagnosis was confirmed. I then found, in addition, that great enlargement of the liver had taken place. Subsequently, I have no doubt, that disease of the liver had taken place, which, when I first saw him, was not sufficient to give any sign to the hand. The organ was neither enlarged nor indurated, but on my second interview with him, I said that there were two diseases—disease of the heart, and disease of the liver; and no doubt the disease of the heart existed first, because that was evident when I first saw him. It is further said in the account in THE LANCET, "the lesion of the heart appeared to me to be the original disorder, and to which ought to be referred successively all the subsequent derangements." After I saw him he was a great deal worse. The ossification had increased, the diminution of the opening was greatly increased, and changes took place, amounting at last to general dropsy. When I saw him there was only œdema of the legs, but before it seems he died there was general dropsy. I regretted very much, when I heard that he was going abroad, that I should not have an opportunity of verifying the statement I had made on the grounds of Laennec's doctrine, but the dissection has confirmed the accuracy of my remarks.

ADVANTAGES OF AUSCULTATION IN CASES OF VOMITING OF BLOOD.

Respecting the cases that have left us, either by death or removal, since the last lecture, I will first relate that of a man who died suddenly, or *rather* suddenly, in William's ward. This case is useful, as pointing out the assistance of the ear in discharges of blood from internal parts. George Bally, aged 38, was admitted on the 8th of March, and said that he had been ill only from the previous Thursday, that is to say, a week; but he had complained of cough for three months. He said that he was seized with a violent vomiting of blood of a dark colour, which was mixed with the food he had taken, and that his stools also were of a very dark colour. Now from this account it would have appeared, that his disease was hæmatemesis, vomiting of blood.

The mode of distinguishing between the source of the blood that is discharged upwards, as to whether it comes from the stomach or the lungs, is threefold. In the first place, you make your distinction from the blood itself; in the next place, from the seat of disturbance of function; and, thirdly, from the union of the blood with the substance with which the function of the part has to do.

Now respecting the blood itself, if it come from the air passages, it cannot be retained long, and therefore is discharged almost immediately; in general almost as soon as it is poured into the bronchial tubes. Irritation is excited by its presence, which induces cough, and it is thus brought up.

If the blood is, as is usually the case, arterial, it comes up of a florid colour. Perhaps I am not right in saying arterial, because the mixture of air may give it exactly the arterial colour; however, it is not allowed to remain stationary there, and whether it was arterial originally or venous, from being in contact with air not being able to collect itself in a mass, it is thrown up at once, and therefore of a bright colour. If it be effused into the stomach, whether it is arterial or not, it is frequently allowed to remain there in a mass, and will coagulate, and even if it was arterial, from remaining in a mass, it will of course become black. You know, that if two ligatures be passed round an artery, the blood between them being at rest, will become as black as venous blood. It becomes black from the want of coming into contact with oxygen.

But it may be that the blood in hemorrhage from the stomach is of a venous character. That, however, is not proved, for if it be of an arterial character, still from being allowed to remain in the stomach it becomes very black. On this account I do not conceive that because the blood is bright in hæmoptysis it is proved to be arterial, and that because it is black in hæmatemesis it is necessarily venous. In the one case the parts are so irritable and so small, that it cannot collect into a mass, but is presently thrown up, and is in contact with oxygen; while in the other, it may be collected into a considerable mass, and is no longer in contact with oxygen. It is a general rule, that the blood in hæmoptysis is of a bright scarlet, and in hæmatemesis, that it is black. I say a *general rule*, not a universal rule.

The next means of diagnosis is to observe with what substance the blood is mixed, whether with the substance with which the function of the lungs has to do, or the substance with which the function of the stomach has to do. Now the substance with which the function of the lungs has to do is air, therefore when blood is thrown up from the lungs, it is generally frothy, and when thrown up from the stomach, if anything has been taken into that organ, it is generally mixed with food, and thus we have the second means of diagnosis. We may also see it *frequently* mixed with the fæces—not *always*.

Then the next point is to observe which function is disturbed, whether the function of the lungs or of the stomach. Now if it be the function of the lungs, you may expect a sensation of heat all over the front of the chest. You may expect some expectoration besides blood, and you will likewise expect that there will be something wrong when you listen to the chest. When it comes from the stomach you may expect that there will be uneasiness in the region of the stomach, and vomiting or nausea. When there is no vomiting, the blood comes up from the stomach without any serious effort, with an effort hardly amounting to vomiting, you may still find the patient complain of nausea. So, when the blood from hæmoptysis is poured up, you generally find cough, but not always.

Now there was considerable difficulty in making a diagnosis in the case of this man. When he came in I did not see the blood, and therefore I could not judge of the character of the blood as to whether it was black or florid, or mixed with food or air. You might suppose that it is very easy to learn all this from the patient himself, but amongst the lower classes people are very frequently quite incapable of giving any account whatever of their disorders. This man made it appear that he had discharged his blood from the stomach, because he said it was black, that it came up in considerable quantities, and that it was mixed with food, and that it had appeared in the fæces. On the second day he gave a different account. He said that it came up with a cough, and that it was mixed with froth. In this dilemma, being unable to gain information from him, I listened to the chest, and thought I found sonorous rattle of the bronchiæ, showing that the lungs were in a state of disease—that is to say, that there was irritation of the bronchial membrane. There was likewise mucous rattle, showing, in all probability, that there was some little blood there, or a great secretion of mucus. At any rate the sonorous and mucous rattle showed, with regard to the functions of the lungs, that there was something wrong in the bronchial membrane, that there was either inflammation present, or fluid there which ought not to be; or, indeed, there might be both.

Treatment.—I therefore, on finding sonorous and mucous rattle, made up my mind that it was hæmoptysis, and administered lead, because nothing is so good in the way of medicine as superacetate of lead. Cold ablution likewise is particularly useful. With respect to the lungs there is nothing to be compared with lead, and with respect to the alimentary canal there is nothing to be compared with oil of turpentine. As I made up my mind that it was hæmoptysis, I ordered two grains of superacetate of lead every four hours, and put him on slops.

The day following, his pulse being full, I had him bled to twelve ounces, and he had no more hemorrhage, and went on very well till he grew dull, stupid, and comatose. A blister was applied to the back of the neck in the first instance, and he had twenty ounces of blood taken from that part, and two grains of calomel were given him every three hours, because I was afraid that effusion was taking place into the brain. Nothing was more likely than that blood had been effused, that there was the same hemorrhagic disposition existing in the head that had appeared in the trunk, and into which blood had been poured forth. I was afraid that there was an effusion of serum, but I said that I should not be surprised if an effusion of blood took place. It is common in many of these cases to give opium with the lead, and some persons make it a rule to do so, but I do not. This man took two grains of superacetate of lead every four hours, and it gave him no pain in the bowels. It did not disagree at all with him, and the hemorrhage ceased. He had nothing more of it before he died. Had I given him opium, I should have been uncomfortable when the coma came on. I should have been tempted to ascribe it to the opium, and when he died I should have been vexed, and blamed myself as having been partly the cause of it, by giving him opium. But he had taken none. He became more and more comatose, and whether people brought him drams, it is impossible for me to say. Leeches were applied to the temples, and a cold wash to the head, but the stupor increased, the pupils became motionless, and the pulse small and quick. The man was obstinate, and refused food and medicine, and everything that was given him, and on the 15th, at three o'clock, he was seized with a fit of coughing, brought up a little blood, and expired.

Now on opening him we found blood in the bronchiæ and stomach, and some, I believe, in the intestines. In the bronchiæ was a quantity of black clotted blood. Having been so exceedingly comatose when he died, I presume there was less sensibility of the bronchial membrane than natural, so that the blood did not cause cough, and thereby lead to its expectoration. It is also to be taken into account, that he died very quickly. Now the blood could not have got there, of course, from the stomach. If he had vomited blood, it is not likely that any quantity would have gone down the bronchiæ, but we found a considerable quantity not only in the divisions of the trachea, but in the small branches of the bronchiæ.

The blood in the stomach was mixed with air, and it appeared to be blood which had been swallowed; I will not say that it was, but it is quite clear that that is no argument against its having come from the lungs, because from lying in the stomach after death, though it had been arterial before, it would become black. It looked as though it had been swallowed, and it is a much more easy thing for blood coming from the bronchiæ to be swallowed, than for blood coming from the stomach to find its way down the bronchiæ; I should therefore conclude, especially as I heard sonorous and mucous rattle, that this all had come from the bronchiæ, but that a part had escaped from the stomach.

The account of the dissection is this:—The trachea and bronchial tubes were filled with blood; there was also a considerable quantity in the duodenum; but there was no organic disease. On opening the head, a serous effusion was seen under the membranes of the brain, both at their surface and their base, and some little deposition of lymph. There was considerable effusion into the ventricles.

The particular interest of this case consists in its pointing out the use of the ear in doubtful cases of this description; where you hardly know whether the function of the lungs is disturbed or not. It enables you to refer the source of the blood to the lungs, and not to any other part.

CHOREA.

Another case to which I will direct your attention, briefly, is that of a girl in Mary's Ward. She came in with chorea; she was about ten or twelve years of age. There was nothing peculiar in the case. It was a common instance of the disease, but it is useful, as showing what I have always found,—that in this affection there generally is no disturbance of any function of the head or the abdomen:—that there is no pain in the head, no giddiness, no drowsiness; nothing that would lead you to suppose affection of the head, except the chorea itself. This may be an affection of the spinal marrow and head, but there is no affection of the head, such as drowsiness, giddiness, weight or pain in the head; neither is there any affection of the abdomen. The bowels of this patient were as regular as those of other people; her tongue was clean, and her appetite was good. This is what I most frequently observe in cases of this disease.

Treatment.—She was put on the carbonate of iron, a medicine which I always employ, because I am anxious to go on with it till I meet with a case of failure,—a thing which I have never yet done, except where the disease was local, or occurred in an adult, in either of which instances I have always failed. She took two drachms every eight hours, and after a time, as she did not go on so quickly as I could wish, the dose was increased to half an ounce, when she rapidly improved, and presently went out perfectly well.

HYSTERIA.

The next case is that of a young woman named Catherine Sullivan, who came in with hysteria, which was marked by a want of colour, and by a deficiency of the catamenia. She was sixteen years of age, and said she had been ill fourteen weeks. She said that sometimes she had palpitation and a difficulty of breathing, increased on exercise; but on listening to her heart, there was no more violent action of one part than of another, no great impulse of the left ventricle only, but a generally increased action, —no preternatural sound,—nothing but palpitation, such as we are all liable to, if we walk quickly. When that is the case, you have no right to infer organic disease. At least there is no reason for asserting that there is organic disease. There was œdema of the lower extremities, pain of the head, and the catamenia were irregular. This appeared to be a state connected with deficient excitement of the system, deficient formation of blood.

Treatment.—In these cases occurring in young women, iron is one of the best remedies, and she took a drachm of the tartrate three times a day. She was admitted on the 9th of February, and began this treatment on the 10th. On the 14th, as her bowels were relaxed, there was added a drachm of the carbonate to the tartrate, and she continued taking a drachm of each three times a day. When you see deficient menstruation attended by paleness of the body, the case is not one for merely stimulating the womb, but one in which the whole system together requires invigorating. It is quite vain to stimulate the womb when there is too little or too poor blood in the system. The best way, then, is to strengthen the whole system, and if the uterus then does not perform its functions, to give additional stimulants to that particular part. Under this treatment, persevered in, she gained colour in a great degree, the palpitation became less, and the menses appeared on the 14th March. She had been ill fourteen weeks, but I do not know that the catamenia had been deficient all the time. They had, however, been irregular and scanty. There was also in this case a sensation of choking, so that altogether it might be called hysteria connected with general debility and a deficiency of menstruation.

HYSTERIA, WITH EXTREME TENDERNESS OF THE BODY.—DECEPTIONS OF HYSTERIC PATIENTS.

There have been several cases of hysteria admitted lately, many of them accompanied by the extraordinary circumstance of tenderness of the surface of the body,—exquisite tenderness, influenced by such pressure as could not affect peritonitis or pleuritis,—such gentle pressure as can affect nothing but the skin, and the substance immediately subjacent to it. I have seen this mistaken over and over again for inflammation of the peritoneum, and persons have been bled, cupped, starved, blistered, and confined to bed, all to no purpose. I believe the chief mode of distinguishing these cases is from the absence of general disturbance,—emaciation, foulness of the tongue, quickness of the pulse, and the usual symptoms that you have in chronic peritonitis, which complaint of course weakens the system excessively. The tenderness, too, in these cases, is increased by the very slightest pressure that can be made, and while it is increased by such slight pressure, the general disturbance of the system is not at all commensurate with it. When there is peritonitis you have extreme tenderness, such as will not admit of the pressure of the bedclothes, but there is a general disturbance of the whole system, quickness of pulse, a sinking of the patient, every symptom indicative of a dangerous state; but here there is not.

One of the best modes of getting rid of this affection, is to give iron. It is one of the things that will give way to iron generally. Sometimes there is vomiting, but in most cases there is not. You may observe, too, that in hysteria it is a very common case for the mind to be affected: there is a disposition to artfulness. In these disorders females are really ill, but they have, I am confident, a morbid state of the mind, a disposition to make themselves appear worse than they really are. Sometimes they will tell you that they have this, that, and the other symptom, which they have not. It is just the same in the case of young ladies. I have known young ladies, who have had the best education, in the enjoyment of every possible comfort in the world, with nothing whatever to plague them, who, notwithstanding, have in hysteria deceived their friends and their medical attendants by all sorts of tricks. From the observations that I have been able to make, I am sure they are ill, but they will pretend that many things exist which do not, and pretend that they are unable to do things that they can do; and this I am sure is a morbid state of mind—a part of the disease.

When you have cases of this description in the hospital, you are by no means certain that it is not an attempt to deceive you simply for the purpose of getting into the hospital. You will recollect that there was a woman here lately, who contrived to present blood which she said she had spit up. Now it was blood of a brown colour; it was not of a fine red hue, neither was it black, but it was of a brown colour, and such as is continually presented where there is reason to disbelieve that it has been expectorated or vomited. It is generally mixed with a considerable quantity of thin fluid and saliva, as though it had been sucked by the mouth. She was clearly in a hysterical condition; she had a great number of hysterical symptoms; but there was the blood, and the question was where it came from. I examined, as well as I could, the state of the chest, but I was unable to satisfy myself that it came from that part. I also examined, as well as I could, the abdomen, but I was still equally unsuccessful in satisfying myself as to that origin of the fluid. I stated, indeed, that I could not say whence it came. I would neither call it hæmoptysis nor hæmatemesis, but I said we would wait patiently until we could clearly ascertain its source. Before my second visit, Dr. Davison found this lady in possession of a lancet, with which it appeared she pricked her gums, and she then sucked them, and presented the blood. I believe a paragraph has appeared in some of the newspapers stating, that we were all deceived in this case, that the girl was not only an impostor, but that she practised her imposition successfully. Now that account is perfectly incorrect, for we never pretended to know the origin of the blood. We could not, as I have already stated, allow that it was hæmoptysis, neither could we allow it to be hæmatemesis, but said nothing about it, having determined to wait and see whence it came; and so doing, we discovered it to be a trick.

There is a patient now in the hospital spitting blood, which exhibits the same appearance. She has had extreme sensibility of the chest and abdomen, but by taking iron the affection has left the former situation, and is confined to the latter. I may mention that she used the shower-bath, and had there been much chronic inflammation, it would most likely have done her injury, which it did not. Now this girl shows blood in the same way, and I dare say that if she have not a lancet, she procures the fluid by suction.

Both these ladies, I think, have some sort of depraved desire, for each of them was found to have a catheter in bed. The first patient had no reason to use it, for I believe her urine passed freely; but still a catheter was found there, though it was a very small thing compared with some articles that have been found under similar circumstances, and she showed a strong desire to secrete it. The other girl, who is now in the hospital, and who spits forth blood, pretends to require the catheter, not for the vagina, but for the urethra, and asserts that she cannot pass her water without it. The instrument has been taken from her, and her water is regularly drawn off twice a day; but it is very likely that this is all a pretence, and that she can pass her water if she pleases, without the aid of an instrument.*

PROPAGATION OF THE CHOLERA.

LETTER FROM DR. JAMES JOHNSON.

To the Editor of The Lancet.

I can assure your "Correspondent" that it is neither my wish nor my interest to misrepresent either men or books; nor has it been my practice, as a journalist, except by inadvertence. Some allowance is to be made for the hurry of a debate, and for the inaccuracy or misapprehension of reporters. Thus in the report in question I am made to deny, that I ever advocated "contingent contagion," though I never ceased to advocate that doctrine. What I denied was, the having changed my opinions. A hasty perusal of the article in The Lancet, on the evening of the discussion, impressed me with the idea that the writer of that article had greatly modified, if not altered, his opinions respecting the propagation of cholera, and a reperusal has only tended to show me, that it would probably have been more correct to say, that "in the opinion of The Lancet, cholera has changed its character relative to the mode of propagation, since it left Newcastle."* The writer maintains that up to the above period, it travelled "step by step, and with all the characters of a disease still *essentially* contagious." But subsequent facts and successive irruptions of the disease in distant and unconnected localities, have induced the writer to acknowledge that "the statistics of cholera have undergone the most complete alteration." "These facts," observes the writer, "prove that *infection* is no longer the *essential* cause, and even tend to indicate that, for a time, its operation may be materially suspended." The above passages led me to conclude, that the writer had changed or greatly modified his opinions, though perhaps my mode of expression was too strong, but certainly not designed to misrepresent the meaning of the passage.

Before closing this note, I would ask the writer of the article, what better proof is there of the *introduction* of cholera into Sunderland from Hamburgh than from Newcastle into Haddington? What better evidence have we that the contagious germ crossed the ocean from the continent to England, than from Haddington to Kirkintilloch, or any other distant locality? One thing is now clear, that, since the disease has come under our own observation, it has either lost its *essentially* contagious character, or more rigid scrutiny has divested it of a character which it may not have possessed, notwithstanding the imposing accounts, and the progressive statistics, of the epidemic. A cholera report is now hardly published without some remote and isolated spot exhibiting the disease, while all attempts to trace its footsteps, so visible (as is said) on the continent, are here given up by almost universal consent.

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* This turned out to be the fact, and the young woman now not only passes her urine freely, but has lost her tenderness nearly entirely, under the use of large doses of iron and a cold shower-bath twice a day, though, before her admission, she had in vain been cupped and starved.—Rep. L.

* Had Dr. Johnson here said, "the indications of its character of propagation," &c., he would have more accurately described our statements and opinions. We repeat, that up to the time when cholera reached Newcastle, every step in its progress indicated the march of an essentially contagious disease. On the other hand, since that time its many-leagued jumps as plainly point to the probable operation of another cause. We also willingly admit that the evidence supplied by the Haddington, Kirkintilloch, and Paris irruptions, materially operates against the conclusions deducible from the statistics of the disease up to the time when it reached Newcastle. We repeat our opinion, however, that no reasoning being, unendowed with the spirit of genuine prophecy, could come to any other conclusions than those we drew from the premises supplied us at each successive period of our debating this subject.—Ed. L.

Among the various appellations by which the *nova pestis* has been described, it is strange that no one has christened it "CHOLERA SALTANS" or the leaping cholera; for no disease has shown such a propensity for hopping from one town or county to another, without touching those places over which it leaped. There is reason to believe that cholera is changing its character in more respects than one. In Paris they have three kinds of cholera, one of which is the *red-hot* cholera, as contradistinguished from the "cold-blue." In Dublin it appears, by a recent communication, that "the malady has decidedly altered its type since first introduced. Not one patient in thirty has it now in the original form." Its more general form on the banks of the Liffey is that of fever, with cramps, and gastro-enteric irritation. These changes in the disease itself, may enable us all, with less *inconsistency* than is seen in higher quarters, to modify our opinions on the score of *expediency*, where we are unwilling to attribute such changes to conviction. I am, Sir, yours, &c.

JAMES JOHNSON.

Suffolk-place, 15th May, 1832.

A Treatise on the Injuries, the Diseases, and the Distortions of the Spine, founded on an Essay to which the Jacksonian Prize for the year 1826 *was adjudged by the Royal College of Surgeons.* By R. A. STAFFORD, M. R. C. S., &c. London, Longman, 1832. 8vo. pp. 302.

THE numerous and important affections resulting from disease of the spinal marrow, of its membranes, or of the bony canal by which this portion of the nervous system is enclosed, have long engaged the serious attention of practical men. Amongst English authors who have written most fully on diseases of the vertebræ and spinal marrow, we may enumerate Earle (Sir John), Macartney, Baynton, Copeland, Ward, Shaw, and Boyle, while the excellent treatise of Ollivier* shows that our continental neighbours have pursued the subject with equal attention.

The treatise now before us, is an addition, though not a voluminous one, to the works on this branch of human maladies,—a plain practical comment on the most important disease of the spine; not a lengthened disquisition, full of historical details, but confined entirely to such points as are most likely to require the attention of the surgeon at the bed-side of the patient. The several affections which the volume embraces are illustrated by numerous cases, the value of which is considerably enhanced by accurate accounts of post-mortem appearances. We sincerely wish that more of our modern authors would imitate the example of Mr. Stafford in this respect, and so substitute important facts, for ill-concocted and groundless theories.

The arrangement of the subject-matter in this work is good. Congenital diseases of the spine are first treated of; next its injuries, comprising concussion, fractures, and dislocations; then diseases of the vertebræ; and, finally, diseases of the medulla spinalis and its membranes. We do not propose to follow Mr. Stafford through the whole of this arrangement, which necessarily contains much with which medical men are familiar, but shall confine our observations to some extremely well-illustrated points.

Of the malformations which arise from imperfect development of the medulla spinalis, or the vertebral column, the chief is that denominated *Spina Bifida*, a congenital disease consisting in a deficiency of the spinous processes, or the rings of the vertebræ. In consequence of this deficiency, a pouch or tumour is formed at some part of the back corresponding with the preternatural opening, and consisting, usually, of the membranes of the medulla, distended by the fluid contained in the canal, which is secreted in increased quantity. The shape and size of the tumour will depend on the form and magnitude of the original malformation of the bones, and it may present itself at any point of the spinal canal, from the cervical portion to the sacrum. The termination of this formidable disease is almost invariably fatal, whether left to itself, or opposed by the skill of the surgeon. The period of time at which death supervenes is, however, very uncertain. Sometimes the tumour bursts during parturition, in other cases life may be prolonged for a great number of years. The author quotes an instance where the individual was in his 22nd year; but the most remarkable example, we believe, on record, may be found in Frank, who describes a patient as being seized with mania, and dying with spina bifida in his 54th year.

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* Traite de la Moelle Epiniere.

The changes of structure which the membranes undergo during the progress of the disease are described by the author to be,

"- - - generally, inflammation, enlargement, stretching, and thickening; and the different degrees of these alterations depend upon the different degrees of activity of the disease. In those cases where the tumour fills rapidly and bursts, or where the infant dies soon after birth, we find only inflammation with enlargement; but when the fluid contained in it accumulates slowly, and the infant lives for some months, or longer, then the parietes are observed to be thickened, without much inflammation. The integuments and skin are liable to undergo those changes which are common to all other parts of the body. Thus, for instance, they are sometimes inflamed, sometimes ulcerated, and sometimes gangrenous; which arises from their great distention."

It is not the least curious circumstance in the history of nervous diseases and malformations, that many affections of the central nervous system proceed to an extraordinary extent without producing any of those symptoms which usually accompany lesion of this delicate tissue. The arrangement of the nerves, in the particular malformation of which we now speak, is very well described by Mr. Stafford. We do not know of any other work in which this part of the subject is so well handled.

"In spina bifida it appears that either part or all the filaments which form the posterior spinal nerves, corresponding to the solution of continuity of the vertebral canal, are distributed upon the internal walls of the tumour, and that they generally terminate there. These arise from the cord in various ways; sometimes only by single filaments, which pass out of the aperture of the canal through the fluid contained in the tumour, unattached to anything until they arrive at its parietes, where they are dispersed. Sometimes again the posterior spinal nerves come out of the opening in the canal, and instead of going straight through the fluid, pass round the internal surface of the tumour, and thus form a network of nervous filaments. In other cases there arises a kind of peduncle in the middle of the tumour, of a nervous mass, resembling a mushroom, of which the stalk is formed of filaments of nerves bound together by cellular tissue, and the head by their extremities spread out and ending upon the upper and internal surface of the tumour.

"The posterior spinal nerves, which are distributed upon the internal walls of the tumour, do not always pass through the aperture connected with the vertebral canal; but sometimes they pierce as it were the tumour, and then distribute themselves on the internal surface. Sometimes again we find that the whole of the medulla is brought out of its course through the aperture of the canal, and that it completely ends in the tumour, leaving the canal below quite hollow.

"The medulla spinalis also in spina bifida, at the part where the malformation of the canal occurs, becomes changed in its structure in various ways; sometimes it has been found inflamed—sometimes not so large as natural—and sometimes of a firmer consistence; sometimes, again, it has been rendered completely soft—and at other times has been found altogether deficient."

It might be imagined from the extensive changes here described, that spina bifida is always accompanied by a paralytic state of the lower extremities, but experience shows that so far from being paralytic the child frequently remains stout and healthy. Frank, to whom we have already alluded, relates the case of a female child affected with spina bifida, who had perfect motion of the lower extremities, though the bladder and rectum were paralyzed. Perhaps we might attempt to explain this variety in the symptoms by reference to a fact established by modern physiologists,—that the functions of the nervous system are not disturbed when an alteration takes place by means of the gradual pressure of a fluid not producing change of structure. Thus in some cases of spina bifida, the nerves composing the cauda equina may be merely unravelled by the fluid, and preserve, intact, their functions, with their original structure.

The formidable nature of this disease baffled the skill of ancient surgeons, who only used palliative measures. It remained for the original mind of the late Mr. Abernethy to propose a successful method of cure,—a method which has since been adopted by surgeons with the greatest advantage. Mr. Abernethy introduced two methods,—compression and puncture. At first he proposed the application of gentle pressure on the tumour, from the commencement, with the intention of giving that support to the dura mater which it had lost from its deficiency of bony covering, and at the same time of producing, if possible, the absorption of the fluid. The other method, or radical cure, consists in frequently puncturing the tumour, making gentle pressure upon it, and healing up the wound. Numerous trials have shown that this practice affords the only chance of a permanent cure, and that it has, in many instances, been attended with complete success.

"The mode in which the consolidation takes place, in the radical cure of spina bifida, is probably as follows :—That as the fluid contained in the tumour diminishes in quantity, the skin and the membranes composing the walls of the tumour become thickened and contracted; adhesive lymph is thrown out from the internal surface of the tumour, which becomes organized also, and so the cavity of the tumour is in progress of time closed up."

The possibility of a spontaneous cure of spina bifida has been much doubted by all preceding writers; but Mr. Stafford brings forward a case which shows that nature is sufficient for the cure of this formidable disease, and that she uses means exactly similar to those proposed by Abernethy, and practised by Cooper. The case is altogether so interesting that we shall extract it entire :—

"Alfred Seymour Church, æt. two years and three months. There is situated on the lower part of the back, over the inferior lumbar vertebræ and superior portion of the sacrum, a tumour of firm consistence and of rather unequal surface, about the size of half an orange; it is moveable, and of a darker colour than the adjacent integuments: when pressed upon or moved, the fæces are instantly evacuated. The spine, above the tumour, is slightly curved outwards; the inferior extremities are singularly deformed, and can be twisted almost in any direction : the flesh is flabby.

"This case is perhaps the most remarkable that ever occurred of spina bifida, as it combines in it almost all the facts of importance that have ever been recorded, both by the ancients as well as the moderns : first, as to the immense size of the tumour; secondly, as to its combinations : there was hydrocephalus of great magnitude—deformed limbs—deficiency of the nates, with imperforate anus, and diminished nervous power; thirdly, as to the progress of ossification of the vertebræ : the rings were not only defective, but the bodies also, and the intestines could be seen through the tumour which communicated with them, as the fæces were evacuated through it; fourthly, as to the subsidence of the hydrocephalus when the tumour sloughed off—the possibility of which fact has been doubted; fifthly, as to the reason suggested by the mother for the origin of the disease; sixthly, as to the spontaneous cure—another fact hardly credited; and seventhly, as to its present state and the age of the patient."

The chapters on the injuries of the spine contain a plain statement of facts without communicating anything very new or very interesting. The summary of morbid appearances in those who die from concussion of the spine, is worth transferring to our pages :—

"The parts exterior to the osseous canal are much contused, and sometimes blood is found extravasated in the cellular membrane. The membranes of the medulla spinalis are in a high state of vascularity, opposite to the point where the blow took place, and occasionally one or more of them are ruptured. There is also more or less blood, either coagulated or not, between the dura mater and osseous canal, the dura mater and arachnoid membrane, the arachnoid membrane and pia mater, and sometimes between all at the same time. When the patient dies immediately after the accident, the medulla spinalis is generally of its natural colour and texture, but now and then it is rather redder, and if minutely examined through a magnifying-glass, the capillaries ramifying upon it may be seen injected with blood. When the death of the patient has been delayed, it is not uncommon to find it of a yellowish red colour, and of a softer consistence, and in some cases where there has been much inflammation, it is rendered into a more fluid state at the part where the concussion took place. Occasionally it is observed that the inflammation of the injured portion has extended itself along its own substance to the brain, as well as along its membranes, to those of that organ, and in this case there is a quantity of fluid found in the vertebral canal."

With this extract we shall close the present notice, adding, that after a careful perusal of the work, we consider it deserving our recommendation to the profession.

The Dublin Journal of Medical and Chemical Science. May, 1832.

(Concluded from our last.)

CASE OF DISEASED HEART.—BRUIT DE SOUFFLET.

The article which follows the papers noticed in our last number is entitled, "A case of diseased heart, accompanied by Angina Pectoris, exemplifying the effects of the Hydrosulphuret of Ammonia." By Andrew Newton, Esq. Though rather tediously detailed, the paper is not devoid of interest. The following incidental remarks on the "bruit de soufflet" are of considerable value.

"On applying the stethoscope over the region of the heart, the head was elevated by each cardiac impulse. The pulsations were irregular and intermitting. A very loud bruit de soufflet was heard, resembling the sound produced by a spinning-wheel whilst in rapid motion; this sound accompanied both the auricular and the ventricular contractions, it was more marked at the left than at the right side, but was distinctly audible over the entire chest; the respiratory murmur was everywhere natural, but feeble, and marked by the bruit. On examining the carotid, by means of the stethoscope, a loud and very distinct bruit was heard, differing however in character from that which existed at the heart, it was accompanied by a strong impulse; this phenomenon was observable, not only in the carotid and large arteries, but even in their minute ramifications, thus it was very apparent even down to the ends of the fingers. His pulse was full, hard, and intermitting, 88 in the minute. On comparing the pulse at the wrist with the pulsations at the heart, the interval between them was much more marked than in health; the frequency of the pulse was not altered by change of posture."

With reference to these phenomena, Mr. Newton subsequently remarks :—

"On opening the pericardium, the heart was found enormously increased in size, this enlargement was principally at the left ventricle, the parietes of which were very much thickened, the cavity was not much larger than natural, the right ventricle was dilated, and its parietes slightly hypertrophied, the auricles were healthy, no valvular disease could be detected either in the heart or arteries, there was a very slight ætheromatous deposit in the aorta immediately beyond the semilunar valves. The arteries of the neck, chest, and abdomen, presented no lesion of structure.

"It is difficult to account for the bruit in this and similar cases; no doubt it is a frequent, almost a constant* attendant upon valvular disease, yet it is not unfrequently found in cases where the valves are perfectly healthy, and in which no organic disease of the heart or arteries can be detected, either during life or on the most minute investigation after death. From the examination of a considerable number of cases during life, compared with the appearances observed on dissection, I have come to the conclusion, that a "bruit," when permanent and combined with other anormal cardiac symptoms, is invariably pathognomonic of organic disease.

"I shall not enter into the question, as to whether this remarkable phenomenon be attributable to spasm, as was supposed by Laennec, or be purely mechanical, as asserted by Dr. Corrigan; further researches are requisite to enable us to determine whether either of those suppositions be correct."

In the treatment of this case, after digitalis and various other remedies of high repute had completely failed, the hydrosulphuret of ammonia was used with great advantage as a sedative and palliative medicine. It was given in doses of from three to ten drops thrice daily in a glass of water. Mr. Newton thus meets some objections made to its use, and recapitulates the therapeutic qualifications he believes it to possess.

"With regard to the nausea which it produced when given in Sir Patrick Dunn's hospital, I cannot help supposing, that this was attributable either to its not having been sufficiently diluted, to its having been exhibited in too large doses, or to some error in its preparation; any of those causes will produce this effect.

"Whilst upon this subject, I may mention, that this medicine has been repeatedly tried, both by Dr. Marsh and myself, in various diseases, principally cardiac and cutaneous, in all it produced a powerful effect in lowering the pulse, the appetite was increased, and also the secretion of urine; in some cases where it was not sufficiently diluted, it produced nausea, headach, and giddiness, these disappeared on omitting its use for a short time, and then giving it in a less concentrated form.

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* In case of valvular disease, whether in the heart or aorta, the bruit may disappear when the circulation has been much lowered, but even in this case it can be easily re-produced, by causing the patient to make any exertion, such as walking across the room, or running up-stairs.

"From the trials which have been made of this medicine in cutaneous diseases, there is reason to hope, that it may be found to possess some value in the treatment of this class of complaints.

"In consequence of the tendency which the hydrosulphuret of ammonia has to undergo decomposition, it should always be recently prepared and kept in a dark place; when used, it should be given by drops, in preference to prescribing a draught or mixture."

SUDDEN RUPTURE OF THE HEART.

Dr. Townsend, an able pathologist, and the well-known translator of Andral, furnishes the next article, which is entitled, "Contributions to Pathological Anatomy," perhaps the most valuable of the many excellent papers with which the number abounds. As the nature of the essay forbids analysis or condensation, we must restrict ourselves to a very brief notice. The first and second cases described, are sudden death from spontaneous rupture of the heart. The remarks advanced on the duration of the symptoms induced by this accident seem to us exceedingly just, and calculated to dissipate much unfounded preconception relative to the cause of sudden death from cardiac ruptures. He believes, for example, that perforation of the heart, from whatever cause it proceeds, is generally followed by immediate death. Bertin states, that of ten cases of this accident, eight died instantaneously, and the other two after a few hours. The extent of hemorrhage into the pericardium cannot account for these fatal consequences, as the quantity of blood effused is often very trifling. The great and sudden shock which the nervous system sustains, is more probably the immediate cause of death. In some rare cases, death does not take place for several days after the accident. It is stated, that in these cases the perforation has been found plugged up with a coagulum of fibrine.

The third case is very singular. Death occurred suddenly with convulsions, and on dissection there was found a transverse rupture of the inner and middle coats of the ascending portion of the arch of the aorta. The quantity of blood effused in the ruptured parts did not exceed a teaspoonful. On this case Dr. Townsend remarks, that the rupture of the aorta was the only anatomical lesion to which the immediate cause of death could with any probability be referred,—that it is evident that this accident could not have preceded the fatal catastrophe many minutes, as otherwise the impetus of the column of blood projected by the heart, and striking directly against the cellular tunic of the aorta, must have protruded that yielding membrane into a pouch, as in the ordinary formation of false aneurism, or at least have detached it to some extent from its connexion with the middle arterial tunic, as in the remarkable case of rupture of the inner and middle coats of the aorta, related by Laennec, vol. ii. p. 700, where the blood insinuated itself between the middle and outer coats of the vessel, dissecting them asunder from the arch of the aorta to the bifurcation of the iliacs. In the present case, he adds, there was scarcely any effusion of blood, but the shock which the nervous system sustained from such an extensive injury of the circulating system, the central organ of which was previously in so very diseased a condition, is quite sufficient, he conceives, to account for the fatal consequences that ensued.

ANEURISMAL TUMOUR ON THE AORTA.

Dr. Townsend next describes an aneurismal tumour as large as a hen-egg on the anterior surface of the arch of the aorta, composed of a fibrinous coagulum, and situated between the *external* and *middle* coats of the vessel. The author thus describes the anatomical relations of the parts.

"On examining the arch of the aorta, a hard solid tumour of the size and form of a large hen-egg was observed on its anterior surface, near the origins of the left carotid and subclavian. On slitting up the aorta, it was seen that this tumour, which projected considerably into the anterior of the vessel, was situated between the outer and middle coats of the artery, both of which appeared perfectly sound, and showed no mark of any solution of continuity. On making an incision into the tumour, it was found to consist of a solid mass of fibrine, of a pale yellow colour; its surface only being stained with the colouring matter of the blood. It was evident, therefore, that this tumour was formed of a coagulum of blood, and was situated between the external or cellular, and the middle or fibrous arterial tunics.

"Its presence in such a situation could, I conceive, only be accounted for on the supposition of its being caused by hemorrhage from the vasa vasorum, which are usually of considerable size in that neighbourhood. It certainly could not be considered as an instance of a spontaneous case of aneurism, as, though the aorta was slit open and examined most carefully, no trace could be found of any communication having ever subsisted between the cyst and the interior of the vessel."

TUBERCULOUS MASSES IN THE LEFT AURICLE.

The last case related is of like rarity with the others. Death supervened from asphyxia, caused by large tuberculous masses developed in the parietes of the left auricle, compressing the trunks of the pulmonary veins, so as to reduce their diameter to that of a crow-quill, thereby preventing the return of the blood from the lungs.

Dr. Townsend thinks this case worthy of record, as it serves to illustrate a morbid condition of the heart and lungs of extremely rare occurrence, and likewise as exemplifying a pathological fact, which he has had repeated opportunities of observing, namely that a *permanent organic lesion* may give rise to symptoms of a *remittent* or even of an *intermittent* character. In this instance, the mechanical obstruction to the pulmonary circulation, and the consequent congestion of the pulmonary tissue, must have opposed a constant obstacle to the due aëration of the blood, and yet the symptoms of distress were only felt occasionally, or at least were greatly aggravated at intervals. In like manner, he states that he has known aneurism of the abdominal aorta distend the nervous filaments of the solar plexus to such a degree, as to lacerate several of them, and yet the pain experienced in this case occasionally intermitted for whole weeks together.

The last original paper in the number is a description of a case of collapse after parturition, in which cold aspersion was employed with the most marked benefit, by Mr. Hayden, of the Anglesea Dispensary. The aspersion was made with a hearth-brush dipped in a basin of cold water, and shaken suddenly and forcibly over the patient's face.

Journal de Pharmacie. No. IV. 18e. Année. Avril, 1832.

The Pharmaceutical Society of Paris forms a striking contrast with the Society of Apothecaries of our own metropolis. The members of the first are ardent in the cultivation of the branch of science to which they are attached, whilst the latter are almost entirely absorbed with the important concerns of the counter and the till. The Parisian pharmacopolists are also anxious to diffuse as wide as possible the knowledge and benefits of the researches which they unremittingly carry on, but the cockney triturators think only of the multiplication of certificates, and the consequent jingling of students' guineas. By an immense stretch of liberality, indeed, they have at last brought themselves during the summer months, to open for the benefit of medical students generally, the Botanic Garden which was intrusted to their keeping by Sir Hans Sloane. What unrivalled consciousness and self-denial, to give to the public what was originally designed for them, after a silent monopoly of more than a century!

HYDROGEN ACID AND THE CYANURETS.

Taking up the number for April of the Parisian Pharmacien's journal, we were attracted by the first paper, which is by Monsieur Pelauze, upon the transformation of hydrocyanic acid and the cyanurets into ammonia and formic acid. He details a number of appropriate experiments, and endeavours to establish the following corollaries, which are worth the notice of our pharmaceutical chemists.

1st. That the hydrocyanic acid is transformed into ammonia and into formic acid, by the action of the hydrochloric and sulphuric, and by other acids.

2d. That the cyanuret of potassium submitted in concentrated solution to the action of heat, is changed into ammonia and formate of potass.

3d. That the same formed at a high temperature and under the influence of an excess of potass, yields hydrogen, ammonia, and a residue of potass.

4th. That a proportion of cyanuret of mercury on acting upon a proportion of hydrochloric acid, gives a proportion of hydrocyanic acid, and a proportion of perchloride of mercury.

5th. That an excess of the hydrochloric acid produces with the same cyanuret a double chloruret of ammonia and of mercury, formic acid, and very little prussic acid.

6th. Finally, the formate of ammonia soumis to the action of heat, is converted at 180 degrees into water and hydrocyanic acid.

After this well-written article, " Reflexions sur les Causes de Cholera Morbus," by M. Nicolé, follow, in which the writer adopts the views of animate contagion entertained by Linnæus, and more fully dilated upon by our talented countryman Dr. Neale.

RESINS OF JALAP AND SCAMMONY.

The third paper consists of a memoir by M. Planché, upon the mode of distinguishing the resin of scammony from that of jalap and convolvulus arvensis, soldanelle or bind-weed.

" The resins of jalap and scammony, too often confounded in medical practice, merit to be distinguished as much on account of their manner of acting on the animal economy, as on account of the phenomena to which they give birth when in contact with chemical agents. For example, the resin of jalap is soluble in alcohol, and insoluble in ether. It is dissolved cold in nitric acid without disengagement of nitrous gas; whilst, on the contrary, the resin of scammony is soluble both in alcohol and ether, and dissolves itself in nitric acid, with the production of nitrous vapour. The resin of soldanelle deports itself with these three agents nearly in the same manner as that of scammony, and although the smell and taste are very different in these two resins, and scarcely permit their being confounded, a young physician has remarked to me, that the characters which I have selected are not sufficiently prominent.

" However little foundation this objection may have, I have attempted by new experiments to remove it, and after several trials, I hit upon the following means of diagnosis.

" Pure milk from the temperature of 10 degrees above 0 on the centigrade scale up to the boiling point, *divides perfectly, and holds in suspension, the resin of scammony.*

" Under the same circumstances, pure milk brings together, and unites into a mass, the divided molecules of the resins of soldanelle and jalap. In this instance two results are produced; first the discovery of an excellent criterion for the resin of scammony, which enables the inquirer to decide, upon the instant, whether this resin be adulterated with that of jalap or bindweed; and the second result is, the attainment for the scammony of a vehicle of which the savour is generally pleasing, and which may be sweetened or aromatized according to the taste of the patient. The following formula I think a convenient one; it affords an excellent dose in obstinate constipation, especially in women. Take of resin of scammony of its natural colour or decolorated with animal charcoal *eight* grains; white sugar *two* drachms; eau distillée de Laurier cerise *three* minims; cows milk hot or cold according to the taste of the patient *three* ounces. The scammony is reduced to powder by trituration in a mortar. The milk gradually added, and finally the sugar and aromatic water."

NEW MODE OF OBTAINING CHLORINE.

Mons. Tourtois communicates an expeditious method of obtaining chlorine. The following are the proportions of mixture adopted by Mr. Chevalier:

300 grains of *marine salt.*
100 grains of *deutoxide of lead.*
160 grains of *sulphuric acid.*
water q. s.

"These substances, states M. Tourtois, being mingled, the following reactions occur. Reasoning on the hypothesis that the marine salt decomposes the water, the deutoxide of lead is reduced to the state of protoxide; of the 10.40 grains of oxygen which it contains, it loses 3.46, which finding the hydrochloric acid set at liberty by the sulphuric acid, decompose it, take away 0.43 grains of hydrogen, and set free 15.30 grains of chlorine. The two last quantities united give 15.73 grains of hydrochloric acid, which are far from representing the whole quantity included in the 300 grains of marine salt, since there are in that salt 160.95 grains. The atomic theory of proportion, therefore, demonstrates that the quantity of salt employed is too great, and that it ought to be reduced so that the exact quantity of chlorine required should be extricated, that is 30 grains, which would produce just 15.73 grains."

By another calculation M. Tourtois shows, that the quantity of sulphuric acid, as indicated by M. Chevalier, is too small to decompose entirely the marine salt, and to unite itself to the oxide of lead. And he concludes by proposing the following formula,

300 parts of *deutoxide of lead.*
100 parts of *marine salt.*
160 parts of *sulphuric acid.*
5500 parts of *water.*

"SACRED BOTANY."

The original communications are concluded by a talented paper, by M. J. J. Virey, styled Pharmacon Hiéron or Sacred Botany.

He arranges the several botanical substances applied to the purposes of religious culture, under four heads.

1. Sacred perfumes, the writer states, were originally employed to purify the atmosphere of the ancient temples, from the impure emanations which arose from the occasional assembling of large masses of people; and above all, from the sacrifice of animals at those times. The incense which was first used for this purpose was the produce of a tree which is very common in India at this day, the *boswellen thurifera.* Another terebinthine plant, the chloroxylum dupoda procures equally an odorous resin, used in the same manner as olibanum, in the pagodas of Hindostan. The writer enters into a pretty minute and highly interesting description of the various odoriferous resins used for religious ceremonies at different ages, and in different countries of the world. This research is, perhaps, no otherwise useful to the medical inquirer, than as tending to enlarge his views as to the mysterious agency of medicinal substances, and to disabuse his mind from the ordinary belief, that the operation of medicine, because taking place under our eyes every day, is easy of explanation. Nothing is so injurious to the progress of our art as the mental satisfaction arising from such deceptive associations.

2. Aromatic woods were also much employed, in oriental temples especially. By the perfume arising from these it was believed, that eminent curative effects occasionally resulted.

A peculiar holy water, distilled from cannella and other aromatic trees, is employed in the Lamaic temples of Thibet.

The priests of ancient Egypt burnt cassia cannella, and cinnamon, in their places of worship. It was thus that the phœnix, the emblem of the sun, was resuscitated from the ashes of its nest formed of these perfumed barks, and the apotheosis of princes and heroes in ancient days took place in the sight of the people, from the bosom of their funeral pile, when these odoriferous woods were consumed.

At the time of the conquest of America, the guaiacum was employed as a perfume in all the temples of the sun at Cuzco. And the name, *palo sante* or *holy wood*, is retained to this day by the Spaniards.

3. Narcotic herbs were likewise much resorted to by the prophets, sybils, and conjurors, of the heathen world. By the abuse of these substances an intoxication was produced, which passed with the ignorant then, as madness does now with the Moors, for inspiration.

It is thus that the *hassich*, or the leaves of hemp of which the smoke is inhaled, excites a holy drunkenness amongst the true believers of Islamism, and makes them rush to martyrdom. It is said that the term *assassin* is derived from the word *hassich*, because the fanatics exalted by its vapours were prompted to suicide and murder.

4. Of sacred plants, one of the most anciently celebrated, is the famous *ficus religiosa*, which bends its branches towards the earth; these branches again taking root, form thus natural arches of foliage under which the innocent Brahmins, who live on vegetables only, are sheltered. They have as great a horror of shedding the blood of animals as of man. In *Genesis*, the passage stating that God clothed Adam with fig-leaves, appears to be an allusion to the primitive kind of life in the forests of India, the first cradle of the human race.

The Sanscrit poets make allusion to an equally celebrated tree, the *plas* or *palasa* of Hindostan, which serves as a principal ornament to *Crishnagar*, one of the divinities of the Hindoos. This is the *butea frondosa*, a tree of the family Leguminosæ, with long blood-coloured flowers, which deter the timid natives of India from wounding it, by exciting a belief that it is endowed with sensibility. Its red astringent juice is a sort of kino.

According to the Brahmins, the nymph *Fulasi* has been metamorphosed into a species of plant, as in the Greek mythology of the transformation of Daphne into a laurel. The *ocymum sanctum* is dedicated to Vishnou, and his worshippers chew every day the stomachic leaves of this plant to facilitate the digestion of the simple vegetables which constitute their diet.

The Celts and the Gauls had also their consecrated trees. Amongst the Druids in Mona and Armorica, the *verbena* officinalis was held in great veneration, for it yields a balsamic odour. Virgil also alludes to this plant, Eclog. viii. 65.

> Verbenasque adole pingues et mascula thura.

Finally, the above flower, almost sacred amongst the ancient Egyptians, as well as amongst the Hindoos, was the aquatic *lotos*, upon which, according to these people, creative power was first exercised, These flowers are of two kinds: the *red*, which flourish in the waters of the Ganges, and are worshipped as the cradle of the divinity fecundating nature, and from which it is said emanates all generative power, as Venus rose out of the bosom of the waves by the influence of the solar rays. The other lotos is that of the Nile, it is the *nympha cærulea*, and is often represented upon the Egyptian monuments. It adorned the head of Osiris, or was plaited into a crown for the brows of kings and priests, or appeared like a *blue* rose in the hands of their divinities. The seeds and roots of this plant being highly nutritive and refrigerant, excited still greater veneration for this plant.

We have thus been rather liberal in our draughts from the *Pharmacon Hiéron*, for we are convinced there is a much closer connexion than is generally supposed, between ancient superstition and modern medicine. The medical inquirer, who pries with sufficient acuteness into these subjects, will find that many of these seemingly ridiculous observances are founded on correct observation, and that when abstracted from unessential circumstances, they may be happily applied to practice in medicine. It is not meant to recommend an indiscriminate deference to the written or traditional aphorisms which abound in the teaching the influence of external agents on the human body, but merely to deprecate a passive conformity with those arbitrary classifications of remedies, when facts which are impossible, and analogies incredible, are assumed as data. It is certain that a leaning to the side of credulity in the efficacy of mechanical agents, is likely to have a more favourable effect upon the mind than a tendency to scepticism; for the first temper of mind is adapted to the candid reception of facts, while the latter checks all inquiry, through the influence of a dogmatic presumption.

THE LANCET.

London, Saturday, May 19th, 1832.

One of those farces denominated the "annual hospital dinner," was performed on Saturday week at the Albion Tavern, Aldersgate-street, by some of the gentlemen connected with Saint Bartholomew's, in the animated atmosphere of Smithfield. The repast was abundant, the wines rich and high-flavoured, the company tolerably numerous, the songs good, but the speeches—oh! the speeches!—"flat, stale, and unprofitable." But let them pass; though we scarcely can withhold a remark or two upon the glumpy and sombre appearance of Mr. Flint Stanley, when, on the toast of "the lecturers" being given, the Little Cock-sparrow sprang upon his perch, and chirruped his thanks to the amusement and edification, doubtless, of his auditors. This display sadly discomfited the unfortunate Mr. Stanley, who, until that moment, had considered that *he* was the most important of the lecturers, and ought to have thanked the company for the honour conferred upon himself and his colleagues. Deprived, however, of the opportunity of displaying his eloquence, he concentrated his wrath against the cock-sparrow, by exclaiming to a friend on his left, "As Earle delivers *gratuitous* lectures, he thinks he's entitled to make gratuitous *speeches*."

We have on several occasions reprobated these "annual" exhibitions of folly and imbecility on the part of their managers. Public dinners, of all assemblies in the world, are the most unsociable, unintellectual, and unentertaining, unless the proceedings be conducted by men of decided ability and commanding eloquence. What, we would ask, is to be gained by hearing such speeches, such twaddling trash as was uttered on Saturday week at the Albion Tavern? All running thus:—

Sir and Gentlemen,—I feel extremely grateful for the honour which has been conferred upon me. This is the happiest moment of my life. Nothing can afford me higher gratification than such testimony (Yes; in salmon and lobster sauce!) of your approbation. The school of Bartholomew's has sent forth some of the greatest geniuses that medicine ever produced. We *had* an Abernethy, we *have* a Lawrence, ("So has the College!" exclaimed a wag at the bottom of the table,) and by the constant discharge of our duties to the patients and to the students, of whose *substantial* interests we are never unmindful, we hope still to preserve the reputation of this renowned establishment.

Mr. Chairman and Gentlemen,—I have nothing more to say at present, except in once more returning my very grateful thanks, and drinking all your very good healths.

However, we might have overlooked the stupidity of the scene, had not the first toast furnished us with a kind of index to the motives which influence *some* of the conductors of these make-baits for students. Who, gentle reader, think you was in the chair? Mr. Pennington, the ex-apothecary of Keppel-street,—medical attendant for many years to all the influential, ultra, rotten-hearted Tories resident in the north-west district of the metropolis. The selection of such a man for such an office, at this juncture of public affairs, indicates very bad taste, if not the influence of a strong political motive, and *that* motive one which ought to be detested by the whole body of young gentlemen educating for such a profession as that of medicine. Mr. Pennington is well known, in his circle, to be an ultra-Tory; hence it was in perfect keeping with that character that the first toast from the chair should be "*Church* and King!" On hearing such an announcement many of the students remained on their seats, and others were observed to turn their glasses upside down, while the *clique* vomited forth their hollow-hearted cheers. On Saturday week the name of William the Fourth was warmly cherished in the affections of the whole of his people; at that time his promise to purify the constitution of this country remained in its full force; hence the monarch was beloved by all, and by none more ardently than the young men of England. But it would have been deemed disloyal by Mr. Pennington, and his right-hand supporter, Mr. Alderman Lucas, to go through the ceremonies of a public dinner, without proposing the health of his Majesty. True, there was a double object—the support of *the monarchy*, and the insult of *the King*. Thus while the *crown* was upheld, *his Majesty* was introduced to the notice of the company under the sanction of the bigoted, Tory-engendering, law-established *Church*. Now we would appeal to Mr. Pennington and his enlightened party, whether, as honourable men, they can reflect with satisfaction upon the conduct pursued by them on this occasion. Independently of the badness of the example, can it be supposed that young gentlemen, who are now conversant with the politics of medicine, can be unmindful of the persecutions inflicted upon them from the ascendancy of

Tory principles? The whole of our old medical institutions are conducted under Tory forms of government, and there is not one of them which holds out a reward for merit, unless it be the merit of sycophancy or intrigue. The College of Physicians is a Church-and-King establishment, the Apothecaries' Company is another institution of the same stamp, and the College of Surgeons is the worst of the three. It presents a perfect model of the Church-and-King system of government. We hope and believe, that the gentlemen now attending our medical schools are not so insensible of the disadvantages under which their predecessors have studied, as to be induced to feel any kind of reverence for the almost exploded toast of "Church and King." During the long period when the High-Church party was in the ascendant, what laws were enacted for the benefit of our profession,—what laws, indeed, sanctioned by *that* party, with a view to benefit any really liberal profession? Referring to events for instruction, we find that the Church-and-King conclave lost no opportunity of raising obstacles to the progress of knowledge generally, and with respect to the cultivation of the science of medicine, in what instance have they attempted its advancement? The members of the High-Church party have ever been notorious for insolence and narrow-mindedness, and have never held out a friendly hand to talent unless with a view to pervert it to the darkest purposes. Under the long reign of Toryism, our public chartered schools have been allowed to slumber in a state of the most criminal inactivity. By the parliamentary returns, it is quite clear that these institutions possess sufficient funds for affording a classical education to the entire youth of this country, and yet with such a power at their disposal, to what ends have the Tories—the real Church party—employed it? The splendid structures built by our generous and well-designing ancestors have been allowed to crumble into ruin, and the funds deposited for their support and for that of their children, designed to be instructed in them, during several generations of misrule have been squandered on rapacious aspirants to place and power. If the conditions of the charters of our public schools had been strictly complied with, would not medical learning in England have far exceeded its present point? Would such a charter as that of the College of Physicians, or that of the College of Surgeons, still exist; or would an "Apothecaries' Act" have passed in the spring of 1815? The details of Church-and-Tory practices have been equally odious with the principles inculcated by the most hateful fanatics in the dark ages. We marvel that even Mr. PENNINGTON,—Lord ELDON's apothecary, could, in a company of gentlemen consisting principally of medical students, pronounce the words "Church and King" without a blush, recollecting as he must the abuses which had sprung up when Toryism was in its ascendancy—abuses which still exist, because the branches of that system are not wholly destroyed. If there had not been many series of Church-and-King governments, would pupils who seek for public instruction in a public institution have been made to pay immense sums to enable them to pass the bare thresholds of those establishments? But the splendour of Toryism is displayed in the trappings of office, and the glitter of gold. Weight of intellect in a *Tory* scale ever kicks the beam when it is opposed to a weighty *purse*. Considerations relating to intellectual acquirements are as nothing when placed in competition with the possession of wealth. Thus while the Church-and-King party flourished and succeeded in obtaining an extent of political and hierarchical power never yet equalled, the science of medicine remained stationary; or if improvements *were* made in that branch of knowledge, they resulted rather from the extraordinary exertions of individuals, than from the labours of corporations acting under the sanction of government. This toasting of "Church and King" at a public medical dinner, is one of the numerous attempts daily made to keep the public in the dark, for the motto for the Tories is this:—"Improve not in the arrangements of your civil and political institutions, even should you by any *misfortune* improve in knowledge." They have ever said to us, "You may perceive the light, but you shall not be guided by it:" accordingly, not only are the imperfections produced by time sanctioned by legal recognition, but attempts made to tolerate abuses of modern growth by referring to others of a grosser character, but of a more ancient date. While these things were in progress, what was the toast received with the greatest acclamation at all the great public dinners? Why, that of "Church and King." But as the public became more enlightened, and the intolerance of the clergy became more apparent, and consequently more odious, the bumpers were quaffed to the toast of "*King*" only, and this was generally the case towards the conclusion of the reign of GEORGE the FOURTH. Even at the Bartholomew's dinners, if we are correctly informed, the toast embraced the monarch only, but *then* (on Saturday week) when we had a Sovereign who had apparently remained firm to his pledges of *reform*, and who by those pledges had proclaimed himself the friend of a just system of government and liberal institutions, it was deemed by Mr. PENNINGTON and the Stewards, a fit occasion to insult the ears of the pupils by proposing, as a toast, "*Church* and King." Of course the circumstance itself is almost unworthy of notice, but indicating as it does the feelings which direct the Tory party in all their movements, convincing us as it must of their desire of continuing abuses in all their forms,—of perpetuating pecuniary demands upon the students,—of increasing the number of fetters by which they are already clogged in their pursuits, we have deemed it worthy of this public reprobation. Nor are we without hope that the remarks here made will have the effect of inducing the company instantly to resent such an insult, should any Chairman—like Mr. PENNINGTON—be so indiscreet or ill-intentioned as to propose, at another public dinner of medical students, a toast which we suppose is now seldom heard except within the cloisters of Oxford. The Church-and-King *system*, whatever may have been its advantages to the clergy, and to an hereditary nobility, has proved a terrible bane to the mass of the people of England, and a bitter curse to the members of the medical profession. Mr. PENNINGTON either knew this, or he did not. If acquainted with it, why was he guilty of hypocrisy so deep as to propose such a toast? If *un*acquainted with it, he was unfit for the office of President, and should have declined it in favour of his footman.

BARTHOLOMEW'S HOSPITAL.

DISEASED KNEE-JOINT—AMPUTATION.

WILLIAM SHEPHERD, twenty years of age, apparently of a scrofulous diathesis, was admitted into Luke's Ward last January, under the care of Mr. Vincent. The complaint, for which he claimed relief, was an enlargement of the knee-joint. He has followed the business of a shoemaker, and about two years since first had an affection of his right knee, which he could not assign to any particular cause. The disease gradually progressed, and the pain became distressing, shooting up the leg, the integuments retaining their natural hue. Upon this he went to the Middlesex Hospital, where he became an out-patient. The treatment adopted was the application of bandages. A gentleman then kindly took him into his house, and had a surgeon to attend him, who prescribed complete rest, and applied lotions and leeches. In January last he entered this hospital as above mentioned, with much swelling about the affected joint, and with his general health impaired by the local disease. The motions of the articulating surfaces upon each other were perfect, but attended with some pain.

The enlargement of the part became at length very considerable, particularly on the inner side, where it was hard and unyielding, and on the outer where the presence of fluid was indicated. Into this latter, therefore, Mr. Vincent made an incision, from which a copious discharge occurred. Within the last three or four weeks, genuflexion has been impeded, and productive of great disturbance. On the admission of the patient, cupping locally and fomentations have been had recourse to, and since that time the treatment has been very varied, but ineffectual; so that last week, finding the state of the limb becoming worse and worse, and operating very prejudicially on the system, Mr. Vincent determined to amputate the leg. The patient has had no cough, no swelling of the glands, nor any symptoms of pulmonary affection.

May 12. To-day the operation was performed above the knee, the leg being taken off at the lower third of the os femoris.

13. He is ordered *acetat. morphiæ*, gr. ¼, *hora somni, si opus sit.*

14. The patient's condition is favourable. No untoward circumstance has occurred since the operation. His bowels are open, and tongue clean. Pulse rather quick. His mind and body seem to have experienced much relief, as he expresses satisfaction at having been relieved from the weight of disease.

May 15th.—Patient continues much the same; is to have mutton chops daily.

To-day Mr. Stanley made an examination of the amputated member in the museum. Upon cutting open the knee-joint, the cavity was found filled with a thick puriform collection, after washing away which the articulating surfaces were examined. Here disease had been busily at work, and not in a very ordinary manner. The cartilage was in some places entirely absorbed, whilst in others, where it still remained, it was completely separated from the osseous structure. Instead of being as usual firm and adherent, it required but a touch to detach it, there being on the bone beneath it a reddish layer apparently of minute granulations. The surface of the patella exhibited the same appearances. The capsular membrane was generally thickened, and here and there inflammation had completely substituted a different structure. The manner in which the cartilages had been thrown off from the ends of the bone induced Mr. Stanley to imagine that the disease must have commenced there, although the pain had not been intense enough to support such an opinion. A section of the extremity of the femur was therefore made, but the interior of the bone exhibited nothing peculiar, nor of a scrofulous character, with the exception of a slight softening of its cancellous structure. The end of the bone exhibited a general enlargement, but the inner condyle presented no specific increase to account, as was expected, for the large protuberance on the left side of the joint, which appears to have been owing to some displacement of the parts from their natural position. The ligaments remained entire, but the patella was thrown outwards from its usual place, whilst the femoral extremity projected more inwardly.

16th. Not so well; complains of smart pains in the stump; had little sleep last night, which he ascribes to the omission of the medicine—the *morphia*. Bowels open; tongue clean; pulse rather small and quick.

LIGATURE OF A NÆVUS MATERNUS.

William Giles, a child ætat. 2, was brought from the country by his mother, for the purpose of having Mr. Lawrence's advice on this affection. The little patient is in excellent health, and Mr. Lawrence recommended an operation. The situation of the "mother's spot" in this instance is on the upper eyelid of the left eye. External to the tarsus arises the tumour, which is circular, and about an inch in diameter, its outer and under circumference being bordered by the external canthus, and lower edge of the eyelid, which is partially pressed down by it, but not so as to obstruct vision. The swelling is subcutaneous and of a purple colour. It attained nearly its present size about twelve months after birth. Compression easily acts upon it, and violent exertion, such as a fit of crying, causes the tumour to swell rapidly.

15th. This morning Mr. Lawrence performed the operation. Were this a common tumour, he said, extirpation would have been very easy with the knife, which was, however, contra-indicated here by the likelihood of extensive hemorrhage, which he had witnessed in some cases even in putting in a seton for the cure of such excrescences. In addition to this, retraction of the eyelid from the wound was to be feared. The mode here adopted, therefore, was *ligature*. Taking two curved needles, strong silk ligatures were introduced through them. These two instruments were then passed through the tumour, which was thus marked out into three parts, and the silk being divided at the eyes of the needles, eight ends were obtained. The two extremities of the tumour were then tightly bound by four of these, whilst with the remaining two the middle portion of the nævus was tied; some degree of care being required in the latter proceeding, on account of the intimate union of the nævus at that part with the inferior edge of the tarsus. Mr. Lawrence at first thought of using only one double ligature, but finally considered the method adopted as safest and surest in this instance. Judging from the cry of the little patient, the operation is not attended with so much pain as might be expected. He is now in "Faith."

FISTULA IN PERINÆO.

May 10,—Daniel Mease was admitted into Henry the Eighth's ward about six weeks ago, with formation of matter around the anus. He is to appearance a very healthy man, and states that he has been tolerably free from disease till five weeks before admission, when he first had pain about the region of the anus. After this succeeded shiverings, and other concomitants of such affections, but he has not been much depressed. Since in the hospital, the part has been leeched and poulticed; the collection of matter gradually increased, and at length "broke" just by the side of the anal aperture. A copious evacuation continually took place, the discharge not being very offensive. To-day Mr. Lloyd, who was "going round" for Mr. Lawrence, after examining the situation and extent of the fistulous cavity, thought it desirable to lay it open immediately. The probe having penetrated some way upwards to the side of the os coccygis, a director was introduced, and a division made with the curved bistoury. Further examination, however, discovered a sinus leading from the line of the first incision across the raphé perinei, and then descending on the opposite side; Mr. L. therefore proceeded to expose every part of the cavity, which he did with a probe-pointed bistoury, the situation of the parts rendering them difficult of access. The man bore the process uncommonly well, though it was apparently a very painful one.

This Mr. Lloyd observed was a very perfect example of that kind of fistula which Cheselden, from its shape, has described under the appellation of the "*horse-shoe* fistula," just that kind of form assumed here by the burrowing of the matter around the anus. In such cases particularly, in order to effect a complete cure, he considers it not only expedient, but necessary, to slit up every remnant of the passage. A small, partial, opening would only be productive of mischief and continual disturbance, for as that incision healed, the matter would be burrowing in another direction. The operation is painful, but it is the only method by which to expect an *extirpation* of the disease.

15.—The wound has been kept open, with a simple dressing applied. There is a good deal of discharge of a "laudable" character, the patient's bowels are open, and regular, and he appears very comfortable.

AMPUTATION OF THE FORE-ARM.

May 3. A boy, ætat. 13, was brought to the hospital this morning with a severe injury to the fore-arm, occasioned by a machine. There was a large lacerated wound, bleeding very copiously, and extending from near the wrist to within two inches of the elbow joint. Through the wound projected a large portion of the ulna, which was fractured in a very comminuted manner. The radius appeared to have escaped fracture, but the nature of the accident rendering amputation necessary, Mr. Lawrence had him taken into the theatre about two hours after his admission. Upon examining the state of the parts, it was found that the bone and integuments were sufficiently sound to admit of amputation below the elbow, which was therefore done by Mr. Lawrence by the circular method. Four or five arteries were secured, after which blood still trickled rather copiously, the origin of which was found to lie in the medullary arteries of both bones. Upon proceeding to form the stump, it was discovered that the end of the radius projected too far to admit of the flap being well drawn over it; the obstacle, therefore, was removed by means of Liston's bone-nippers. This instrument, however, was not so effectual or speedy in operation as was desirable, a circumstance which called from Mr. Lawrence the remark, that the handles were too short—that the addition of more length in that quarter would give a greater purchase, and more power might consequently be applied to the blades with the exercise of less force. Nothing has occurred to merit notice since the operation.

AMPUTATION OF THE ARM.

May 5. Another amputation was performed to-day, by Mr. Earle. The subject of it was a lad, Alfred Gunton, ætat. 14. He had been standing that morning in the street, with his arm round a post, and whilst in this position, a waggon came along, and before he could be aware of it, the limb was terribly crushed between the wheel and the post. Upon admission here, Mr. Earle saw him, and found the injury so extensive, and the necessity of immediate operation so obvious, that he deemed it desirable not to wait for Mr. Lloyd, who had been sent for, the boy being *his* patient. The lad was then taken into the theatre by Mr. Earle's desire, where he amputated the arm below the shoulder-joint. The head of the radius was found to be broken off, the elbow-joint severely injured, and there was also compound fracture of both bones of the fore-arm.—The patient is doing well.

WESTMINSTER HOSPITAL.

ŒDEMA OF THE LUNGS, WITH ANEURISM OF THE AORTIC ARCH.

Mary Anne Dent, a widow, 46 years old, admitted March 27th ult. She is a tall thin person, with dark hair, eyes, and complexion. She states that she has, for many years, been in service, and enjoyed excellent health until six months ago. The catamenia ceased about three months after an exposure to cold immediately subsequent to great exertion. She had cough and pain in the chest, with expectoration of tenacious mucus. About a month ago she spat blood, which was at first dark but afterwards florid. She has perspired much at night, and has lost appetite and flesh.

28th. Her lips and face are livid; laborious respiration; pulse feeble; extremities indicating lentor of the pulmonary circulation. The chest is pretty sonorous, and the râle sonore is evident almost on every phase of it, but more particularly at the base of the lungs, *A resonance amounting almost to pectoriloquy* was discernible in the supraclavicular region of the right side, and less remarkably on the left. Sputa copious, consisting of rounded yellowish masses, mixed with glairy fluid, and expectorated with difficulty. Heart's action *natural*, though feeble; pulse 86, *regular*. Skin hard and dry; sleeps indifferently.

29th. The patient was taken suddenly in the night with a sense of suffocation, and died in the course of a few moments.

Post mortem, 30 hours after death, in the presence of Dr. Roe, Dr. Edwards, and others.

The body was emaciated generally, but the right arm and side in a greater degree than any other part. The *head* presented no other morbid appearance than a slight serous effusion under the pia mater and into the ventricles; and the cerebral substance was perhaps a little softer than natural. In the *chest* the lungs were observed not to collapse on the admission of air. They were of a pale grey colour externally, and pitted on pressure. Some old cellular adhesions existed on the right side. On slicing the lungs, serum flowed copiously from the divided surfaces, which presented a sodden appearance, except in the centre of the left lung, which he paritated to the *grey* degree. In the lower part of the same lung marks of recent inflammation were evident, such as the florid red colour of the tissue. And in various parts of both lungs pus oozed out of the bronchial ramifications. The larynx was paler than natural, as was also the trachea, except at its bifurcation, where it was redder. There were two or three ulcerets penetrating through the anterior part of the trachea plugged up by coagula and communicating with a cyst formed in the adjacent cellular tissue. The bronchial tubes were filled with frothy sputa, and their inner tunic was of a good colour. At the root of the arteria innominata, and including part of the aortic arch, was an aneurism of the size of a small apple; it was circumscribed and filled with recent coagula, its sides were dense and tough as those of the aorta itself. It communicated with the channel of that vessel through an aperture as large or larger than a shilling. The arch was a little enlarged at this part, but the carotids and subclavians were of natural diameter, the coats of all the vessels were considered to be healthy. In the ABDOMEN all the viscera were sound.

This patient was so short a time in the hospital that no opportunities were afforded to occasional attendants to discover any indications in the habits of the patient. There can be little doubt, however, that she exhibited that indisposition to motion and exertion of any kind which is characteristic of those patients having aneurism in any of the great vessels, and more especially near the heart. So universal indeed is this fact, that it may safely be considered pathognomonic. The influence of this dread of moving is manifest even in the instinctive actions, for the patient attempts to restrain the cough which is occasionally present, and to check the natural action of the heart by an effort of the will. In this case there is every reason to suppose the patient died of suffocation, which was brought on by this habitual resistance to the effort of expectoration. When this resistance was carried to a certain point, the effusion occurred and proved fatal very quickly.

NOTE FROM MR. SALMON.

Sir,—I committed an error in the case of Stricture of the sigmoid flexure of the Colon, which I transmitted to you for insertion, and which appeared in your journal of the 24th of March. I stated, when giving the particulars of the case, that "no examination of the rectum had ever been made," whereas it should have been, "no examination of the rectum, with the bougie, had ever been made." By inserting this in your next number, you will oblige, Sir, your obedient servant,

Frederick Salmon.

May 17th, 1832.

CORRESPONDENTS, &c.

The obstacles thrown in the way of the custom alluded to by *Mr. R. J.* are almost innumerable, but we hope shortly to overcome them.

The letter of *Dr. Clanny*, the paper of *D. Harrison*, and the second lecture of *Mr. Salmon*, shall appear next week.

THE LANCET.

Vol. II.] LONDON, SATURDAY, MAY 26. [1831-32.

CHEMICAL PATHOLOGY OF CHOLERA.

REMARKS ON "DR. THOMSON'S ANALYSIS OF THE BLOOD OF INDIVIDUALS AFFECTED WITH MALIGNANT CHOLERA."*

By W. B. O'Shaughnessy, *M.D., &c. London.*

It has afforded me much satisfaction to perceive, that Dr. Thomson of Glasgow has devoted his attention to the investigation of the chemical pathology of the malignant cholera. As the results he obtained are of considerable importance in deciding the question, what are the *universal facts* which chemistry has ascertained in the pathology and etiology of this disease, I deem myself not unprofitably employed in laying before the readers of this journal an outline of Dr. Thomson's investigations, accompanied by some passing remarks which they will occasionally elicit. My object throughout is to contrast Dr. Thomson's results with those described in my own report on this subject, to show in what points they differ, and in what they correspond,—where difference exists to examine how far the processes employed by either experimentalist may have influenced the result; finally to derive from these inquiries whatever conclusions they legitimately afford, as to the *essential* and *universal* chemical facts ascertained concerning this epidemic.

In the commencement I should briefly state, that the results of my own experiments indicated, *first*, a material diminution of water in the blood of the cholera patient, the specific gravity of the serum ranging from 41 to 54; *secondly*, a notable decrease in the quantity of soluble salts, amounting, as far as regards the serum, to a mean loss of one-third of these substances; *thirdly*, that the solid constituents of the crassamentum, including its salts, retained their normal *proportions*, wanting merely the addition of water to restore it to the standard state; and, lastly, that the dejections were alkaline and albuminous, and contained the water and soluble salts in which the serum of the blood was deficient.

In the first of these results, Dr. Thomson's analysis corresponds very closely with mine. Of five specimens of serum, the specific gravities were as follow:—

Standard of health, 1.0280.

1. 1.0116 pure yellow serum.
2. 1.0443 very slightly tinged red.
3. 1.0520 very red.
4. 1.0550 very red.
5. 1.0570 a very deep red.

In my experiments conducted in Newcastle, and published in my report, the specific gravities stood thus:—

1. 1.0410 pure yellow serum.
2. 1.0450 ditto.

In the experiments conducted in London, of four specimens the specific gravities were:—

1. 1.0427
2. 1.0540
3. 1.0470
4. 1.0513

To these I may add, the results of the analysis of Rose of Berlin, which afford, as the mean of his experiments, 1.0447.

In an analysis, in other respects extremely faulty and imperfect, published by Hermann, of Moscow, the specific gravity of the serum was also greatly increased beyond the natural standard. I conceive, then, that we are fully entitled to consider the universality of this fact in the chemical pathology of cholera, as completely and satisfactorily proved.

The second division of Dr. Thomson's paper treats of the proportions between the serum and crassamentum; a question very difficult to solve *in a mechanical point of*

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* Philosophical Magazine and Annals, May, 1832, Art. XLIX, Chemical Analysis of the blood of Cholera Patients, by Thomas Thomson, M.D., Professor of Chemistry in the University of Glasgow.

view, owing to the obstinacy with which the crassamentum retains the serum in its interstices. Indeed it would be wrong under any circumstances, to pronounce with apparent accuracy on these proportions from mechanical computation alone. We may, however, with some justice for the present, assume the standard to be 57 serum to 43 crassamentum.

In the only case in which I had leisure to make this kind of estimation of the proportions in cholera blood, they were 43 serum and 57 crassamentum, being precisely the reverse of the normal standard.

In Dr. Thomson's five specimens, the apparent increase in the solid ingredients was still more remarkable, viz.—

	Serum.	Crassamentum.	
1.	32.34	67.66	serum pure yellow.
2.	32.—	68.—	ditto slightly red.
3.	38.45	62.58	ditto very red.
4.	35.66	64.34	ditto very red.
5.	27.59	72.41	ditto very deep red.

The mode of analysis here requires particular attention.

In my analyses the proportions were thus ascertained. As much serum as possible was first mechanically separated from the crassamentum. A portion of the serum was then evaporated to dryness to ascertain the amount of its solid contents. *As the water of the crassamentum exists therein in the state of serum*, a portion of crassamentum was then evaporated to dryness, and the loss of water noted. The data afforded by the analysis of the serum, thus gave me, by a ready calculation, the precise amount of serum retained by the crassamentum.

Dr. Thomson, on the other hand, simply "poured off as much of the serum as possible, the crassamentum was then poured on a cotton cloth, and left for twenty-four hours, till all the serum that would had dropped through the cloth, the weight of the remaining crassamentum was then determined." This proceeding is obviously not quite so exact as the former, and tends to exaggerate materially the proportion of the crassamentum in the specimens he examined.

Dr. Thomson's third section refers to the composition of cholera serum. He assumes Marcet's and Berzelius's analysis for his standard, viz.—

	Berzelius.	Marcet.
Water	90.5	90.00
Albumen	8.0	8.68
Salts	1.5	1.32

In the experiments on cholera serum, Dr. Thomson's results were as follows, and I have to request particular attention to the circumstance, that *he calculates the albumen and salts together*.

	Water.	Albumen and Salts.	
Standard	90.5	10.	
No. 1.	83.95	16.05	serum pure yellow.
2.	83.96	16.04	slightly red.
3.	81.17	18.83	very red.
4.	81.1	18.9	very red.
5.	80.82	19.18	very deep red.

In my experiments I assumed Lecanu's analysis for my standard, and estimated the albumen and salts *separately*, a proceeding which I believe to be preferable, for reasons which I will subsequently explain. I also estimated by the 1000, not by the 100. In order, however, to contrast my results more intelligibly with Dr. Thomson's, I may thus state the issue of the two analyses made in Newcastle, taking the albumen and salts together in the mode practised by Dr. Thomson.

	Water.	Albumen and Salts.
Standard	90.60	9.0
No. 1.	85.40	15.70
2.	86.68	13.92

It is thus seen that my experiments here agree with Dr. Thomson's in again denoting a great deficiency of water, and *a corresponding increase of the albumen in the serum* compared with the normal standard. Dr. Thomson moreover, in his analysis, makes no allowance for the fatty matter which Lecanu and Denis have recently proved to exist in healthy blood, and which occured in both my analyses of cholera serum, and which are associated with the albumen in Dr. Thomson's analyses. The difference between us in the amount of albumen is thus shown to be more apparent than real. It is also to be remembered, that in no two cases can the ingredients of cholera blood be numerically the same, so completely disorganised is the condition of that fluid. Thus, in my own analysis of various specimens of serum, there existed these differences between Nos. 1 and 3.

	No. 1.	No. 3.
Water	8.54	8.66
Albumen	1.33	1.24
Salts	5.80	2.92
Urea	40.—	—
Fatty matter	1.43	1.28

This fact is an instructive one, as it proves mathematically that we are not to look for strict numerical correspondence in the results of various inquirers into the *chemical* pathology of this malady. Accordance in the broad results can alone be expected.

The next section of Dr. Thomson's paper, on the salts of cholera serum, is one to which I wish particularly to solicit the attention of the reader, inasmuch as some writers have theorised very liberally on the presence, absence, or varied proportion, of these ingredients in cholera blood, and have even based some remarkable therapeutic propositions on the second of these assumptions. I shall therefore examine this section in some detail. If, in doing so, I should have occasion to offer objections to the indications which Dr. Thomson's experiments apparently afford, I trust that distinguished and respected individual will believe me, when I state that my object in this inquiry is solely the eliciting of truth. I have no theory or opinion to support, and had I one I would cheerfully abandon it the moment it was rationally shown to be incorrect.

In order to render the subject as clear as possible to the general reader, I deem it necessary to premise these observations by stating, that our knowledge of the composition of the blood has been greatly increased within the last year, by the instructive and numerous analyses of M. Lecanu, of Paris, detailed in his prize essay, read before the Academie de Medécine. To my mind his experiments are altogether faultless, and his results are consequently pre-eminently entitled to be received as our standard and guide in all inquiries concerning this fluid. According to this eminent chemist, normal serum consists of water, a fatty matter, *an organic substance soluble in alcohol and water, a compound of albumen and soda soluble in water,* various saline matters partly soluble in water, and partly insoluble in that fluid.

Of these ingredients, the organic matter, soluble in alcohol and water, and the albumenate of soda constitute 3.79 per 1000 parts.

Premising these remarks, I may now with more advantage approach Dr. Thomson's experiments on the quantity of salts in cholera serum.

"To determine the quantity and nature of the salts contained in cholera serum, 394.36 grains of serum were exposed to the temperature of 159° in order to produce coagulation. The coagulum thus obtained was dried in a heat of about 200° till it ceased to lose weight. The whole was now reduced to 48.85 grains."

On this step of the process, which is intended to expel all the water of the serum, I feel it necessary to offer a brief observation. There is scarcely any process which requires more patience than the complete drying of albumen. When coagulated at first it retains much water and uncoagulated albumen in its interstices, and then from its imperfect conducting power much of the fluid albumen contained in these interstices is solidified, owing to loss of water, without undergoing coagulation. A firm pellicle or crust is also formed on the upper surface of the albuminous cake, which tends to retain much of the hygrometric moisture. For these reasons it becomes absolutely necessary to remove the cake from the vessel, when apparently dry, to triturate it carefully in a mortar into minute powder, to heat it again in a temperature of 212°, and continue this manipulation with much assiduity until the powder is almost impalpable. If this precaution be neglected the accuracy of the subsequent steps in the analysis is a little interfered with. The next sentence in Dr. Thomson's analysis illustrates the truth of this observation.

"This dry matter, in the state of a coarse powder, was digested for two days in a Florence flask on the sand-bath. The whole was then thrown on a filter, and the liquid which passed through was evaporated to dryness. This water had dissolved a notable quantity of albumen, which was separated during the evaporation in a hard transparent crust. I endeavoured to separate it as completely as possible by dissolving the salt three times successively in water, and evaporating the solution to dryness. By every successive evaporation some albumen was separated; but after the third evaporation I could obtain no more albumen by this process. The saline matter remaining weighed 2.39 grains: it was grey, acted powerfully as an alkali, and by the test of chloride of platinum appeared to contain both potash and soda."

"The salt contained no sensible quantity of sulphuric acid, nor of lime, nor of phosphoric acid: there existed in it 1.47 grain of common salt; the remaining 0.92 grain was alkali and a brown-coloured salt, which deliquesced, and which dissolved in alcohol: it doubtless also contained some albumen."

The digestion in water in the process thus described must have removed from the dried albumen, first, all the salts, such as the muriate and carbonate of soda soluble in water; *secondly, the organic matter soluble in alcohol and water; thirdly, the albumenate of soda*; fourthly, urea, a probable ingredient in cholera serum; and fifthly, *any uncoagulated albumen.* Now, for any of these organic matters, Dr. Thomson makes no allowance. In his tabular summary, he speaks of the 2.39 grains, as *salts*, without further comment or explanation; whereas it is manifest that a notable proportion of organic matter should have been deducted from this amount, for *the organic matter soluble in alcohol and water, and for the albumenate of soda*, independent of the albumen, much of which was separated during the process. In short, it is not at all improbable, that the 0.92 grain, or more than one-third of the entire, was the albumenate of soda, *and the organic matter soluble in alcohol and water,* the presence of which in serum has been pointed out by Lecanu and Denis, and confirmed by numerous experiments of my own.

Dr. Thomson then proceeds:

"The albumen thus freed as completely as possible from salt by digestion, was dried and exposed to a red heat for half an hour in an open platinum crucible; it left a grey ash, weighing 0.77 grain: this ash was partly soluble in water. The portion dissolved was common salt and an alkali, weighing 0.53 grain. The remainder dissolved in nitric acid, and was precipitated in white flocks by ammonia and by caustic potash added in excess; it was, therefore, phosphate of lime: it weighed 0.24 grain."

From these experiments, Dr. Thomson thus states the composition of the cholera serum, No. 1.

Water	83.950
Albumen ..	15.015
Salts	1.035 or 13-50 per 1000.

In my examination of serum, the salts obtained by solution in water, *were freed of the organic matter and the albumen of the albumenate of soda, by incineration,* before they were weighed, and accordingly my results were, in No. 1,

Water	85.40
Albumen	13.30
Salts perfectly free from organic matter	0.56

Had Dr. Thomson's mode of analysis been adopted, and *the organic matter soluble in water* been included in the estimation of the salts, my results in No. 2 would have stood thus:—

Water	85.40
Albumen	13.30
Salts	1.08

We thus find, that allowance being made for the organic matter associated with Dr. Thomson's "salts," his results and mine correspond in indicating a deficiency in the amount of the soluble salts in cholera serum. Indeed the analysis of the dejections evidently shows the presence of these salts in the rice water-fluid, and of course leads us to expect a priori, a diminution of their quantity in the blood.

Dr. Thomson's next statements on the quantity of these saline ingredients in cholera blood generally, are apt I fear to lead a superficial observer slightly astray. Dr. Thomson proceeds to furnish a table of the saline contents of the other four specimens of cholera serum, *calculated on the analysis in No. 1, and not derived from experiments on each specimen.* Now a single fact is sufficient to demonstrate the inapplicability of calculation to *this* branch of the investigation, and to prove the necessity of making each specimen the subject of separate analysis.' In my examination of cholera serum *no two specimens contained the same quantity of salts,* although in all the average was below the normal standard. Even the nature of the salts occasionally differed. Thus, in 1000 parts—

	Healthy Standard. Lecanu.	Malignant Cholera. No. 1.	Malignant Cholera. No. 3.
Muriate of Soda and Potassa	6.08	4.00	2.17
Carbonate of Soda.. Phosphate of Soda.. Sulphate of Soda ..	2.10	No carbonate of soda, but 1.60 of phosphate and sulphate of soda, earthy salts and iron.	0.5
Earthy Salts and Iron	0.91	——	0.70

This fact is decisive proof that proportional calculation cannot be admitted into our reasoning on the quantity or kind of saline matters in cholera serum.

I regret much that Dr. Thomson has not given any account of the symptoms of the patients who furnished the blood on which he operated. It is to be expected that the amount of the saline ingredients of the serum which are soluble in water, must hold a marked relation to the amount of the purging experienced.

On the whole the consideration of this section of Dr. Thomson's paper, and the results of his single analysis relative to the salts,' coincide with my experiments in showing, that as a universal fact the salts in cholera serum are diminished in quantity, but never completely absent.

The fourth section in Dr. Thomson's paper relates to the crassamentum. The amount of fibrin was found in the manner employed by Lecanu. The proportion of colouring matter was determined *by evaporating the water employed to wash the crassamentum to dryness in a temperature rather under* 212. The amount of saline matter was then examined by burning a portion of the colouring matter in a platinum crucible.

Before stating the results thus obtained, I find it requisite to notice the manner in which the colouring matter was examined, which tends materially to increase its apparent quantity. Besides the colouring matter itself, the washing took away the large amount of albumen retained by the crassamentum in the form of serum, and amounting in the serum of specimen No. 1 to 15.015 per cent. The colouring matter we shall thus find was in this manner apparently increased nearly one third above its actual quantity in No. 1. Indeed Dr. Thomson does not describe the colouring matter in his tables as pure, but gives it as "mixed with albumen." Dr. Thomson analysed two specimens to determine the amount of colouring matter.

There is perhaps no other ingredient of the blood, concerning the standard proportion of which so much difference of opinion exists among eminent chemists, as with respect to the colouring matter. The processes again devised for its separation are quite as numerous, quite as opposed to each other, and many of them (like that employed by Dr. Thomson) exaggerating remarkably the amount supposed to be obtained. In my experiments, the plan pursued was one which mathematically admits of no fallacy, if the operator possesses ordinary manual dexterity. A certain quantity, say 1000 parts, of moist drained crassamentum is taken, the analysis of the serum having been previously performed. A part of the crassamentum is then evaporated to dryness, the residue triturated to powder, and subsequently heated, &c., so that all its moisture is accurately expelled. A simple calculation, founded on the previous analysis of the serum, then tells at once how much serum was retained in the moist clot, and this you deduct from the weight of the residuum.

Another portion is now washed to obtain the fibrin, the amount of which when dried is carefully ascertained.

A third portion is incinerated, and the amount of saline matter noted.

Deducting then from the 1000 parts of moist crassamentum, the water, the albumen, &c., of the serum it retained, the fibrin, fatty matter, and the salts, the remainder shows precisely and infallibly the quantity of colouring matter. This is the simple, beautiful, and unerring method devised by MM. Provost and Dumas, followed by Lecanu, and adopted in my own experiments on cholera blood.

Another process, and one of much precision, is that practised by Denis, and pursued in the immense number of analyses described in his brilliant work.* It is founded on the fact, that albumen mixed with colouring matter, if much diluted, does not coagulate by heat, while the colouring matter, diluted with fifty parts of water, coagulates at 150 Fahrenheit.

The object of these processes is *to free the colouring matter from albumen.* That the results thus obtained are infinitely better entitled to be taken as our standard than those derived from the process followed by Dr. Thomson, *which retains the albumen, extractive, &c.,* amounts to a self-evident proposition.

In ascertaining the standard again, it appears from Lecanu and Denis's experiments, that the sex, age, temperament, and circumstances, of the individuals, must not be lost sight of. In some of Denis's experiments, for example, *the pure colouring matter amounted to* 18 *per cent.** In others it was as low as 6 and 7. The *mean* of all Lecanu's experiments *gave* 13 *per cent* of *pure* colouring matter *perfectly free from albumen.*†

We are now prepared to examine with more precision the indications afforded by Dr. Thomson's analyses of two specimens of cholera blood.

	No. 1.		No. 4.
Albumen	4.856	..	6.305
Fibrin	0.378	..	1.340
Colouring matter, *with albumen*	27.450	..	23.160
Salts	1.195	..	1.255
Water	66.121	..	67.940

At first sight the excess of colouring matter here appears considerable, but two circumstances tend to render this excess much more apparent than real. The first is the large quantity of albumen mixed with the colouring matter, owing to the deceptive process employed. A simple calculation suffices to show the precise amount of this source of error in the analyses before us.

The crassamentum in No. 1 (Dr. Thomson) contained of water 57.60 per cent. Now the water of the crassamentum exists therein in the state of serum.‡ The solid contents of that serum being soluble in water, are consequently included by Dr. Thomson in the sum of the colouring matter, because Dr. Thomson estimates the colouring matter by washing the *moist* crassamentum with water, and *evaporating the entire washings to dryness.* Now the previous analysis of the serum, belonging to that crassamentum (see *Phil. Magaz.*, page 353), showed, that exclusively of the salts, the serum contained of albumen 15.015 per cent. If then 83.950 parts of water are equivalent to 15.015 of albumen, 57.60

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* Recherches Experimentales sur le Sang Humain Consideré a l'Etat Sain. Par Prosper Sylvain Denis. 8vo. pp.358. Paris, 1830.

* See Denis Op. Cit., p. 142, 216, 222. 231.

† See Journal de Pharmacie, September, 1831. p. 502.

‡ Prevost, Dumas, and Lecanu. Journal de Pharmacie, p. 500.

parts of water are equivalent to 10.302, and a minute fraction, with which I need not encumber the calculation. We have, therefore, 10.302 to deduct from 40.57, the amount of colouring matter with albumen in Dr. Thomson's analysis of cholera crassamentum, which leaves the amount of pure colouring matter in 100 parts of blood, 20.480, because 40.570 of the impure colouring matter, containing 10.302 of albumen, 27.450 of impure colouring matter, must contain of albumen 6.970, which, deducted from 27.450, leaves of pure colouring matter 20.480 per cent.

Again, in his second analysis, No. 4, the water of the crassamentum was 60.66 per cent. Applying the same rule, we find that the serum of this specimen contained, according to Dr. Thomson's statement, p. 353, in every 100 parts—water 81.100, albumen 17.681. The 60.66 parts of water of the crassamentum are therefore equivalent to 13.224, and a minute fraction of albumen. The actual amount of pure colouring matter was therefore 14.651 per cent.

From these data it is manifest that the results of these two analyses, instead of denoting a great excess of colouring matter in the cholera blood, show that in No. 4 it had barely reached the normal standard as fixed by Lecanu, and fell short of the amount afforded in numerous analyses of healthy blood by M. Denis.

The standard of comparison, selected by Dr. Thomson, is unquestionably not deserving of confidence. I use these free terms, because it is not founded on Dr. Thomson's own experiments, but is the result of a single analysis, faultily performed by another individual, and long before Prevost and Dumas pointed out the important fact that the water of the crassamentum existed therein as serum, and that, consequently, a great deduction must be made from the quantity of colouring matter, *if estimated by the evaporation of the washings of the crassamentum.*

Again, another and most important circumstance to be taken into consideration, is the bulk or volume (contradistinguished from the *weight*) of the moist crassamentum, compared with the free or floating serum. If the crassamentum be large and loose, as it invariably is in cholera blood, or in any blood deficient in water or imperfectly coagulated, the crassamentum must retain a proportionately great quantity of serum, and the colouring matter (if analysed by Dr. Thomson's process) be associated with a great quantity of albumen. If, on the other hand, the blood coagulates firmly, as in health or in inflammatory affections, the crassamentum contains infinitely less serum, and the colouring matter will retain a proportionably small quantity of albumen, so that if analysed by the process followed by Dr. Thomson, the contrast between the standard and the cholera colouring matter, will be very striking indeed, and the apparent excess of the latter proportionably augmented.

Another observation will perhaps render this circumstance more readily understood. It will be admitted as a general proposition, that cholera blood is deficient in water. Now if a cholera patient and a rheumatic patient be bled to eight ounces each, the entire blood drawn in the former case will contain much less water than that in the second. But after coagulation, *the crassamentum of the cholera patient will contain more water* by many degrees than that of the firmly-contracted rheumatic clot. Now if an analysis of both be performed by Dr. Thomson's method, the cholera colouring matter may apparently amount to five times that of the rheumatic crassamentum, and yet be really in no excess.

It follows as an inevitable conclusion from these facts, that the sources of fallacy are strictly in proportion to the bulk of the crassamentum, so that by the process followed by Dr. Thomson, a pretty correct estimation of the amount of colouring matter may be made in the well-contracted crassamentum. It is, therefore, no proof of the accuracy of the process, if it affords the normal proportions in the analysis of a standard or healthy specimen of blood. It is in the morbid state, and especially in cholera, that the fallacy must most materially prevail.

To apply these remarks to the elucidation of the pathology of cholera, we find, that as far as these two analyses of Dr. Thomson bear on the question, the *pure* colouring matter is not necessarily increased beyond the normal standard, so that a healthy crassamentum of equal size and weight might contain precisely the same proportion of colouring matter with that of a cholera crassamentum. If, however, the serum be added to each in equal measure, then a given sum of the cholera *blood*, say 100 parts, would contain rather more colouring matter than the normal blood, *in proportion to the deficiency of water of its serum.* This increase, however, would not be very material; for example, the lowest amount of water in cholera blood, is in Dr. Thomson's analysis 67 per cent. The lowest in the *standard* analysis of Denis and Dumas 78, and of Lecanu 77, so that the difference of eleven per cent., supposing the standard amount of colouring matter 13, could not occasion more than an increase to 14.68, or 1.68 per cent.

The preceding train of reasoning is borne out by my own analyses, in which, in two specimens examined in Newcastle and four in London, there were in each 100 parts :—

Pure colouring matter, No. 1	16.25
2	15.6
3	16.43
4	14.7
5	11.28
6	13.6

These results were obtained by an accurate repetition of Lecanu's process, by which the weight of the fibrin, salts, fatty matter, water, and albumen, are first ascertained apart, and then the remainder gave the colouring matter in a pure state. In several other specimens which I examined by another process, namely, by the washing of the clot and precipitation of the albumen from the coloured fluid by the acetate of lead, the quantity varied from 13 to 18 per cent. I do not, however, rely so confidently on this process as on the other, which, if performed with common dexterity, is not susceptible of fallacy.

My experiments, therefore, coincide with Dr. Thomson's, regarding the crassamentum also, when the deductions are made from his numbers in the manner I have shown to be essential. Even did our results differ, the circumstance would still be instructive, as my experiments would then show, that the supposed increase in the quantity of colouring matter was not connected with the *essential* pathological phenomena of the disease, in other words, that it was not *a universal fact*, and, consequently, that it could not be considered as one of the essential events of the disease.

The next part of Dr. Thomson's paper which requires attention, is that in which he alludes to the quantity of fibrin in the cholera blood, and of which he speaks in these terms:—

"The deficiency of fibrin in cholera blood is very remarkable, and struck me very forcibly. The fibrin in the cholera blood No. 1, is only about one-tenth of that in healthy blood; the quantity in the blood No. 4, is nearly four times as great; but it is still only about the third of the quantity which exists in healthy blood."

The quantity of fibrin in 100 parts of blood, according to the vague standard in Dr. Thomson's paper, is rated at 4.45 per cent. In his cholera analysis in No. 1, the fibrin was 0.378; in No. 4 it was 1.348. Had Dr. Thomson consulted Lecanu and Denis, an inspection of their results would have spared him the trouble of speculating on the cause of the deficiency, and of inquiring whether "the increased quantity of colouring matter may be accounted for by any alteration in the fibrin." In point of fact, Dr. Thomson's specimens of cholera blood afforded very nearly the standard quantity of fibrin, as the following table will sufficiently prove. I take 20 examples from Lecanu, and as many from Denis:—

Quantity of Fibrin in Healthy Blood.

In one thousand parts. LECANU.		In one hundred parts. DENIS.	
1	1.360	1	0.25
2	1.690	2	0.25
3	1.800	3	0.27
4	1.870	4	0.40
5	2.140	5	0.30
6	2.200	6	0.25
7	2.210	7	0.27
8	2.410	8	0.25
9	2.140	9	0.27
10	2.578	10	0.27
11	2.720	11	0.25
12	3.130	12	0.28
13	3.240	13	0.21
14	3.565	14	0.31
15	3.730	15	0.27
16	3.904	16	0.35
17	4 250	17	0.35
18	4.630	18	0.22
19	5.000	19	0.23
20	5.050	20	0.23

Compare these with Dr. Thomson's analysis of cholera blood:—

	No. 1.	No. 4.
Fibrin	0.378	1.345 per cent.

The cause of the difference is simple. The standard of comparison selected by Dr. Thomson, gives 44.5 per 1000, as the proportion of fibrin! an error dependent on the imperfect mode of drying resorted to by the experimentalist. Chevreul has satisfactorily shown that fibrin, when apparently dry, still retains *four-fifths* of its weight of water. Accordingly, in Dr. Thomson's own analysis, where I presume the fibrin was much more perfectly dried, normal results were very closely approached.*

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* There still remains a topic in Dr. Thomson's paper on which I would minutely enter, had not my remarks already extended beyond the limits suited to a weekly journal. I allude to the quantity of albumen in cholera *blood* generally, which, in Dr. Thomson's tables, is far below the natural standard. Now, in the *serum* of the same blood, Dr. Thomson has shown that the albumen was double the quantity of the normal state, being 15 per cent in two specimens, and 17 per cent in three others, while the standard, according to Denis, Marcet, Berzelius, and Lecanu, never exceeds 8 per cent. Had due allowance been made by Dr. Thomson for the large proportion of albumen which I have shown to be included in his colouring matter, it would have been found that the albumen, instead of being deficient, was considerably in excess. In No. 1, for example, the amount of albumen in the colouring matter was 6.970, which, added to the albumen in the table, makes 11.826 per cent. In No. 4, again, the albumen amounted to 8.509, which, added to that in the table, makes 14.814 in 100 parts.

The following deductions evidently proceed from the several facts and observations I have brought forward.

1. That the universality of the diminution of the quantity of water in cholera blood is extensively and satisfactorily confirmed.

2. That a moderate diminution took place in the salts of the serum in the individual instance Dr. Thomson examined.

3. That Dr. Thomson's *calculation* of the quantity of salts in other cases is inadmissible because the quantities vary in every case.

4. That the apparent excess of colouring matter in the blood is chiefly dependent on the mixture of albumen, owing to the defective process employed in the analysis.

5. That this admixture of albumen is especially liable to take place in examinations of cholera blood, owing to the bulk of the crassamentum, and the quantity of serum it retains.

6. That nevertheless the same process may give correct results on standard blood, owing to the comparatively minute quantity of serum retained by the crassamentum. A great apparent difference, therefore, in the results of a standard analysis, and of cholera analysis, performed in the same manner and by the same individual, is not a proof of correctness in the estimations of the ingredients in cholera blood.

7. That the quantities of fibrin and albumen obtained by Dr. Thomson were not below the natural standard.

Before I conclude, I wish to add a supplementary word respecting another branch of our chemical knowledge of this disease. I allude to the peculiar evacuations. Since the publication of my results in THE LANCET of December, 1831, and of my report addressed to the Board of Health, the *alkaline* character of the evacuations has been ascertained in innumerable cases. I have, however, met one case myself, and I have heard of a second, in which the alvine evacuations possessed an *acid* reaction. In several dissections which I superintended in London, the contents of the stomach were also generally acid. The cause of this is sufficiently obvious. The stomach and small intestines naturally secreting a free acid, and in considerable quantities, the serous and alkaline exhalation of cholera is insufficient to neutralize the natural acid in these situations. If the acid secretion has again been unusually profuse before the attack of cholera, all the alkali of the blood will be insufficient to neutralize it, and consequently the evacuations must retain acid properties. The exception, therefore, does not invalidate the test by which cholera diarrhœa may be distinguished from ordinary bowel affections.

I have also to notice the interesting fact of Dr. Christison having discovered urea in the serous exhalation on the brain of a person deceased of cholera in Edinburgh. I wait with great impatience the publication of his results in this inquiry. I may venture to predict, that he will leave nothing more to be learned of the chemical pathology of this curious and deeply-interesting disease.

COMPOSITION OF HEALTHY AND OF CHOLERA BLOOD.

LETTER FROM DR. CLANNY.

To the Editor of THE LANCET.

SIR,—I have read the critique which appeared in THE LANCET on the 5th of this month, and though I could enter most freely and most satisfactorily upon the points animadverted upon therein, I shall forbear, in my present position, from occupying valuable time by any remarks except the following, which being quite to the point as to my chemical investigations of healthy and of cholera blood, I trust you will, with your usual candour, give an early insertion of them in the pages of your valuable and independent journal.

You are perhaps aware, Mr. Editor, that Professor Thomson of Glasgow has favoured us with analyses of healthy and of cholera blood, an account of which appeared on the first of this month in the *Philosophical Magazine*. I shall submit analyses made by myself and by Professor Thomson to your readers, and leave the chemists and medical practitioners to form their opinion of the value of my humble services in the investigation of this new and most formidable disease.

	Healthy Blood.		Cholera Blood, No. 1.		Cholera Blood, No. 2.	
	Dr. Clanny.	Professor Thomson.	Dr. Clanny.	Professor Thomson.	Dr. Clanny.	Professor Thomson.
Water	75.6	78.4	72.0	66.1	64.4	67.9
Albumen	12.1	8.5	6.1	4.9	3.1	6.3
Fibrin	1.8	4.4	.6	.4	.6	1.3
Colouring matter	9.1	7.4	21.0	27.1	31.9	23.2
Salts............	1.4	1.3	.3	1.2	.0	1.3

Albumen.—Professor Thomson states, in his analyses at page 355 of the *Philosophical Magazine*, the quantity of albumen wanting to make up the quantity in healthy blood to be, in No. 1, 3.6, and in No. 2, 2.2, but seems to conjecture that part might be absorbed by the cloth through which it was filtered. With me, at No. 1, .6, and in No. 4, .9, to make up the quantity in healthy blood.

Fibrin.—Professor Thomson states the fibrin in cholera blood No. 1 to be about one-tenth of that of healthy blood, the quantity of blood No. 4 to be nearly four times as great, yet only one-third of the quantity that exists in healthy blood. With me, No. 1 and No. 4, to contain only one-third of the quantity that I found in healthy blood.

Colouring Matter.—Professor Thomson, at page 357, states "that the great excess of colouring matter in cholera blood is no less remarkable than the deficiency of fibrin. If we make allowance for the albumen and salts still contained in the colouring matter, and take a mean of the quantity of colouring matter in Nos. 1 and 4, we shall find it to be little short of four times the quantity of colouring matter in healthy blood." With me, No. 1 to be 2½ times, and No. 4 nearly 4 times the quantity of colouring matter, &c., in healthy blood.

Salts.—Professor Thomson finds the salts in healthy and cholera blood to be nearly equal. I find the salts in healthy blood to be nearly 4½ more than in cholera blood No. 1; and in No. 4 the salts are not present. Compare this with the solid contents of the dejected fluids in cholera cases.

It will readily be observed, that in the above statement of results by Professor Thomson and myself, there is some difference in the numbers, which must be attributed to the nature of the cases from which the blood was taken; besides, I dry the albumen at 160°, and press the fibrin, and dry it in the atmosphere, so that these substances may be estimated in a more accurate manner than formerly practised, as I have mentioned in the *Edinburgh Medical Journal*, from finding, that by considerable or long-continued heat, I can, in the former method dry * in the albumen * one half.

Taking these things into consideration, I am satisfied that the numbers which Professor Thomson and I have given, are as near as may be, and are satisfactorily corroborative as regards the nature of the disease in question.

I need scarcely remark, that in the above tables I add the *free carbon* to the colouring matter, and the reason why I adhere to the plan in my work, of giving these substances in a separate form, as observed in my tables, is because I consider that they exist in the blood in that state, and I have no doubt but all unprejudiced persons will agree with me on that point.

My work is now before an enlightened community, and will be judged according to its merits, and not by what any individual may now say upon my method of analysis, or upon the accuracy with which my experiments were performed, except the contrary be proved by experiments.

After all that has been written in the above-mentioned critique, I beg to remark that my method of analysis and accuracy of experimenting in the investigation of the free carbon of the blood are unimpeachable, and that the hint which I gave towards the conclusion of my work, as to the gases contained in the colouring matter of the blood was only an after thought, and that they were not considered as forming gaseous constituents of healthy or cholera blood, more than the cinder which I found in the platinum vessel along with the free carbon, and cannot be estimated as of any importance as regards the nature of the blood circulating in the veins of healthy persons, or of individuals affected with the cholera.

No person who has performed experiments upon blood but must be sensible of the disagreeable smell emitted when the colouring matter is submitted in a crucible or platinum box to a high temperature, and in cholera cases this empyreuma was abundant and highly offensive. This remark does not apply to experiments with the blood of the horse, as I have lately found in prosecuting my experiments upon the blood of that animal; but of this more hereafter.

I cannot enter further upon this critique except I become personal, and may perhaps be induced to cut deeper in a certain quarter than for my own sake and for the sake of your readers I ought to do. Though I live at a considerable distance from London, I know more facts, as connected with the subject of cholera, than some persons suppose I can possibly do.

I never saw the constituents of blood according to Michaelis, being perfectly satisfied with those of Berzelius, Brande, Thomson, and my own.

The two latter I have given above. In the critique the carbon contained in carburetted hydrogen gas, and carbonic acid, which I mention, appear to be forgotten! So much for*

As to the severe animadversions on free carbon of blood, allow me to say that we are compelled to admit its existence from what we know and acknowledge as connected with the process of respiration, independently of the changes induced upon the blood, from impeded respiration generally, and my experiments particularly. I remain, &c.,

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* Blanks in the copy.—Ed.

* The seal has here obliterated a word.—Ed.

W. Reid Clanny.

Sunderland, May 12, 1832.

LITHOTRIPSY.

To the Editor of The Lancet.

Sir,—I am requested by Baron Heurteloup to transmit for insertion, in your valuable journal, a digest of his memoir on lithotripsy lately read before the "Institut de France." The improvements which are detailed cannot fail, I think, to interest your readers, as they are so important as to give a new aspect to the science. I have taken the liberty of incorporating with the text the voluminous notes of the Baron, and I beg to state that if any of the terms employed should seem obscure, a full explanation of them will be found in a work lately published by him on the "Principles of Lithotrity." The efficiency of the new instrument was demonstrated on artificial calculi, introduced into the bladder of subjects in the presence of the commissioners appointed by the "Institut" to examine into its merits. M. Dupuytren was the chairman of these gentlemen, and he, as well as his colleagues, after comparing the operation of the instrument with that of the perce-pierre in the hands of another surgeon, remained convinced that the vibration of the former is much less than that of the latter. The Baron states that accident first suggested to him the idea of his "Percuteur." Having, in one instance, forgotten his drill-bow, his only resource was to strike the drill with a hammer; and in this manner he succeeded in breaking a calculus of moderate size. He afterwards frequently resorted to the same means, especially in breaking the coque or shell after the operation of the "evideur à forceps." I have the honour to be, &c.

D. O. Edwards.

9, Duke-street, Westminster,
May 19th, 1832.

ON THE NEW METHOD OF DESTROYING

VESICAL CALCULI,

BY MEANS OF PERCUSSION.

WITH EIGHT SUCCESSFUL CASES.

From a Memoir

By Baron Heurteloup.

The art of removing the stone from the urinary bladder without incision, hitherto designated by the term *Lithotrity*, may, if we reflect on the time which has elapsed since its first invention, be considered as of recent date; but if we consider the labour which has been bestowed on its cultivation, and the astonishing improvements it has undergone, we shall probably be disposed to allow that it is better entitled to the honours of antiquity than many others.

The modifications which have been admitted in this operation make the original name no longer applicable, for its derivation from λιθος, and τετράω, or τερέω, imply that its action is to *bore*, not to *break*. The term *lithotripsy*, from λίθος and τριϐω τριψις, will, I think, be acknowledged as more appropriate to the nature of the operation. Certain writers, having a cacoethes for coining words, have had no difficulty in forming grandiloquent names for the different stages of the lithontriptic process; such as *lithocoilancy*, from λιθος and κοιλαινω, and άοιλος. Other equally barbarous terms have been manufactured from λιθος and θλαω, as well as λιθος and κοπτω. But these tasks may properly be resigned to Dr. Pangloss. The word lithotrity, however, does not now apply with any exactness to the general scope of the process, and may perhaps be advantageously supplanted by the term "*lithotripsy*." Having had the good fortune of contributing a large part of the materials with which this art has been raised to its present state of perfection, I am anxious to deposit, in the archives of the Institut, this further fruit of my labours, which I hope will afford a convincing proof that perseverance will occasionally enable us to attain results such as could not be anticipated by any person influenced by prejudices.

Hardly are we recovered from the surprise excited by the discovery of a means of wearing away the stone from the bladder by slow degrees, than the possibility of rapidly destroying these bodies by percussion, is practically demonstrated.

Had our first exertions not been shackled by long-existing prejudices, the method of percussion would probably have first suggested itself as the most natural means. It will readily be conceived that the more minute the particles into which a calculus is reduced, the more easily will they be evacuated, and the more prompt will consequently be the patient's relief. The means of breaking down calculi, immediately and completely, into powder, have steadfastly been sought for, but all attempts towards this end have hitherto been abortive.

The idea of effecting an *immediate and complete pulverization* was of necessity given up, and a more tardy process of fracture was next thought of.

There are *three* modes in which fractures may be produced. The first and most simple of these is, to place the stone upon a fixed plane, and to strike it with a moveable plane, ex. gr., a hammer and an anvil.

The second in point of rapidity and simplicity is to place the stone between long moveable planes, and thereby compress and crush it.

The third mode, and the most tedious and ineffective, is the gradual consumption of the stone by a process of attrition.

I think that any person when called upon to break a stone into powder on the outside of the bladder, would naturally have recourse *first* to percussion, as the most efficacious and rapid way; *then* to crushing; and *last of all* to progressive trituration.

Such is the course which the surgeon would naturally have pursued, but the power of established theories, operated to warp his judgment. In fact, nature having placed limits to the size of the instruments to be employed, the practicability of *violently* breaking calculi in the interior of the bladder was not likely at first to suggest itself, for it would appear next to impossible to invest such instruments of contracted bulk with sufficient resistance to allow of the powerful actions of *percussion* and *crushing*.

The union of *power*, *lightness*, and *delicacy*, requisite for seizing calculi in the bladder might naturally appear a desideratum not easily to be accomplished. Concluding, therefore, that the combination of *great force* and delicacy of operation was perfectly impracticable, the inventor was naturally led to the original lithotritic means.

Fully impressed that a sudden collision of forces could not safely occur in this organ, and that the process of destruction must be slow, it was thought necessary to construct one instrument to hold the stone, and another to act upon it. And if, before the construction of his set of instruments, the inventor thought the powerful means alluded to ineligible, he was much more likely to think so afterwards, for the greater the number of pieces in a tube of given dimensions, the less is the strength of each, and the greater the feebleness of the entire apparatus.

Having once begun his course, the lithotritist soon meets with incidents that show him, even whilst using the drill, that the operation might often be much accelerated by crushing small stones or fragments with the claws of the instruments. With the instruments employed to produce excavation or perforation, percussion was made, and often succeeded in breaking the calculus. These incidental facts, of which experience soon supplied a great number, led the surgeon to perceive that force would be a valuable acquisition, since the diameter of the instrument allowed of its development, and hence the more expeditious plan of crushing had its birth.

The application of this system gave to lithontriptic agents the important property of making the same parts of the instrument effect both the seizure and the fracture. This improvement also, by diminishing the number of pieces in the tube, rendered it less complicate, and increased its power.

The abandonment of the practice of using the hand of an assistant in the operation, and substituting for it the point d'appui of the rectangle-bed, was a circumstance favourable to the application of more powerful instruments. This enabled the surgeon to effect his purpose with great steadiness, and contributed to place the surgeon and the patient in the best possible position with respect to each other.

The agency of such a coarse and powerful instrument as a hammer, would appear incompatible with the delicacy of manœuvring necessary in the interior of the living bladder, and it might be supposed that many trials would be made previous to a successful adoption of the system of percussion.

It was indeed after a train of speculations, such as I have described, that I was led to contemplate the application of such an instrument to the purpose.

After having had the good fortune to devise and bring into practice the apparatus called "*evideur à forceps*," which accelerated the old process, by adding the principle of excavating; after having contrived an instrument which has proved effectual in the *crushing* of calculi, and denominated "*brise-coque*;" after having ascertained the best respective position, both for operator and the patient, and facilitated the movement of the latter by means of the rectangle-bed; after having invented a secure fulcrum for the instrument, while in the bladder, in the *point fixé*, attached to the foot of the bed; after finding all these designs approved of, and imitated, by highly respectable practitioners, and after proving their efficiency by numerous successful operations; and after having my labours repeatedly crowned with the high sanction of the Institut, I have once more the happiness of presenting for your examination a novel apparatus, in which the agency of percussion is resorted to.

I have called the new instrument the "*percuteur courbe à marteau*." It is made of steel, and is about fourteen inches long. For the sake of convenient description, it may be distinguished into that part which is contained in the urethra and bladder, and that which is external. The former has the appearance of a large sound, straight for about eight inches of its length, and presenting at its extremity a curve equivalent to the quadrant of a circle of which the radius is *two* inches. This curved extremity separates into two portions by a section at right angles with the straight part of the sound. The straight part is composed of three pieces, two of which are lateral, and the third internal; the former are a continuation of the external portions of the curve, the latter of the internal or anterior part. The terms external and internal relate here to the convexity and concavity of the curve. Now since the true external portions are firmly secured in a square piece of steel forming the "*armure*" of the instrument, and since the internal portion is moveable, it may be drawn forward; a hiatus is thus formed like the beak of a bird by which the calculus is grasped. The pulverization of the stone is effected with the aid of a hammer. The outer part of the instrument is smartly struck with the hammer, carried without loss of power through the steel rod, and imparted to the foreign body sought to be destroyed.

The mode of proceeding in percussion is this:—I place the "armure" of this instrument in the point fixé, and make it secure by means of a wedge-shaped piece of steel. With this provision, powerful blows may be dealt on the external part of the instrument, without exciting any vibration in that part which is within the bladder.

By means of this instrument a vesical calculus, however hard, is broken to pieces as soon as seized, indeed it is difficult to say which is most remarkable—its facility of prehension, or energy of comminution—and, strange to say, the size and hardness of a stone may for the future be considered only as trifling obstacles to the success of an operation. The dispatch which may now be ensured in the destruction of stone is a most important advantage, and forms a striking point of superiority in the new apparatus. The history of a few cases will, I think, contribute to establish the character of the "operation by percussion."

Mr. K., of Liverpool, 68 years of age, after suffering the torments of stone for *three* years, consulted me. Finding a small calculus, I thought it advisable to employ the *perce pierre* of Messrs. Leroy and Civiale. I was prevented seizing the stone with this instrument by "an anomalous fleshy prominence," which, by protruding between the claws of the instrument during its depression towards the bas-fond of the bladder, was necessarily pinched. After two fruitless attempts, I was convinced that this instrument could not be employed without lacerating the fungous or hernial mass which was protruded in my path. This excrescence I considered to be either a fibrous mass resulting from arectal fistula, which opened externally by a sinus terminating near the anus, or from the enlargement of the third lobe of the prostate gland. I employed the percuteur à marteau, and in *four* applications of three minutes each, I enabled the patient to void the detritus of a calculus which must have weighed *four* drachms, and which was composed at its nucleus of lithic acid, and at its exterior of the mixed phosphates.

Mr. C., of London, 48 years of age, suffered with symptoms of stone for *four* years. I ascertained with a sound the presence of *two* oval-shaped calculi, smooth, and composed of lithic acid, apparently sixteen or eighteen lines in diameter. The "percuteur courbe" was employed, and in six applications, of from *three* to *four* minutes each, the patient was completely relieved.

Mr. Rogers, 64 years of age, residing at Limerick, after having had the stone about a year, came to London. I sounded him and felt a calculus, which I estimated to be 16 or 18 lines in diameter; it was of a flattened oval shape, and moved freely in the bladder. In four applications of the "percuteur courbe," lasting the usual time, the whole of the stone was removed.

Mr. G., 62 years of age, had had stone for eight or ten years. He requested a surgeon in London to perform lithotrity upon him. The surgeon consented, and made use of the "perce pierre," but the stone was not laid hold of, and the patient, tired with fruitless attempts, consulted me. I waited a month, that the bladder might recover its former tranquillity, and that the catarrh of the organ might cease. I then employed the "percuteur courbe," and in *six* applications of the usual duration, a smooth, oval-shaped, lithic acid calculus of ten lines diameter was destroyed and brought away.

Mr. O., 63 years old, residing in Sussex, about 60 miles from London, had experienced for upwards of four years all the sufferings attendant upon stone in the bladder. I was called into consultation by Mr. Dodd and Mr. Caudy, his surgeons. I found a smooth, oval, uric-acid calculus of from 20 to 24 lines in diameter. In *seven* applications of the percuteur, and *two* of the brise-coque, Mr. O. was restored to perfect health.

Mr. Goodwin, of London, ætat. 66, was afflicted with stone *three* years ago, and underwent lithotomy, which was successfully performed by Mr. Brodie. A year after the operation he had a return of his former symptoms. He then resolved to have recourse to lithotripsy. He consulted a surgeon in London who undertakes this branch of surgery. This gentleman proceeded to operate with the perce-pierre, but he did not succeed. The stone was not caught, although *three* several trials, of three quarters of an hour's duration, were made. I was then consulted, and Mr. Goodwin was relieved of his stone in four applications of the "percuteur."

Mr. Wells, 48 years of age, residing about 70 miles from town, had suffered from the stone for four years. When he came to me I found a large lithic-acid calculus in his bladder, about 24 lines in diameter. Notwithstanding a considerable catarrh of this organ, I thought it right to commence operating; and in *five* applications of the "percuteur," and one of the brise-coque, I was enabled to get quite rid of the stone.

Mr. F., the president of the College of Carlow, 62 years old, had suffered from lithiasis for eight years. He consulted Mr. Crampton, surgeon-general for Ireland, who was polite enough to recommend him to my care. I sounded the patient, and discovered a calculus of very large dimensions, which was, however, tolerably moveable in the bladder; it seemed of an oval shape rather elongated and situated transversely. The sensibility of the patient was very acute. In *seven* applications this stone was completely destroyed and evacuated.

These operations were performed in the presence of Sir Astley Cooper, Sir Anthony Carlisle, Messrs. Brodie, Copeland, Samuel Cooper, Green, Hamilton, C. Hutchison, Aston Key, North, Swann, Travers, Vincent, &c. &c. &c. I was also fortunate enough to be enabled to operate with the "percuteur," in the presence of M. Magendie.

If the details of these cases are well looked into and considered, and the instrument itself carefully examined, and a fair trial given to it, the following conclusions will be established.

1. That the most powerful means, viz. percussion, that could possibly be devised for pulverizing calculi, under any circumstances, have been rendered safely applicable in the interior of the bladder.

2. Lithotripsy cannot henceforth be deemed inapplicable to large, flat, and oval calculi. As trophies of the prowess of the "percuteur," I beg to present to the "Institut" the fragments of stone voided by Mr. Wells; they fill a round box of 21 lines diameter, and 11 in breadth. I also present the model of Mr. F—'s stone, which I obtained in the following manner. I succeeded in filling exactly a cylindrical tin box, with the fragments evacuated, and afterwards formed a wax cast of the interior of this box. This cast I modelled into an oval shape. I had ascertained by my instruments the diameter of the stone, and knowing this, there was not much difficulty in approximating to the size and shape. This cast measures five inches, all but one line, in its smallest circumference. If now we reflect, that in this case the instrument was applied *seven* times, each application lasting about *three* minutes, we may say that the aggregate duration of the entire operation was only *twenty-one* or *twenty-two* minutes; and it is well known that lithotomy, in cases of large stones, has often continued longer.

3. The continuance of this operation need now never exceed a few minutes. It is only requisite to examine the "percuteur" to feel convinced that it cannot be otherwise, for it may be opened and closed 160 times in a minute; and its power will be seen to be irresistible.

4. Cases in which straight instruments cannot be introduced into the bladder, need no longer be considered out of the province of lithotripsy.

5. Frequent renewals of the operation are now prevented, even in cases of voluminous calculi, as is demonstrated by the history of previous cases.

6. Disease of the bladder is a comparatively less formidable circumstance, for the cases of Messrs. Rogers, Goodwin, G— –, and C——, with severe affections of that organ, were relieved as promptly nearly as those of the most healthy persons.

7. The chance of disease of the bladder succeeding the lithotriptric process are in the direct ratio of the time consumed, and are consequently diminished by the greater expedition of the percutient method.

8. Those cases which were peculiarly difficult on account of the stone not being situated in the axis of the *straight* instrument, are now easily managed, as is shown by the cases of Messrs. Goodwin and F——, in which the perce-pierre was useless for that reason, and the "percuteur" successful.

9. From the case of Mr. R——, where unsuccessful attempts were made with the perce-pierre, we may conclude that an enlargement of the third lobe of the prostate is not an insurmountable obstacle, wherever the percuteur may be had recourse to.

10. The influence of the improved method is also felt in cases where several small stones exist, for their destruction is speedily effected, even after the failure of the perce-pierre.

11. Even the ill effect of contraction is in a great degree obviated, on account of the small space which is adequate to the manœuvring of the percuteur; and we can make the bladder yield without danger of injuring it.

12. The evacuation of the bladder too is much facilitated, for the shape of the fragments resulting from the percuteur, is more favourable for their expulsion.

13. Less pain is inflicted, and less danger incurred by the use of this instrument than by the former ones. During the operation the patient is completely free from any painful sensation, and when the instrument is fixed immoveably on the proper fulcrum, not the slightest vibration is produced by the blows of the hammer.

These are the conclusions to which this newly-invented instrument, and the system it represents, give rise. They may, perhaps, be deemed of sufficient importance to deserve the attention of the "Institut," who have already expressed an interest in the invention of a new method of destroying urinary calculi. I believe this instrument has no resemblance to any hitherto constructed, and I trust it will be looked upon as an important step in the improvement of the science.

ON THE

CAUSES, SYMPTOMS, AND MORBID ANATOMY, OF SIMPLE AND MALIGNANT STRICTURE OF THE RECTUM.

BY

FREDERICK SALMON,

Senior Surgeon to the General Dispensary, Aldersgate Street.

(AS DESCRIBED IN A LECTURE DELIVERED AT THE INSTITUTION, APRIL, 1832.)

GENTLEMEN,—When I had the pleasure of meeting you last, I exhibited to your notice an example of what I termed *simple* stricture of the sigmoid flexure of the colon, and endeavoured,—by contrasting the early symptoms of the case, and those which occur during its progress, with such as usually accompany malignant disease,—to show that it was not one of the latter character. By comparing the morbid anatomy of the two descriptions of disease, I also illustrated the same position.

Now the like circumstances will occur in the rectum, and if we look to the morbid anatomy of stricture of this part, which is the subject I shall more particularly allude to on the present occasion, we shall find that in its early stages there is very little deviation from the healthy appearance. This is easy of explanation when we consider, that most descriptions of stricture are, in all probability (at their commencement), simply spasmodic affections dependent upon some particular circumstance giving rise to local irritation. Now this spasmodic action necessarily disappears with the vital powers, hence you may fail in discovering any disease after death, although the most unequivocal symptoms of stricture appeared during the patient's existence.

The first change which takes place in the rectum may be well elucidated, by referring to a fact with which most of you must be familiar. In examining the bodies of persons who have died of diseased lungs, it is not an uncommon circumstance to find the lower part of the colon and the rectum contracted to their smallest natural extent, and which may be mistaken for disease. The parts, however, are easily dilated, by inflating them with air, or distending them with water, proving, consequently, that there is no organic alteration.

The first alteration that takes place is in the mucous coat of the bowel, which is thrown into large folds, as is shown in this preparation. This, however, is no disease of the part, but results simply from the fact of the mucous coat not being indued with contractile power equal to that which the muscular possesses. Thus the latter naturally assumes the appearance you here see represented. As disease advances, irritation is set up in the bowel, in consequence of which, deposition ensues, not in the muscular coat, nor in the inner tunic, as some writers have represented, but in the cellular tissue which connects these two parts. This fact is incontestably proved in the preparation I now show you. Here is, as you perceive, an enormous extent of thickening, so that the canal of the rectum at six inches from the anus is nearly obliterated; even water will scarcely pass through it; yet there is no disease of the inner coat of the bowel. The individual from whose body this preparation was taken, did not die of the disease I now show you, bad as it is, but from an affection of the lungs. Indeed the disease of the rectum was not discovered until after the patient's death.

Such is the *ordinary* appearance of simple stricture; but at other times inflammation is set up in the rectum, and ulceration takes place in various parts of the mucous surface, as is here shown. In this instance there is contraction and deposition, together with destruction of a portion of the mucous coat of the intestine. In extreme cases, adhesions, more or less extensive, according to the period of the existence of the stricture, form between the rectum and uterus, or vagina, or between the bladder and the rectum. Here are two preparations admirably illustrating these facts. Sometimes it unfortunately happens, if the case be not attended to before it is much advanced, that ulceration takes place of that part of the rectum and the vagina, which are in contiguity, and thus a communication is formed between the anterior and posterior passages, as is here exhibited. It is really surprising how serious an extent of disease will occur in the rectum without vitally affecting the individual. Communications between the bladder and the rectum in simple stricture are not common, but I have seen them more than once; and I am acquainted with an individual who for more than two years has passed a considerable portion of his fæces in this manner. They always come away in a liquid state before he voids his urine. The suffering he endures at these periods is indescribable, yet, from his general health and appearance, you would not consider that he laboured under a disease of any moment.*

It sometimes, though *very rarely*, happens, that you have lymph thrown out, forming bands in the rectum, and if you do not carefully examine the bowel, you may mistake these for stricture of the gut itself. I have a patient now under my care who has been on the books of this charity for four or five years, in whom the septa at the lower part of the rectum are extremely numerous; and I have been endeavouring for some time to induce her to allow me to divide them, but to no purpose; in her case, upon introducing your finger, you discover the septa extending from the anterior to the posterior parts of the rectum leaving a passage above and below, the largest of which channels will now barely admit of the introduction of a tobacco pipe. You may say that these are very extraordinary cases, and I do not deny that they are so, but still they are facts which I know to be true, and they verify my position as to the extent to which disease will go on without producing any great derangement of the general health.

Now these are the usual appearances when the disease is situated high up, beyond the reach of the finger, or, I would say, above the last curve of the rectum. I have shown you that there is little ulceration of the mucous coat when the stricture is high up; but as it approaches the anus, you almost invariably find the reverse. The preparation I now show you, which was taken from the body of a man who accompanied the expedition to the North Pole, illustrates this fact; and I believe the disease in this individual to have arisen solely from his frequently sitting upon ice, the extreme cold producing spasmodic action of the circular fibres of the rectum, which, as you know, are more prevalent near the anus than in any other part of the bowel. It is for this reason that you will generally find circular contractions within reach of the finger. This circumstance has led to an error that has sacrificed many a valuable life. Medical men, finding circular stricture within reach of the finger, have expected that they should meet with the same description of contraction in the upper parts of the bowel. Such is not the case. At this situation the obstruction is produced by a partial obliteration of the cavity, from deposition between the coats in the direction of the longitudinal fibres. As the stricture near to the anus progresses, irritation is set up in the soft parts around the entrance of the bowel, and small abscesses, forming fistulæ, follow, as is here shown. Here is another instance of circular contraction within reach of the finger, wherein the cavity of the intestine is almost obliterated. I had not an opportunity of seeing the patient from whom this preparation was taken during his lifetime, but I attended, in the preceding instance, from its commencement, as likewise in the case I now exhibit, in which the cavity of the bowel will not, you perceive, admit a common tobacco pipe.

Now no one of the individuals from whom these morbid parts were taken, died from the disease of the rectum; and it is a fact that patients are very seldom carried off by simple stricture, which circumstance, again, has in all probability led to the belief, that diseases of the rectum are much less common than they in reality are. I have noticed a fact which occurred in the cases of these individuals many, many times, and that is, that persons do not die from disease of the intestine, but from consumption. Here I will take the opportunity of setting myself right with many members of the profession, for whom I entertain the sincerest regard, who have attributed to me opinions I never advanced, to wit, that disease of the rectum *causes* phthisis. I have never stated any such thing. If you will peruse what I have written on the subject, you will find, that all I say is, that I have seen the two diseases occur in conjunction, and that the affection of the rectum precedes the disease of the lungs. On this latter point I am at issue with those who state that fistula is the *consequence* of disease of the lungs. I believe the reverse to happen, and I say so from experience, for I have known repeated instances in which, upon the removal of the fistula that occurred as a consequence of circular contraction of the rectum, structural disease of the lungs has supervened. But however this may be, when the disease is near the anus, you will generally find fistula and external piles, but if the obstruction be beyond the reach of the finger, you will more commonly have prolapsus. This fact will be clearly accounted for when you reflect, that if the constriction be near the orifice, there cannot be sufficient of the mucous membrane below the stricture to form a prolapsus.

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* A case was here mentioned in which an artificial anus had been for many years formed in the groin, the particulars of which are suppressed in this report of the lecture.

Now such are the appearances usually found in what I would denominate *simple stricture*; but if we go to the morbid anatomy of *malignant* contraction of the rectum, we have such a state of things as is really astonishing. Here is a specimen of this description of disease, in which the whole of the lower, and a great portion of the second curve of the intestine, are destroyed, together with a part of the bladder. Here is another preparation, exhibiting carcinomatous disease of the rectum, where the entire sphincter, together with a great portion of the extremity of the bowel, is eaten away. It is the character of malignant disease, that it will destroy any part with which it comes in contact. You have scrofulous disease of the rectum, though I believe, as far as experience enables me to judge, this description of contraction is very rare. The specimen I show you, illustrating this fact, was taken from the body of a patient who recently died, and whom I first saw about nine months since. Upon examining the rectum with the finger at that period, I found the extremity of the bowel hardened and tuberculated. Looking further, I discovered the obstruction two inches from the orifice. He was then passing blood and matter with his motions, the inguinal glands in both groins were greatly enlarged, and his general health altogether was much disturbed. He ultimately died of scrofulous disease of the lungs. In this instance you see the mucous membrane at the anus, and for three or four inches up, is totally destroyed, and that it is ulcerating in small patches in the upper curves of the rectum.

You read of *syphilitic contraction* of the bowel, but I have no example of it to show you, neither have I ever seen an instance of the disease. I have seen many cases, especially in women of the town, where there was a vast mass of enlarged glands around the external verge of the anus; this affection is by no means an uncommon one, but I have never seen anything like syphilitic disease of the intestine itself.

So much for the morbid anatomy of simple and malignant contraction of the rectum. Now I do not place this before you as being anything like a complete elucidation of the subject; a great deal more might be said respecting it, yet I think I have said sufficient to enable you to distinguish between that description of contraction which I term simple, and malignant disease of the intestine.

I will now say a few words concerning the *situation* of stricture.

It is a commonly-received but very erroneous opinion, that stricture of the rectum is always within reach of the finger, and, certainly, malignant contraction, as well as the most difficult forms of simple stricture (I allude to circular contraction in particular), usually occurs at the extremity of the bowel. But if you refer to the anatomy of the part, you will readily perceive why stricture may occur in any part of the intestine. By the model of the pelvic viscera I now show you, you perceive that the rectum is divided into three curvatures. Here you have the first, here the second, and here the third curvature of the bowel.* Now when the bowels are habitually constipated, fæces are apt to accumulate at the angles of these curvatures, and to set up irritation, giving rise to chronic inflammation, from which, deposition in the cellular tissue connecting the mucous and muscular coats, results, producing a permanent thickening. Hence obstructions are more frequently found at these than at any other portions of the rectum. Here are various examples of the stricture occurring in different parts of the bowel, but all beyond the reach of the finger, and some to such an extent as nearly to obliterate its canal. Now malignant disease commences, in all probability, in the mucous glands, which, as you know, are far more prevalent near the anus than in any other part of the rectum; hence, it is this description of contraction that is so frequently found near the orifice. It should seem as if nature increased the glandular structure near this part, to guard against local irritation from any accumulation of indurated fæces, by lubricating the bowel and softening its contents.

Next, as to the causes of stricture.

If strictures, as I have told you, are, in their commencement, the result simply of inordinate action of the muscular coat of the bowel, it follows, as a necessary consequence, that they may be produced by any circumstance giving rise to undue action of this part, and, if I were to be asked what habit I consider has, more than any other, induced contraction of the rectum, I should reply, the frequent use, or rather abuse of purgative medicines. Indigestion, acrimonious substances lodged in the bowel, habitual costiveness, or, in fact, any circumstance which promotes irritation in the canal, unquestionably may give rise to the disease. It may be produced from some peculiar formation in the intestine, which is sometimes unusually long, so that there are several lateral curvatures in the upper part of the bowel, in which the feculent matter accumulates and causes irritation. It may originate in tumours, as, for example, polypi; in a mass of piles; or in any preternatural growth in the bowel. A common cause of stricture will be found in some particular conditions of the sphincter muscles. You all know that the office of the external sphincter is to retain the contents of the rectum; but if the power of this muscle be too great, it prevents the easy evacuation of the fæces; hence it requires excessive straining to empty the rectum; this sets up the irritation, and causes violent contraction of the bowel, and, ultimately, permanent stricture. This condition of the sphincter varies. Sometimes you will find a complete separation between the external and internal sphincter, so that there is nothing to relieve the extremity of the rectum, by which the difficulty of passing the motions is materially increased. This peculiar formation of the rectum has been often mistaken for stricture, and, certainly, if you do not make a careful examination, you may readily fall into this error; it is, however, no disease of the bowel, but arises simply from the internal sphincter not uniting, as it usually does, with the external. At other times you find the *external* sphincter remarkably broad and thick. There is a great similarity between this condition of the muscle, as giving rise to stricture of the rectum, and that which occurs in contraction of the orifice of the urethra. You will sometimes find the meatus externus extremely narrow, consequently offering a powerful opposition to the expulsion of the urine, and just the same effect happens in the rectum. This fact of contraction of the orifice of the urethra is not generally noticed by writers, yet it is of great practical importance, and one which you will very commonly find.

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* Here was exhibited a beautiful model in wax of the pelvic viscera, constructed by Halma Grande, by whom it was brought to this country.

But the circumstances I have mentioned are not sufficient to account for the prevalence of stricture, which not unfrequently attacks many members of the same family. I have seen repeated instances of this fact. It should seem, therefore, that there is some common predisposing cause in which the disease originates. Mr. White stated, that he believed stricture very frequently resulted from a narrowing of the sigmoid flexure of the colon; and if you look to the position of the bowel, and the natural consequence that is likely to follow from an accumulation of fæces in this part of it, it is not improbable that his opinion was correct. Hence, if this narrowness of the part be consistent with birth, the individual may be born with a predisposition to stricture; and this I believe is frequently the case. Now we all know that a child usually resembles his parents in its features; and if the parent has a disposition to stricture of the rectum from this peculiar formation of the sigmoid flexure of the colon, I confess I see nothing irrational in supposing that the same similarity may occur internally, and is visible externally; and if this is admitted, the prevalence of the disease is in great measure accounted for.

And now with regard to the *symptoms* of stricture, and with which it is extremely important that you should be well acquainted.

I would remark here, that there is not, perhaps, any disease with which we are conversant, that is really so insidious in its early stages. A man has his bowels irregular, which he attributes to simple costiveness; he takes a dose of opening medicine, and finding relief, gives himself no further trouble about the matter. The same thing goes on from week to week, from month to month, and even from year to year, for certainly diseases of the rectum, when of a simple nature, are very slow in their progress, and still slower in producing any serious effect on the general health. It is extraordinary what an extent of time persons will go without passing an evacuation. Individuals have told me, that they have gone a fortnight, three weeks, and even a month, without having a stool. If you refer to the second volume of the "Transactions of the Society for the Improvement of Medical and Surgical Knowledge," you will read of a case of stricture at the sigmoid flexure of the colon, the subject of which lived the extraordinary period of fifteen weeks, without voiding anything from the bowels, and during a portion of that time scarcely any inconvenience was produced. The real nature of this case was not discovered until after the death of the patient, a lamentable mistake, which, I fear, too frequently happens. A person has stricture; immense quantities of opening medicine are given to relieve the constipation, but no good results; and ultimately he dies, the post-mortem examination, alone, proving, too late, the cause of his disease.

Another feature in the complaint is the condition of the stools, as regards their size and shape. Now these points are by no means conclusive. A man may have stricture to a serious extent, yet the evacuations may be in every respect well formed. Take, for example, the instance of stricture situated five or six inches up the bowel.

The feculent matter may slowly drip through the stricture into the healthy portion below it, and there become consolidated. Hence you may inspect the motions, and being deceived by their appearance, never suspect disease high up in the bowel. Again, their shape may be formed by passing over an enlarged prostate, or a displaced uterus, either of which circumstances may render them thin and flat. Patients commonly tell you, that their stools are flat, like ribands. There may be piles near the orifice of the rectum, and the evacuations passing over these, they assume some particular form. For these reasons, therefore, you must not place too great a reliance on the form and size of the motions. The difficulty of voiding them is a far more conclusive symptom of stricture, and you will find this feature more prominent when the obstruction is near to the anus. The power of the abdominal muscles in propelling the contents of the rectum is here lost, and the fæces are expelled by the contractile action of the *internal* sphincter, consequently if the stricture be situated near the orifice, this essential function is destroyed or materially lessened.

But as the contraction becomes more confirmed, you have irritation set up in the adjacent organs. If the stricture be high up, the feculent matter accumulating above it presses upon the fundus of the bladder, and prevents its entire distention, so that the patient is annoyed by a frequent disposition to void his urine. On the contrary, if it be near the orifice, you will more commonly have incontinence of urine. The neck of the bladder becomes irritated, by which the organ is rendered incapable of holding its contents. I have known many cases of incontinence of urine result from stricture of the rectum, which you will not be surprised at when you reflect on the contiguity of the parts. In the female the functions of the womb become disturbed. Sometimes the patient is irregular as regards the menstrual discharge; at one time there is amenorrhœa, at other times you will have menorrhagia. Many miscarriages result from this cause; the morbid adhesions formed between the uterus and the rectum, preventing the ascent of the former organ, it relieves itself by rejecting its contents. I have known this to happen over and over again. Muco-purulent discharges often take place from the vagina in the female, and from the urethra in the male; occasionally the individual has attacks of colic or spasm in the colon, and if the stricture be high up, nature sometimes accommodates herself to the diseased action, and makes an artificial rectum, if I may use the expression, in an enormous enlargement of the colon, as is here shown.

Now I shall claim your indulgence while I just refer to this person's case. The model which you here see was taken from a man now living, who was labouring under stricture of the rectum at about its union with the sigmoid flexure of the colon. The model was moulded about twelve months ago. When I first saw the patient, the enlargement of the colon was full three times as great as you here see it represented. I met the man from whom it was taken by accident, he was begging, and I was so struck with the peculiar expression of his countenance, that I inquired what was the matter with him. He told me that he had some disease in the bowels, and that he had been discharged from all the hospitals in this metropolis as incurable. Now I took some pains to ascertain whether he told me the truth, and I know that he was in three public charities, where his complaint was denominated "internal abscess;" but it proved to be neither more nor less than an enormously distended colon. At the time I saw him he was spitting matter from the chest, and appeared to be fast approaching phthisis; he, however, recovered, and is still living. He had been a soldier in the East India Company's service; and I interested myself to procure him a pension, which I little expected he would have lived to enjoy so long. He has left me his body, when he dies; and to keep my own eye upon him, I allow him sixpence a month, to call upon me and show himself; and I can assure you I find this a very effectual mode of looking after him. He seldom fails in paying me a visit when his sixpence is due.

Now reflect what must be the effect of such a distention of the colon as you here see; why, by pressing upon the kidneys it will disturb their function, it will derange the digestive power, as likewise the action of the liver, and furthermore, by pressing upon the aorta it will prevent the free circulation of the blood in the lower extremities; hence the patient will be annoyed by cramps in the legs and feet, and, in extreme instances, by producing a violent determination of blood to the head, it is very likely to give rise to apoplexy.

I mention these circumstances to you, not because I consider them as the usual symptoms of stricture in the rectum, for I should be sorry if you supposed that I thought any such thing; but in order that you may see what may follow neglected disease of this part. It is not at all an uncommon feature for persons to be attacked with inflammation of the bowels or peritoneum, and you will find extraordinary cases on record, some of them of recent date, in which patients have died from obstruction of the bowels, resulting from stricture in the rectum, where this disease was not even suspected during life, and would probably have never been known at all, but for the post-mortem examination. It often happens, that simple stricture of the rectum goes on to a most extraordinary extent, and the patient ultimately dies, not from the affection in the rectum, but from some other disease, which is the consequence of it.

In malignant contraction, however, there are characteristics by which you can always discover the disease in its early stages; in fact, no man who has a malignant affection in the rectum ever fails mentioning it; his suffering is so acute, that he is compelled to seek the advice of his surgeon, and that at a very early period; whereas common diseases of the rectum are often kept by patients to themselves. They do not experience much pain from them, and they would rather bear that than what they are pleased to call, expose their complaint. But in malignant affections of the rectum, you have the most intense pain in the part, even in the very earliest stages of the disease; a smarting is felt in passing the stools, which patients will describe as feeling as if they had so many cuts over which you were passing a hot iron, or as if there were something grating the part with sand. Every now and then you have gushes of blood from the bowel as portions of it ulcerate, and in the latter stages of the disease you will have a most offensive discharge, just such a discharge as you usually see accompanying carcinomatous affection of the uterus. Large portions of the mucous membrane frequently come away in the stools, and in conjunction with these local symptoms of irritation, the constitution equally suffers. The patient soon becomes emaciated and weak, and exhibits those fatal signs which a man who is an observer of diseased action, knows will generally be followed by death.

For these reasons you may easily discriminate malignant disease, but according to my experience, I confess to you that I do not believe malignant contractions of the rectum are so common as has been apprehended. You will read of scirrhous contracted rectum, but I have never seen many instances of what I should call *true* scirrhus of the rectum. I have shown you many cases of simple stricture of long standing, in which there was great thickening from deposition, but I think there is a distinctive difference between these appearances and those of true scirrhus. I mention this fact, in order that you may not set down all cases of stricture which may come under your observation in their early stages as being of malignant character, even though they are within reach of the finger, nevertheless you must not suppose that you will not have malignant disease beyond the reach of the finger, because, though it usually occurs near to the anus, recollect it may take place at any other portion of the rectum.

Such is the morbid anatomy, such are the causes, and such the symptoms, that occur in the progress of stricture of the rectum, whether it be simple or malignant. I will not detain you on the present occasion by going into the treatment of the disease, because it would occupy much more time than you would now be willing to spare. I will, therefore, defer it till the evening of our next meeting.

INJECTION OF
SALINE SOLUTIONS
IN EXTRAORDINARY QUANTITIES INTO THE VEINS IN CASES OF
MALIGNANT CHOLERA.

Communicated to the Editor *from the* Central Board of Health, *London.*

Letter I.

Sir,—I conceive it to be my duty to let you know, for the information of the Central Board of Health, that the great desideratum of restoring the natural current in the veins and arteries, of improving the colour of the blood, and recovering the functions of the lungs, in Cholera Asphyxia, may be accomplished by injecting a weak saline solution into the veins of the patient. To Dr. Thomas Latta of this place is due the merit of first having recourse to this practice. He has tried it in six cases, three of which I have seen and assisted to treat. The most wonderful and satisfactory effect is the immediate consequence of the injection. To produce the effect referred to, a large quantity must be injected, from *five to ten pounds* in an adult, and repeated at longer or shorter intervals as the state of the pulse and other symptoms may indicate; whenever the pulse fails, more fluid ought to be thrown in to produce an effect upon *it*, without regard to quantity. In one of the cases I have referred to, 120 *ounces* were injected at once, and repeated to the amount of 330 ounces in 12 hours. In another 376 ounces were thrown into the veins between Sunday at 11 o'clock a.m., and this day (Tuesday) at 4 p.m., that is, in the course of 53 hours, upwards of 31 pounds! The solution that was used, consisted of two drachms of muriate, and two scruples of carbonate, of soda, to sixty ounces of water. It was at the temperature of 108 or 110.

The apparatus employed for injecting

was merely one of Reid's common syringes (the fluid being put into a vessel rather deep and narrow) with a small pipe so fitted that it might easily be introduced into an incision in the vein of the usual size that is made in bleeding. It may however be well to keep in mind that, in the event of the operation being frequently repeated, it may be advisable to inject by different veins.

I forbear at present to enter further into particulars; nor have we had sufficient experience to speak decisively upon the subject. I may however mention, that the idea of having recourse to the remedy in cholera occurred to Dr. Latta, from being convinced (which I am also) that the evacuations upwards and downwards, are in reality the serum of the blood; that it is the duty of the physician to replace it as speedily as possible, by injecting a fluid as similar to the serum as can be formed artificially, directly into the veins, which has been done here with wonderful, and so far as we can yet judge, excellent, effect. An immediate return of the pulse, an improvement in the respiration and in the voice, an evolution of heat, an improvement in the appearance of the patient, with a feeling of comfort, are the immediate effects. The quantity necessary to be injected will probably be found to depend upon the quantity of serum lost. The object of the practice being to place the patient in nearly his ordinary state as to the quantity of blood circulating in the vessels.

I have, &c.
(Signed) Robert Lewins, M.D.,
Fellow of the Royal College of Physicians,
and Member of the Leith Board of Health.

Leith, 6, Quality-street, May 15, 1832.

To W. Maclean, Esq.,
Secretary Central Board of Health.

Letter II.

Sir,—I did myself the honour to address a letter to you lately, on the effect of injecting a saline solution into the veins of a patient labouring under cholera. We have not frequent opportunities now of trying this, which I denominate admirable remedy, as the disease is decidedly less frequent here: but I have seen it employed in two other cases in the course of the last two days, with the same excellent effect. Sixty ounces are generally thrown in at once, and repeated at the end of three or four hours. In a case to-day where I saw fifty-eight ounces injected (being the third time of performing the operation), the patient's pulse at the commencement was 180, very small and very feeble. She was excessively restless, with a feeling of great weakness and tormenting thirst. Before twelve ounces had been injected, the pulse began to improve; it became fuller and slower, and it continued to improve, until, after 58 ounces had been injected, it was down to 110. Before I left the patient (a woman), her condition was altogether amazingly ameliorated. There was a fine glow, and a slight perspiration on her face; the veins on the back of her hands were well filled; the restlessness was removed, the feeling of excessive weakness gone, and the thirst had ceased! The pulse was under 100, and full, free, and soft! Verily, Sir, this is an astonishing method of medication, and I predict will lead to wonderful changes and improvements in the practice of medicine.

I have addressed you upon the subject, as the organ, from your high official station, of most speedily and effectually disseminating a knowledge of the extraordinary facts referred to. It will of course give me great pleasure to enter further into particulars upon any particular point on which you may require information, in reference to the cases that have come under my observation.

I have, &c.
(Signed) Rob. Lewins.

6, Quality-street, Leith,
May 18, 1832.

In the hands of a man of ordinary dexterity, the common injecting apparatus alluded to in my last will be found to answer the purpose perfectly well; but if the practice I recommend is, as I hope it will be, generally adopted, it will, I conceive, be expedient to advise that a regular and perfect transfusion apparatus be used: at all events, to warn those who inject to beware of allowing air to get into the vein. The tubes, of course, must be filled with fluid as well as the pipe in the vein before commencing, and considerably more fluid than it is intended to use ought to be in the vessel from which it is pumped.

R. L.

ARSENIC IN ANGINA PECTORIS, &c.

To the Editor of The Lancet.

Sir,—I am not aware that arsenic has been given, or recommended, in the very formidable complaint termed "Angina Pectoris," and having some time ago given this powerful medicine to two patients labouring under alarming attacks of that disease, who had been under an ineffectual plan of treatment for some time previous, with great apparent advantage, I am induced to make the profession acquainted with the fact through the medium of your excellent journal.

M. Martinet is of opinion, that Angina is a neuralgic affection of the phrenic nerves; from this circumstance, and the known influence arsenic has in similar affections of the ischiatic and facial nerves, I was led to the administration of it in the present instances. The subjects were weak, attenuated men, between 50 and 60 years of age, with a tendency to asthma. They took ten minims each of the solution, in a little bread and milk, every eight hours between the paroxysms. In each case it was only necessary to continue the medicine about three weeks, and in neither has there been a relapse.

I have given this medicine in many cases of neuralgic headach with great benefit, nor does constitutional diathesis influence its good effects; for, in two cases lately, that were of a remarkably obstinate nature, and which had resisted almost every other mode of treatment, every symptom completely disappeared after the medicine had been taken for about *nine days*. The one case was that of a carpenter, *thick set, and of ruddy complexion;* the other was a *female, of delicate but healthy aspect;* the latter, in addition, took extract of stramonium at nights.

I am, Sir, your obedient Servant,
ROBERT JONES.
Strefford, near Ludlow, May 14, 1832.

ST. GEORGE'S HOSPITAL.

To the Editor of THE LANCET.

SIR, — The following account of some recent doings at our Hospital will, I hope, deserve a place in an early number of your journal; they indicate, in the strongest manner, the presence of most superlative and degrading carelessness, neglect, and nepotism.

A patient (Thomas Walker) was admitted into the Hospital March 28th, with calculus in the bladder. He was 68 years old, and of a countenance and constitution strongly indicative of great nervous anxiety respecting the operation, which was to have been performed on Thursday, 5th of April; but when that day arrived, it was deferred until the following Monday; and why? *Because Mr. Keate had cut his finger, and could not use the scalpel.* Monday came, and then *it was not expedient to operate, because Mr. Keate's cut finger had not quite healed;* but the operation was certainly to be performed on the following Thursday, *if the finger was well enough!* Thursday came, and the operation was performed; and I am bound to say, in justice to Mr. Keate, in a most masterly and scientific manner; and (*mirabile dictu*) the man has been doing very well ever since. The calculus was very large, and altogether the operation fully deserved the encomium which Mr. Brodie passed upon it, viz., that he had never seen one performed in a more scientific or skilful manner. I have, however, a few remarks to make on the above. So many patients have recently died in the Hospital after the operation of lithotomy, that, judging from every circumstance, very few expected the man to recover. And may I ask—

Had he died during the week of delay on the part of Mr. Keate, and had a Coroner's Inquest been held upon the body, what would have been the verdict recorded? It surely should not have been—"Died by the visitation of God." And yet I dare to say, the sapient jurymen would have starved for a week rather than impugn the character of Mr. Keate by bringing in his name in the verdict.

Giving Mr. Keate every due credit for his manual dexterity and science, may I ask—Whether Mr. Brodie is not fully his equal in these respects; and if so, why was this man made to undergo a week of extreme torture and suffering, because, forsooth, the President of the Royal College of Surgeons *had cut his finger?* Why was not Mr. Brodie requested to perform the operation on the day that it ought to have been done, since Mr. Keate well knew that he could not perform it? I pause for a reply to these questions.

On the evening of Wednesday, 18th April, a man was brought to the Hospital with hernia; and it being Mr. Keate's accident week, a messenger was dispatched to Albemarle-street for him. Mr. Keate was not at home; and the messenger went on to the College of Surgeons for him. And what answer do you suppose was returned? —"*That if the man could wait till midnight, Mr. Keate would then come and operate on him.*" When this answer was received at the Hospital, Mr. Babington was forthwith sent for, and he came and performed the operation. I need scarcely add, that the man died two days afterwards. On this latter case I shall make no remark: it needs none from me. Let Mr. Keate's memorable answer speak for itself.

Before I conclude, may I ask whether it is the intention of the governors of this Hospital to convert the board-room into an operating theatre. If not, why did Mr. Hawkins operate for nævus in the board-room a few days ago?

I am, Sir,
ALPHA.
St. George's Hospital, May 20th.

ROYAL INSTITUTION.

Friday, May 4, 1832.

MR. FARADAY ON VOLTA-ELECTRIC INDUCTION, AND THE EVOLUTION OF ELECTRICITY FROM MAGNETISM.

The power which electricity of tension possesses of causing an opposite electrical state in its vicinity, has been expressed by the general term induction, which may also be used with propriety to express the power which electrical currents may possess of inducing any particular state upon matter in their immediate neighbourhood.

Certain effects of the induction of electric currents have been already described : as those of magnetization ; Ampere's experiments of bringing a copper disc near to a flat spiral ; his repetition with electro-magnets of Arago's extraordinary experiments ; and perhaps a few others. It was unlikely these could be all the effects which induction by currents could produce, especially as upon dispensing with iron, almost the whole of them disappear, whilst a great many bodies, exhibiting definite phenomena of induction with *electricity of tension*, still remain to be acted upon by the induction of *electricity in motion*.

It seemed extraordinary, that as every electric current was accompanied by a corresponding intensity of magnetic action at right angles to the current, good conductors when placed within the sphere of this action should not have any current induced through them, or some sensible equivalent effect produced. Stimulated by the hope of obtaining electricity from ordinary magnetism, I have at various times investigated experimentally the inductive effect of electric currents, and I have at length arrived at positive results ; and have not only had my hopes fulfilled, but obtained a key which appears to me to open out a full explanation of Arago's electric phenomena, and also to discover a new state, which may probably have great influence in some of the most important effects of electric currents.

With the aid of coils or helices of wire, copper, iron, or silver, so arranged with respect to each other as to form two distinct circles ; metallic contact being everywhere prevented, the one helix communicating with a galvanometer, and the other with a powerful battery, certain effects were rendered evident. When contact with the battery was made, a slight and sudden effect on the galvanometer was perceived, and a similar effect when the contact was broken. But whilst the voltaic current was continuing to pass through one helix, no galvanometrical appearances of any effect like induction upon the other helix could be perceived, although the active power of the battery was proved to be great, by its heating the whole of its own helix, and by the brilliancy of the discharge when made through charcoal. On a repetition of the experiment with a more potent battery, it was ascertained that the slight deflection of the needle occurring at the moment of completing the connexion, was always in *one direction*, and that the equally slight deflection produced when the contact was broken, was in *the other direction*.

These results led me to believe that the battery current through one wire, did in reality induce a similar current through the other wire ; but that it continued for an instant only, and partook more of the nature of the electrical wave passed through from the shock of a common Leyden-jar, than from that of a voltaic battery, and, therefore might magnetize a steel needle, although it scarcely affected the galvanometer. This expectation was confirmed, for on substituting a small hollow helix, formed round a glass tube for the galvanometer, introducing a needle, making contact as before, and removing the needle *before the contact was broken*, it was found magnetized. When the battery contact was made first, then an unmagnetized needle introduced into the small helix aforesaid, and, lastly, the *battery contact broken*, the needle was found magnetized to an equal degree as at first ; but *the poles were of a contrary kind*. When the unmagnetized needle was put into the indicating helix *before* contact of the inducing wire with the battery, and remained there till *after* the contact was broken, it exhibited little or no magnetism, the first effect having been nearly neutralized by the second. The force of the induced current upon *making contact*, was found always to exceed that of the induced current at *breaking of contact* ; and if, therefore, the contact was made and broken many times in succession, whilst the needle remained in the helix, it came out at last, not unmagnetized, but a needle magnetized as if the induced current upon making contact had acted alone on it.

These experiments were again and again renewed with a different disposition of the wire, but the needle was deflected according to the same rule. When the wires were approximated to each other in the neighbourhood of the needle, the induced current was in the *contrary* direction to the inducing current. As the wires receded, the induced current was in the *same* direction as the inducing current. When the wires remained stationary, there was no induced current.

By numerous experiments I have shown, that collateral currents, either in the same or opposite directions, exert no permanent inducing power on each other affecting their quantity or tension.

Neither evidence by the tongue, by spark, or by heating fine wire on charcoal, could be obtained of the electricity passing through the wire under induction. Neither could any chemical effects be obtained. This deficiency of effect is probably due to the brief duration and feeble intensity of the electric current.

Attempts to obtain similar effects to these by the use of wires, conveying ordinary electricity, were doubtful in the results. Still it does not follow that the discharge of ordinary electricity through a wire does not produce analogous phenomena to those arising from voltaic electricity; but as it appears impossible to separate the effects produced at the moment when the discharge begins to pass, from the equal and contrary effects produced when it ceases to pass, inasmuch as with ordinary electricity these periods are simultaneous, so there can be scarcely any hope that in this form of the experiment they can be perceived.

From all these facts, it appears to me evident, that currents of voltaic electricity present phenomena of induction somewhat analogous to those produced by electricity of tension, although many differences exist between them. The result is the production of other momentary currents, parallel or tending to parallelism, with the inducing current. By reference to the poles of the needle formed in the indicating helix, and to the deflections of the galvanometer needle, it was found in all cases, that the induced current produced by the first action of the inducing current, was in the *contrary* direction to the latter, but that the current produced by the cessation of the inducing current was in the *same* direction as I have already said. I propose calling this action of the current from the voltaic battery, *Volta-electric induction.* The properties of the wire after induction has developed the first current, and whilst the electricity from the battery continues to flow through its inducing neighbour, constitute a *peculiar electric condition* to be considered hereafter.

Having considered the effects of volta-electric induction, Mr. Faraday next inquired into the possibility of evolving electricity from magnetism. He procured a welded ring of soft round bar-iron, nearly an inch in thickness, and the circle being six inches in diameter. Three helices A were put round one part of this ring, and were insulated from the iron and from each other, and superposed in the same manner as in the first experiment. On the other part of the ring about sixty feet of similar copper-wire, in two pieces, were applied in the same manner, forming a helix B, which had the same direction with the other helices, but was separated from them at each extremity by about half an inch of uncovered iron. This arrangement was applied as in the former experiments. The last helix being attached to the galvanometer, and the first to a battery of ten pairs of plates four inches square, the galvanometer was immediately affected, and to a degree far beyond what occurred when, with a battery of tenfold power, helices without iron were employed; but though the contact was continued, the effect was not permanent, for the needle soon came to rest in its natural position. Upon breaking the contact with the battery, the needle was again powerfully deflected, but in the *contrary direction.*

Upon arranging the apparatus so that the helix B should be out of use, the galvanometer be connected with one of the three wires of A, and the other two made into a helix through which the current from the trough was passed, similar but rather more powerful effects were produced.

When the battery-contact was made in one direction, the galvanometer-needle was deflected on the one side; if made in the other direction, the deflection was on the other side. The deflection on breaking the battery-contact was always the reverse of that produced by completing it. The deflection on making a battery-contact always indicated a current proceeding in a direction opposite to that from the battery; but on breaking the contact the current was shown to be in the same direction. No continuance of the battery-current caused any deflection of the galvanometric needle.

Upon using the powers of 100 pairs of plates with this ring, the impulse at the galvanometer, at the making or breaking of contact, was so great as to make the needle spin four or five times round.

By a modified series of experiments with this ring, Mr. Faraday demonstrated that a feeble voltaic current sent through helix B and the galvanometer, did not affect the powerful current proceeding from the 100 pairs of plates connected with the helix. That when copper was introduced instead of iron, no effect beyond that of the helices are produced.

Similar effects were produced by *ordinary magnets.* Thus the hollow helix formerly described, had all its elementary helices connected with the galvanometer by two copper wires, each five feet in length; a soft iron cylinder was introduced into its axis; a couple of bar-magnets, each 24 inches long, were arranged with their opposite poles at one end in contact, so as to resemble a horse-shoe magnet, and then contact made between the other poles and the ends of the iron cylinder, so as to convert it for the time into a magnet; by breaking the magnetic contacts, or breaking them, the magnetism of the iron cylinder could be destroyed or reversed at pleasure.

Upon making magnetic contact the needle was deflected; continuing the contact, the needle became indifferent, and resumed its first position; on breaking the contact it was again deflected, but in the opposite direction, and then it again became indifferent; when the magnetic contacts were reversed the deflections were reversed.

When the magnetic contact was made, the deflection was such as to indicate a current of electricity induced in the opposite direction to that fitted to form a magnet having the same polarity as that really produced by contact with the bar-magnets.

As it might be supposed that in all the preceding experiments it was by some peculiar effect taking place during the formation of the magnet, and not by its mere approximation, that the momentary-induced current was excited, the following experiment was made. A cylindrical magnet of the same size was substituted for the soft iron cylinder. One end of this magnet was introduced into the axis of the helix, and then the galvanometer needle being stationary, the magnet was suddenly thrust in; immediately the needle was deflected in the same direction as if the magnet had been formed by either of the preceding processes. Being left in, the needle resumed its first position, and then the magnet being withdrawn, the needle was deflected in the opposite direction. When the magnet is introduced, the needle of the galvanometer is deflected in a certain direction; but being in, whether it be pushed quite through or withdrawn, the needle is deflected in a direction the reverse of that previously produced. When the magnet is passed in and through at one continuous motion, the needle moves one way, is then suddenly stopped, and finally moves the other way.

Whichever end of the helix the magnet goes in at, and consequently whichever pole of the magnet enters first, still the needle is deflected the same way. On the other hand, whichever direction is followed in withdrawing the magnet, the deflection is constant, but contrary to that due to its entrance.

These effects are simple consequences of a law hereafter to be described.

All care was taken to guard against any direct action of the inducing magnet upon the galvanometer.

Mr. Faraday made several experiments with the great magnet of the Royal Society, and with striking results. Notwithstanding the great power of this magnet, if the contact was continued the needle resumed its natural position, being entirely uninfluenced by the position of the helix. But on breaking the magnet contact, the needle was whirled round in the opposite direction with remarkable force. With this potent agent numerous variations in the experiments already were made, but with effects always corresponding in kind with those resulting with lesser arrangements.

All attempts to obtain chemical effects, by the induced current of electricity, failed, although every precaution that could be thought of was adopted. Upon repeating the experiments with a loadstone belonging to Professor Daniell, and capable of lifting about thirty pounds, a frog was powerfully convulsed each time magnetic contact was made.

These experiments prove the production of electricity from ordinary magnetism. That its intensity should be very feeble and quantity small is not wonderful; for, like thermo-electricity, it is evolved entirely within the substance of metals, retaining all their conducting power. But an agent which is conducted along metallic wires in the manner described, which, whilst so passing, possesses the peculiar magnetic properties and force of a current of electricity, which can agitate and convulse the limbs of a frog, and which, finally, can produce a spark by its discharge through charcoal, can only be electricity.

The similarity of action, almost amounting to identity, between common magnets and either electric magnets or volta-electric currents, is striking, and furnishes strong reasons for believing that the action is the same in both cases; but as a distinction in language is still necessary, Mr. Faraday proposes to call the agency thus exerted by ordinary magnets,—*magneto-electric*, or *magnelectric* induction.

Tuesday, *May* 8, 1832.

IMPROVISING.

The Marquis de Moscati, an Italian refugee, delivered this evening an animated lecture on an institution celebrated in Italy, and entitled Arcadia, in the course of which he was led to give an interesting description of his initiation as an improvisatoire. When a paroxysm of inspiration came on, he felt the most extraordinary excitement analogous to intoxication, but much more violent. His breath became irregular, his heart palpitated violently, he lost sight of present objects, and a loud buzzing noise rung constantly in his ears. At this time his mental perception and power of association became wonderfully enhanced, and "thoughts that breathe and words that burn," came and went at command. When the paroxysm ceased, however, he fell into a state of great exhaustion, and lay motionless on the floor for *five* and *six* days together. Whilst the lecturer was in Paris he consulted his friends, Drs. Broussais, Magendie, and Edwards, and all of them agreed in pronouncing him mad. To convince him of the truth of their opinion, Rostan and Magendie took him to the Salpetrière, and showed him a lunatic patient who was highly accomplished, and thought justly on all subjects save one, which, when touched upon, rendered him outrageous. The Marquis saw all this, and yet was not convinced of his own insanity. He then gave slight sketches of several well-known improvisatoirs, both ancient and modern, learned as well as unlearned. Amongst the most renowned of these impromptu-poets, a Neapolitan Lazaroni, whose name we understood to be Luciato. These Lazaroni are great admirers of Ariosto, and are divided into two grand parties, the admirers of Rinaldo, and the admirers of Tancredi, and their improvisatoirs, of whom they have several, are employed in a constant poetical warfare on this subject. Luciato was entirely uneducated, and could neither read nor write, yet he was exceedingly intelligent, and had acquired a respectable fund of information. The Marquis and a friend, a physician, sought an interview with this Homer of the Lazaroni, and with *golden* arguments invoked the sybil of his poetic genius. The subject selected was—"*the distinction between a physician and an assassin*." The Lazaroni replied in a string of correct Italian verses, animated with great wit and fancy. The point of satire upon which the whole turned was this:—"The assassin," says the poet, "first kills a man, and then takes away all that he has. The physician first takes every thing that a man has, and then kills him. *Voila la difference*." In conclusion, the Marquis, in order to prove himself a true Arcadian, offered to improvise upon any two or three subjects the audience might choose. The mode of proceeding was the following. The auditory select a given number of rhymes which are to be included by the poet in his versification, thus preventing the possibility of the poet coming with a prepared subject. The subject is then chosen also by the auditory. The Marquis, without hesitation, recited the required number of couplets, upon the two following serious and comic subjects—*La Morte de Patroclo* and *La Tirania de l'Amore*, including in both pieces the *same* rhymes. There was much spirit in both productions, but the inspiration of which the Marquis spoke was not very discernible.

The Marquis de Moscati is a highly ingenious and talented man. He is an exile from his country, and has forfeited his estates in consequence of his attempts to liberate his native land from the grasp of foreign tyrants. He is consequently reduced, during his banishment in this country, to direct his talents to the gaining of a livelihood. He is deeply versed in Italian literature of all ages, and is about to deliver a course of lectures on his native language at Week's Rooms, Great Windmill Street, formerly the Hunterian School. We recommend such of our readers as have a penchant for the beautiful *lingua Toscana*, to patronise this amiable and high-minded nobleman in his laudable efforts.

THE LANCET.

London, Saturday, May 26th, 1832.

The last number of The Lancet had scarcely been sent to press when the melancholy intelligence reached London of the decease of Dr. Andrew Duncan, the late inestimable Professor of Materia Medica in the University of Edinburgh. The fall of so great a pillar of this ancient edifice is an event which may well awaken the attention, and excite the sorrow, of the medical profession.

It would, indeed, be a vain and purposeless task to recapitulate in these columns the many items which swelled the account of fame of the deceased Professor,—which gave him an honoured place among the children of science in every nation, and which will hand down his name to future times in association with the best and brightest ornaments of medicine. It would be vain for us, we repeat, to attempt the task. It baffles our ability as much as its fulfilment would exceed our limits; while there is not, we believe, that lisper in our profession to whom Dr. Duncan's attainments are not familiar, and by whom the nature of his many claims on professional gratitude is not fully appreciated.

The late Andrew Duncan was one of the rare examples which the history of medicine supplies, of a man eminent in all the numerous and dissimilar departments, of which the abstract and almost ideal term "medicine" is constituted. An accurate anatomist and physiologist, he was the first to teach the rational method of dissecting the heart, and demonstrating the direction of its fibres. and he was amongst the foremost who directed attention to urethral and vesical malformations. A surgical pathologist, he established the existence of a new species of inflammation, erroneously regarded by previous and contemporary writers. A profound and original chemist, he discovered *cinchonine*, and thus lit up the lamp which enabled Pelletier and Caventou to detect and demonstrate the existence of quinine, and many other vegetable alkalies. In this capacity also he left us an additional monument of his skill in the analysis of mudar and detection of mudarine, the results of which are given to the profession in the last paper published before his decease. As a practical medical botanist he stood unrivalled; of his ability as a clinical teacher and practitioner our pages show splendid evidence. His fame as an author is based on the firm structure of twelve editions of his voluminous and invaluable Dispensatory. As a writer, he was distinguished for his perspicuity, his reasoning powers, and the modesty and amiable polish of his style. As a lecturer, his profound knowledge of every subject on which he treated, was equalled only by his zeal for the communication of that learning to his classes. The pupils' true and steady friend, he was their stern though merciful examiner; their willing and generous attendant in the hour of sickness; their kind, encouraging, and hospitable patron wherever merit deserved his peculiar favour; he was the defender of their privileges, and the active reformer of the wrongs by which they were aggrieved. Such is a scant epitome of Dr. Duncan's public merits, while in private life his diversified accomplishments his disciplined and excellent taste, and his innate benevolence of heart, made him all that an affectionate family should love, and that the more extensive circles of society should value and admire.

We speak these things of the departed Duncan, in the terms of exact though melancholy truth. His colleagues and old pupils intend, we understand, to place his bust in marble in the Library Hall of the University. We trust that no inscription will deface the pedestal. The memory of such a man requires no epitaph.

Next to the sorrow felt for the death of an illustrious public man, the most natural tendency of the human mind will lead to the contemplation of a fit successor in the public offices held by the deceased. In alluding to this subject, we must set out by stating that we know not, except by mere rumour, who are the candidates for the vacant Professorship. Neither have we the least personal interest in the kind of appointment that may be made. Anxious, however, for the preservation of the high character the Edinburgh University still maintains, despite the absurd and barbarous jurisdiction to which it is subjected, we willingly take up the theme, and proceed to inquire briefly who is the person most worthy to assume the gown of the late Professor.

> "Is there none left on whom the robe of fire
> Falls like the prophet's mantle?"—

The unanimous voice of all who possess the least knowledge of the present state of the Edinburgh University, must reply, that within the walls of that institution, there is an individual so eminently qualified to discharge the duties of that Professorship, that nothing short of the most perfect blindness, or the grossest jobbing, will countenance any other appointment, should the distinguished gentleman (we of course allude to Dr. Christison) think proper to accept the Professorship, and we emphatically call upon him, for the credit of the University, not to shrink from a post to which he is so peculiarly fitted.

We are perfectly convinced that had the late Dr. Duncan been asked to appoint a successor to his chair—one who would maintain the rank to which he had raised it, who would keep pace with science, and who possessed all the collateral accomplishments requisite for this peculiar Professorship, Dr. Christison would have been the instant object of selection. It is absolutely impossible to produce an abler candidate. Dr. Christison's medico-legal pursuits necessarily constitute him a botanist, anatomist, and physiologist. His excellent and varied writings give him a conspicuous rank among the pathologists and practitioners of the present day. As a chemist he stands unrivalled, and we are well aware of his proficiency in every subordinate acquirement which such a situation demands. And then if we take fame into the calculation, where can be found another candidate, more extensively and favourably known to the professional world in every civilized country? He is the author of the best book but one which the Edinburgh University has produced, and though last, not least, he possesses physical qualifications for excellence as a lecturer of the most rare and valuable kind.

If then the venerable Baillies, &c., whose influence goes far to decide these appointments, retain a particle of honesty or national feeling, they will, without hesitation, *offer the office* to Dr. Christison. They should remember, that a rival University is daily shooting into vigour, and that it is only according to preponderance of talent that public favour will be awarded to one or the other of these institutions.

It has been stated to us, as an objection to Dr. Christison's appointment, that his services are so valuable as a lecturer on forensic medicine, that he must be retained in that position. The immaculate propriety of the Town Council could by no means sanction the holding of two professorships by the same individual—Pshaw! This is too thin a veil to hide the mammon of place and patronage. The professorships do not clash in the least degree. The pursuits of one, favour, instead of counteracting, the duties of the other. It would be rank injustice towards Dr. Christison and the public not to insist on his retaining both appointments.

We beg to call attention to the papers on the chemical pathology of cholera, inserted in this day's LANCET. It will be seen that Dr. Clanny offers no reply to our strictures. As to his claiming merit for the coincidence of his results with those of Dr. Thomson, we have only to remark, that Dr. O'Shaughnessy's observations on the experiments of the latter, save us the trouble of saying more, than that the process employed by Dr. Clanny for appreciating the amount of colouring matter (*not free carbon*) is the same as that employed by Dr. Thomson, and which Dr. O'Shaughnessy shows to be exposed to several sources of fallacy.

LONDON HOSPITAL.

COMPOUND FRACTURE OF THE SKULL, WITH DEPRESSION.

R. A., ætat. 59, was brought into the Accident Ward of this Hospital on the 26th of March, with compound fracture of the os frontis. While walking in the road he had been thrown down, and fell with his head against the edge of the curb-stone; he was stunned by the blow, but quickly recovered, and when brought into hospital immediately afterwards, was perfectly sensible. On examining him, there was found to be a contused wound, situate a little above the left superciliary ridge, being about four inches in length; and around this to some extent, a depression of bone was very perceptible. Notwithstanding this depression of bone, the patient did not manifest any symptoms of compression of the brain. He was able to answer, distinctly, questions which were put to him; the breathing was free from stertor, and the pupils were obedient to light; the pulse about 82, and the motions of the body perfect; it was therefore not deemed necessary to elevate the depressed bone. He was bled to sixteen ounces, and cold lotion was applied to the head. He was ordered to take two grains of calomel every three hours.

28. Pulse 74, strong; tongue tolerably clean; pupils natural; complains of pain over the wound; he is perfectly sensible; has had very little sleep; bowels open twice.

29. Pulse 86, full; bowels have not been opened to-day.

30. Slept well; pain in the head less; pulse 78, full; tongue clean; bowels open; complains of his mouth being sore.

31. Free from pain; bowels open; sleeps well; has very little pain in the head.

April 1. Pulse 84, full, but soft; bowels open six times; passed a very restless night; complains of the calomel griping and purging him very much; mouth very sore.

2. Pulse 78, full, but soft; free from thirst; tongue clean; skin cool; has no pain in the head; in every respect doing well; the wound looks healthy. From this date the patient has continued doing well, and in all probability he will leave the hospital in a few days with the wound entirely closed.

FRACTURE OF THE CRANIUM WITH DEPRESSION.—DEATH.

—— Smith, ætat. 62, was brought into this Hospital about eleven in the morning of the 18th April, labouring under fracture, with depression of the skull. The persons who brought him hither stated, that while employed with four other men removing a dyer's vat, in passing it out of a door-way it slipped, and he falling forwards at the same time, his head was jammed between it and the door-post.

When admitted he was sensible; pupils dilated; pulse slow; respiration free and easy, without any stertor. On examination, a fracture was detected on the left temple, with a depression of a portion of the temporal and parietal bones, about the size of a crown piece; he was bled to 20 ounces, and immediately after the bleeding, he vomited the contents of his stomach.

About two hours after his admission, symptoms of compression of the brain came on; the breathing became stertorous; pulse slow and labouring; pupils dilated and immoveable; lies perfectly insensible, as though in a deep sleep; can be roused on shaking him, or speaking loudly to him, but instantly relapses into a state of insensibility; it was, therefore, determined to elevate the depressed portions of bone, and the trephine was accordingly used, by the use of which, and Key's saw, two insulated portions of bone were removed.

About six o'clock in the evening, he became extremely restless and violent, so much so as to require the restraint of the strait-waistcoat. At eight o'clock considerable reaction had taken place; he was bled to eighteen ounces, and fifteen grains of calomel were given him; he was ordered to have four grains every four hours. On being visited by Mr. Scott at nine o'clock, the calomel was increased to ten grains every four hours, and he was ordered to have twelve leeches applied to the forehead.

19. On visiting him this morning, found he had passed a very quiet night; bowels open twice; breathing free from stertor; pulse 80 and soft; skin cool; to continue the calomel as before directed.

At six o'clock in the evening, his pulse having increased in quickness, he was bled to twelve ounces, after which he gradually sunk until nine o'clock, when he died.

We were not present at the post-mortem examination, but learned that the head was examined about sixteen hours after death, when a fracture was found extending from the left side across both orbits, and extending to the spot immediately opposite to the injury; there was slight extravasation of blood on the surface of the brain, and the lateral ventricles were filled with a bloody serum.

AFFECTION OF THE MEDIAN NERVE AFTER AMPUTATION.

On the 21st April, Mr. Luke dissected out a portion (about one third of an inch) of the median nerve, from the stump of the arm of Elizabeth Burkitt, ætat. 19.

About three years and a half since, this poor girl went into St. Thomas's Hospital, with injury to the left-hand, occasioned by a fender having fallen across the back of it. After being there for several months, it was found necessary to have the hand removed, owing to the excessive suppuration which had taken place, and by which her general health became very much impaired. She states, the stump healed kindly, but shortly afterwards, she suffered excruciating pain in the stump, and for which amputation was performed by Mr. Langstaff above the elbow; the stump healed up, but shortly afterwards, violent pain, with convulsive twitchings of the stump came on, which prevented her getting any rest night or day. She applied at this hospital for relief, and desired the stump might be removed at the shoulder joint, but which was not acceded to, and this operation proposed instead, to which she gladly consented.

Immediately on the division of the nerve, the pain and spasmodic twitchings of the stump ceased. She was placed in bed, and ordered to be kept perfectly quiet. The day following the operation, she complained of severe pain in the head, her skin hot and feverish, for which she was ordered the saline mixture three times a day, half a drachm of the extract of hyoscyamus at night, and four leeches to each temple.

23d. Passed a very restless night, owing to the severe pain in the head; bowels open, tongue clean, pulse 84, skin hot, complains of great intolerance of light. *V. S. ad ℥x ; rept. mistura.* After a short time, these febrile symptoms gave way, and she then suffered retention of urine, for which the catheter was introduced night and morning.

May 12th. The operation appears to have entirely succeeded, the wound having very nearly healed, and there being no return of pain; there is considerable numbness in the stump. She feels extremely grateful for the operation, being now enabled to enjoy a degree of comfort to which she has been an entire stranger for the last three years. She feels so much relieved by the operation, that, to use her own expression, she can hardly think she is her own self.

T. R., ætat. 7, was admitted into this hospital on the 9th instant, with laceration of the fingers of the left hand, caused by the machinery of a flock mill.

The dresser immediately amputated the first phalanges of three of the fingers, since which the little fellow has done very well.

COMPOUND FRACTURE OF THE TIBIA AND FIBULA.—AMPUTATION.

On the 19th of April, Mr. Andrews amputated the lower extremity of T. W., labourer, ætat. 48. This man was admitted into the hospital as far back as the 26th December last, with compound fracture of the tibia and fibula, having been run over by a cart wheel. Shortly after his admission it was feared, from the extensive injury, it would be impossible to save the limb, and he was advised to submit to the operation; but which he refused. The imminent danger of his situation was explained to him, but he resolutely and firmly adhered to his former determination. The fractured portions of bone were brought as accurately together as could be, and the limb laid in splints. Excessive suppuration followed; the man became hectic; had profuse night sweats, small quick pulse; cough, with purulent expectoration and great wasting away. The operation has been several times proposed to him, but he always refused submitting to it.

19th. On examining the limb to-day, it was found that no union of the bones had taken place, abscesses occupying nearly the whole of the leg, and the man extremely reduced by hectic, although he has been allowed a liberal diet, with wine and porter. Under these circumstances he was again informed there was no chance of saving his life unless the limb was removed; he at length consented, and was at once taken to the operating theatre, when Mr. Andrews commenced by making a circular incision about three inches above the knee, dividing the integuments; the muscles were next divided, and then dissected back; after which the bone was sawn through, very little blood was lost, and ligatures were applied.

The first night after the operation passed extremely well, he felt quite easy and comfortable, and had very little pain in the stump; his cough was not quite so troublesome as it had been; pulse 108, small and weak; skin cool; spirits good.

21st. Pulse 88, weak, spirits and appetite good, expectoration much diminished, tongue clean, bowels open, skin cool, feels quite comfortable.

23d. The stump was dressed to-day, looks remarkably well, adhesion having taken place throughout nearly the whole extent; there is a small quantity of healthy pus discharged; pulse 90, soft; bowels regular, appetite good, skin cool, tongue clean.

26th. The stump looks extremely well; discharge healthy, and in small quantity; most of the ligatures came away to-day; in every respect doing well.

28th. Continues improving; the last of the ligatures came away to-day.

May 12th. This case has continued improving. The stump looks remarkably well, being now nearly united. He was ordered to sit up daily; and from his progressive improvement there is every reason to suppose he will shortly be in a state to leave the house. Although this case has terminated favourably, it is greatly to be regretted that this man obstinately refused for so long a period to submit to a necessary, and as the case has proved, an ultimately inevitable, operation, as by so doing the cure has not only been protracted but his life endangered.

WESTMINSTER HOSPITAL.

GANGRENE OF THE LUNGS, VOMICÆ, AND PNEUMATOSIS THORACIS, WITHOUT COMMUNICATION WITH THE BRONCHIÆ.

April 27, 1832. William Hill, a smith, thirty years old, had enjoyed a fair share of health all his lifetime, but was never particularly hale. He was much addicted to drinking, and led a very irregular course of life. He in consequence became very susceptible of attacks of cold upon the lungs, which, however, were always quickly removed by rest, regimen, and medicine. In the winter-time, for the last few seasons, he had been annoyed with a pertinacious cough, which was short, frequent, and unattended with expectoration. The surface of his body did not show any cicatrices or other vestiges of syphilitic action. He was well formed, of good symmetry, moderately fine complexion, and well developed muscular system. About a fortnight ago, this man was affected with pain of the head and other febrile symptoms. He was obliged to desist from work immediately, and was attended by the parochial doctor, but without any benefit, and on admission to-day he presented the following picture. Countenance and the whole respiratory apparatus expressive of anxiety. Breathing accelerated; respiratory murmur enhanced on both sides of thorax; sense of constriction and pain in the chest; headach, nausea, and general debility; tongue coated with a brown fur; dry along its median line; red at its apex and edges; no appetite; bowels open; urine copious and high-coloured; pulse strong and hard; skin hot and dry. *A calomel and antimonial bolus to be swallowed directly; venesection to sixteen ounces, and the following medicine every four hours:—Take of ipecacuanha wine, three drachms; distilled water, eight ounces. Mix, and let an ounce of it be taken for a dose.*

29. The blood drawn is highly buffed and cupped. Last night he was seized with a shivering fit, which was followed by heat and sweating. There is a loud *râle sonore* on every aspect of the chest. He has an acute pain at the scrobiculus cordis; tongue moist and white; bowels open. *A blister to the pit of stomach; repeat medicines.*

Two p.m. Great excitement of respiration and sanguiferous system; has just recovered from a fit of rigors, and breathes 80 times per minute. Pulse quick, full, and hard; tongue covered with dark brown fur; skin hot and dry; pressure on abdomen produces great pain. He complains of acute pain under the floating ribs on both sides, which is much aggravated by the act of drinking. Râle sonore very loud. Venesection to twelve ounces.

30. Somewhat relieved by the loss of blood, which is again buffed and cupped, but the respiration is still as rapid as ever, attended with occasional gasping. Percussion *dull* on the right side of chest; râle sonore loud and strong, but modified by a *peculiar vibratory sensation.* The voice is feeble, and produces no pectoral reverberation. The patient always refers the pain to the epigastrium. *Cupping of the sides; mustard cataplasm to abdomen.*

May 1. The pulse has, for the last two days, been very irregular, varying every minute in force, frequency, and character. The anxiety of the patient's countenance is rather augmented than diminished. Dyspnœa intolerable; râle sonore has a *ringing* character. He expired at noon.

Necrotomy 48 *Hours after Death.*

The patient was not much emaciated, but his contracted visage even then commemorated the agony of the struggle he had undergone. Upon raising the sternum the pleuræ were discovered distended with air, and on opening these bags the lungs were seen compressed against the mediastinum and vertebral column. There was on both sides a large quantity of serum, in which were floating flocculi of lymph. A strong gangrenous stench emanated from the lungs, and on separating a portion of the external surface of the superior left lobe from the thoracic wall a large cavity was opened, of which the greater part was engraven in the substance of the lung. A portion of its parietes, however, also consisted in the costal pleura to which the lung was adherent. This sac contained a drachm of highly offensive sanies, and was itself gangrenous to a great extent. A small aperture communicated with the interpleural space, and was evidently the consequence of the sphacelation of the point of the pleura pulmonalis. The contents of this vomical sac had escaped between the pleuræ, but no direct or satisfactory communication could be ascertained to exist between the morbid cavity and the bronchial tubes. Two other solutions of continuity, similar to the one just described, were discovered communicating with smaller vomicæ. On making longitudinal incisions through the lungs, the substance or parenchyma was found to be in general pretty healthy, although, in consequence of the external pressure, a few air-bubbles only could be perceived in them. Several small abscesses, of the same character as the first were met with, dispersed through the substance of both lungs, distant from each other, and abruptly separated from the surrounding tissue by a dense sac. These were filled with putrid pus, and had no outlet, either to bronchiæ or pleura. A small cluster of white pulpy tubercles, forming altogether a nidus not so large as a small hazel nut, appeared in the course of an incision through the apex of left lung. The whole of this little aggregate was enclosed in a thick membranous tissue, which probably formed the sac of the future vomica. On the right side there was a greater secretion of lymph amongst the serum than on the left, but without lung, though a due share of abscesses were present; no tubercular deposits were encountered. The largest vomica on the right side was in the upper part of the inferior lobe, and had an opening through the pleura. There was a remarkable healthiness in the texture of both lungs, even in those portions which were in immediate contact with the very walls of the vomicæ. These vomicæ were rendered apparently impervious to the air of the bronchiæ, by the coriaceous sacs which involved them. It is probable, however, that the air which distended the two pleuræ penetrated the texture of these sacs. The whole of the mucous membrane was highly injected, and afforded indications of long-standing inflammation. The projecting edges of membrane, seen at the axillæ of the bronchiæ, were universally thickened. The larynx was sound. In the other divisions of the body, no morbid traces were discernible.

ST. BARTHOLOMEW'S HOSPITAL.

CUT THROAT—HEMORRHAGE FROM THE LINGUAL ARTERY.—DEATH.

MAY 15. ——— was admitted into Powell's Ward about five days ago. He was a man-servant, and in a state of high excitement had seized a penknife, and inflicted a wound just above the os hyoides. This act was preceded, we believe, by an attempt to drown himself. Upon admission we did not see the case, but there was, we understand, a good deal of hemorrhage, which appeared to be chiefly venous; this however, was shortly put a stop to without any vessels being secured, as it was not known that any artery of consequence had been divided. The state of the man's mind soon became tranquil, he lay calmly in bed, and seemed likely to go on very well. Yesterday morning hemorrhage suddenly came on, which the sister arrested by cold applications. About eleven o'clock last night, *having returned by himself from the water-closet*, and got into bed, the attention of the nurse was suddenly called to him by a brief ejaculation. She found him with red blood streaming from his nose and mouth, and although Mr. M'Whinnie, the house surgeon, arrived in a minute or two, he found the patient lifeless, having lost about three quarts of blood. This unexpected termination of the case excited much interest in the *autopsy* which was instituted to-day, and the most rational way of accounting for death appeared to be the supposition that some vessel unknown to be wounded had been plugged up by a clot of coagulum, which was disturbed by the exertion of going to the water-closet. Upon examining the body, the excessive pallor of the skin, the empty state of the heart, and the vascular condition of the brain, described as present, under such circumstances, came under observation. There was an ounce and a half of fluid in the ventricles of the latter organ. Dissecting out the vessels around the wound, it was found that the *lingual* artery of the left side had been divided, and the extremity lay exposed in the gash which had been made. The other blood-vessels remained entire. It is proper to state, that the man had no instrument within his reach with which he could have effected a second cut.

ANOTHER CASE.—DEATH IN THREE WEEKS.

April 28th. A— B—, a strong, stout man, ætat. 56, was this morning brought to Coulson's Ward by some policemen. This was another case of cut throat. The account given by the policemen was, that about one hour previously the man, who was a cobbler, entered a barber's shop. There he was shaved; after which, taking up the razor in the barber's absence, he inflicted on his throat the present injury. His shirt, under-waistcoat, and upper clothes, are saturated with blood, but there is now very slight hemorrhage. His face is pallid; pulse scarcely perceptible. His eyes are closed, and great determination is imprinted on his countenance. Once he has opened his lips to exclaim, "I'm not a villain"—marking some conflict in his mind. Upon removing his garments the wound was found with some strappings on it, and ligatures applied on two or three small vessels. The dressings being removed, one very large gash was discovered, dividing the windpipe at the upper edge of the thyroid cartilage, and extending far laterally so as just to escape the great vessels. Besides this cut there were three smaller ones transversely, and one perpendicularly down the front of the larynx, but these were merely through the integuments. The wound was sponged and cleaned, and the edges approximated by raising the head and neck on pillows. Brandy was administered immediately on his admission, and continued afterwards, much escaping through the wound. The pulse soon acquired a more natural power. Head to be shaved. *Acetat. Morphiæ*, gr. ¼, statim.

29th. Has been very restless through the night; frequent delirium; persists in a wish to destroy himself, and even attempted to do so by hanging. Upon the administration of food by the stomach-pump he tries to push the tube into the larynx. Ordered *Vin. Antim.*, and apply *Emplastrum lyttæ amplum* to the nape of the neck.

30th. Outrageous during the night, but quiet to-day. The blister has been applied. Wound is sloughing and looks very formidable.

May 1st. Much the same. Continued thirst, to alleviate which cool spring water is applied to his lips; pulse moderate.

3rd. Extensive sloughing of the wound, causing a very copious and fetid discharge; mind appears more easy. The *morphia* is administered as need may be. A large bread poultice is to be applied over the whole wound.

6th. The poultice appears to have had a good effect on the wound, which looks somewhat cleaner and healthier.

From this time there is little use in giving a regular report. Suffice it to say, that from the state of excitement, the little nourishment which could be conveyed into his body brought him to a very low state, whilst the wound made no progress towards improvement. He subsisted on arrow-root, but was gradually sinking. This state of things continued until the 19th, on the night of which, after being in vain stimulated with doses of brandy, he died quietly.

21. The *post-mortem* inspection was obtained to-day. The body was much emaciated, the fat being absorbed, except here and there, where an isolated lump remained on the legs and arms. Wound dirty, ragged, ununited in any part, and forming the source of a muddy-looking discharge which burrowed down, beside the larynx, towards the cavity of the chest. The thyroid cartilage was laid bare by the sloughing process. Inside of larynx and trachea paler than natural; lungs and pleura healthy, as also the abdominal viscera. The examination of the brain and its investments exhibited nothing peculiar, except a slight increase of vascularity in the latter, and a more soft state than is natural to the former. The fluid in the ventricles was about equal to the usual quantity.

The history of this patient is not known further than that he is said to have been incited to suicide by taunts raised against him by some companions.

PROPOSAL TO FRANK ALL MEDICAL PERIODICALS.

To the Editor of The Lancet.

Sir,—I observed in the title-page of the *Cholera Gazette*, "FRANKED to any part of the kingdom." Now it is not my wish or intention to quarrel with this piece of liberality, deeming it a very proper facility for circulating a publication which certainly *ought* to possess great value; but, Sir, as there are several weekly and monthly periodicals, wholly devoted to the purposes of disseminating equally, not to say more, useful medical information, I should very much like to know, why this boon of FRANKED might not be extended to them also? As the desire of doing good solely, no doubt, influenced this free transit of the Gazette, would not an extension of this privilege be eminently beneficial to science, and consequently to health? Pray turn your attention to this point; and, as you are so capable of taking an enlarged and enlightened view of subjects in general, see if you cannot so unfold my meagre suggestion, as to show at once its justice and reasonableness in such a manner as to secure, if not its adoption, at least the hearty exertions of the profession for ultimately securing it. Being a metropolitan, no advantage would accrue to myself, but my "country cousins" would feel it by the earlier receipt of their mental food. I am, Sir,

An Invalid Subscriber to
The Lancet.

TO CORRESPONDENTS.

Communications have reached us from Mr. E. F. Williams—A Surgical Reformer—U. X.—Mr. W. Haylett—Mr. Bedingfield—Philo-Lancet—Dr. Harrison—S. P.—A Bartholomew Student—Mr. P. Summers, Mr. Waters, and other gentlemen.

The notice sent by our friend, in the neighbourhood of Cavendish Square, would be charged at the Stamp Office as an advertisement.

Dr. Ayre's paper on the treatment of cholera reached us too late for insertion, but it shall appear in our next Number.

Want of space has compelled us to postpone the publication of several communications till next week. Amongst others, interesting cases forwarded by Dr. Wetherall of Liverpool.

The acknowledgment on the part of T— is not less creditable to his veracity than to his manly sincerity.

Inquirer. We understand that several of the gentlemen *did* signify their disapprobation of the toast. No one can doubt what they *ought* to have done.

The complaints of *H. W.*—*A Dresser*—and others, shall be kept in view. If their own remonstrances fail of effect, we will try what can be accomplished by a little public castigation.

Mr. R. D——. The paper *was* received. It is under consideration.

THE LANCET.

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LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

LECTURE XXXI.

CHEST AFFECTIONS IN THE HORSE.

I HAVE hinted at the disadvantage under which the veterinary surgeon labours in his treatment of the diseases of the chest, and particularly in the horse. He must not only subdue the malady, but he must remove all its consequences. *He must leave his patient perfectly sound*, or he has done comparatively nothing. This is a task always difficult, and sometimes impossible to be accomplished. The two most frequent consequences of severe chest affections in the horse—are recognised under the terms *thick-wind* and *broken-wind*. The breathing is hurried in both, and the horse is generally much distressed when put upon his speed; but it is simply quick breathing in the first, with a peculiar sound like *half roaring*; the inspirations and expirations are rapid, forcible, but equal: in the second the breathing is also hurried, but the inspiration does not differ materially from the natural one, while the expiration is difficult, doubly laborious. The changes of structure which accompany these states of morbid respiration, are as opposite as can be imagined. Induration of the substance of the lungs, diminution of the number or the caliber of the air-passages, are the causes of *thick-wind*. If the portion of lung employed is lessened, as the bronchial tubes will not admit so much air, the quick succession of efforts must make up for the diminished effect produced by each. In *broken-wind* there is rupture of the air-cells, there is an unnatural inter-communication between the air-cells in the same lobule, or between those of the neighbouring lobuli. It is no longer, if we may dare so to consider it, a rounded cell with one opening into it, through which the air will easily pass during the expansion or contraction of the cell, but the membrane between the cells being ruptured, a kind of labyrinth is formed, in which the air becomes entangled, and from which it is impossible to force it by a single effort, or even perfectly by the double effort that characterises *broken-wind*. The structure of the lung, and the discharge of function, and the treatment, too, being so different, these diseases require separate consideration.

THICK-WIND.

Hepatisation.—When speaking of pneumonia I observed, that not only are the vessels which ramify over the delicate membrane of the air-cells gorged with blood, but they are sometimes ruptured, and the cells are filled with blood. The black, softened, pulpy appearance of the lung thus produced, is the *rottenness* of the groom and farrier, proving equally the intensity of the inflammation, and that it is of recent date. If the horse is not speedily destroyed by this lesion of the substance of the lungs, the serous portion of the effused blood is absorbed, and the solid becomes organised. The cells are obliterated, and the lung is *hepatised*, or its structure bears considerable resemblance to the parenchyma of the liver. This may occur in patches, or it may involve a considerable portion of the lung.

Effect of this on Respiration.—Now the horse has no lung to spare in the performance even of the ordinary service we require from him, much less under the cruel tasks which we sometimes impose upon him. If, therefore, a portion of the lung is thus rendered impervious, the remainder will have additional work to perform. The same quantity of blood must be supplied; and if the working part of the machine is diminished, it must move with greater velocity as well as force, or the respiration must be quicker and more laborious. This quick and laboured breathing can be detected even when the animal is at rest, and it is indicated plainly enough by his sad distress when he is urged to unusual or continued speed. The inspirations as well as the expirations, are shorter, as well as more violent; the air must be more rapidly admitted and more thoroughly pressed out; and this is accompanied by a peculiar sound. I have called it a half-roaring, that can rarely be mistaken.

Vomicæ.—The healthy structure of the lung is rarely or never restored when the air-cells have been once filled with, and obliterated by, this organised deposit. The hepatisation, however, does not always remain, for the organisation is not complete; it does not perfectly resist various and perhaps slight causes of decomposition, and a softening process usually commences. This deposit, or some portion of it, begins to dissolve. I know not how we are to ascertain the commencement of this change, or to take advantage if we could discover it; the membrane has been compressed, destroyed; the minute ramifications of the pulmonary arteries have been compressed, destroyed; and one uniform mass has resulted, which is now taken up by the absorbents, and instead of the restoration of the primary air-cells, with their mesh-work of innumerable vessels, we have larger or smaller cavities — ulcerations — vomicæ. There are few cases of old hepatisation in which there are not some parts in a state of softening; and the cases are numerous in which the vomicæ have been very extensive, while nothing has indicated their existence. Indeed when we consider that they happen in a part already removed from the function of respiration, and furnished with organic nerves alone, or with few sensitive fibrils, we need not wonder that this secondary change should proceed so far, and we remain perfectly unconscious of it.

Advantage of Auscultation.—This brings me back to a circumstance on which I have already laid much stress—the habitual and anxious recourse to auscultation in every inflammatory affection of the chest. We may guess at the commencement of that congestion which precedes hepatisation, by the laborious heaving of the flanks; but by auscultation alone can we *ascertain* its commencement and progress. The increase of the crepitus will tell us that the mischief is beginning, and the cessation of the murmur will clearly mark out the extent of the congestion.

The inflammatory stage of the disease having passed, and comparative health being restored, and some return to usefulness having been established,—the horse being now *thick-winded*, auscultation will be far more valuable than is generally imagined. It will faithfully indicate the extent of the hepatisation, and so give a clue to the degree of usefulness, or the degree to which we may tax the respiratory system; and it will also serve to distinguish, and that very clearly, between this cause of *thick-wind*, and the morbid changes that may have resulted from bronchitis, or the thickening of the parietes of the air-passages, and not the obliteration of the air-cells. This leads me to the consideration of a second cause of thick-wind.

Thickening of the Lining Membrane of the Bronchiæ.—I have described bronchitis as inflammation of the lining membrane of the bronchial tubes. I need not tell you that one of the tests of inflammation is tumefaction, increased deposition in the substance of the part inflamed. This is peculiarly exemplified in bronchitis; the thickening and the sensibility of the membrane produce in that disease a disturbance in respiration unknown in any other malady, and almost incredible. It will often be found that this unnatural deposition, this thickening of membranes, will not always subside with the subsidence of the inflammation. If that has been intense, and especially if it has not been promptly attacked, the engorgement will long remain materially interfering with the function of the part. The Professor of the Veterinary College, and my valued friend Mr. Percivall, are apparently at issue here; the former attributing thick-wind to effusion into the air-cells, and the latter to a thickening of the membrane lining these cells or the passages to them. The truth of the matter is that they are both right. The effusion and the thickening, and particularly the latter, are causes of thick-wind. The previous disease, when it can be ascertained, will determine to which of the morbid changes of structure the impeded function of respiration is to be attributed.

Auscultation.—If we know nothing of the previous disease, auscultation will generally determine the matter for us. There will be no deficiency of murmur, stillness, at any point, but an increased sound everywhere—a whizzing—a kind of roaring. In some instances, both causes seem to have combined. While there has been thickening of the bronchial membrane, there has been deposit in the air-cells. So far then as *thick-wind* is connected with previous diseases, it is plainly referable to one of these causes, hepatisation of the substance of the lungs, and obliteration of the air-cells, or thickening of the lining membrane, and diminished caliber of the passages leading to these cells. Whether the cells are fewer, or the passages are contracted, a less quantity of air can be admitted to arterialise the blood at each inflation of the chest, and then the act of respiration must be oftener, and consequently more forcibly performed, and that in proportion to the hepatization or engorgement.

Treatment.—Of this, Gentlemen, I have little to say. I know not by what means I can excite the absorbents to take up the solid organised mass of hepatization, or to restore the membrane of the cells and the minute vessels ramifying over them, now confounded and lost. I have a somewhat better chance, and yet not much, in removing the thickening of the membrane—counter-irritants extensively and perseveringly applied to the external parietes of the chest might do a little. If thick wind immediately followed bronchitis, it would certainly be justifiable practice to blister the brisket and sides, and that repeatedly; and to administer purgatives if we dared, or diuretics, more effectual than the purgatives and always safe.

Food.—Our attention must be principally confined to diet and management. A thick-winded horse should have his full proportion, or rather more than his proportion of corn, and a diminished quantity of less nutritious food, that the stomach might never be overloaded, and press upon the diaphragm, and so upon the lungs, and increase the labour of these already overworked organs. Particular care should be taken that the horse is not worked immediately after a full meal—the overcoming of the pressure and weight of the stomach will be a serious addition to the extra work which the lungs already have to perform from their altered structure.

Exercise.—Something may be done, and more than has been generally supposed, in the palliation of thick-wind, by means of exercise. If the thick-winded horse is put, as it were, into a regular system of training, if he is daily exercised to the fair extent of his power, and without seriously distressing him, his breathing will become freer and deeper, and his wind will materially improve. We shall call to our aid here one of the most powerful excitants of the absorbent system—pressure,—the pressure of the air upon the tube, the pressure of the working part of the lung upon the disorganised; and adjusting this, so as not to excite irritation or inflammation, we may sometimes do wonders. You know, Gentlemen, that this is the very secret of training, and that the power and the durability of the hunter and the racer depend entirely upon this. I may almost say that every untrained horse is, to a certain degree, a thick-winded one; there is either a deposition of adipose or other matter, or there is an indisposition in the parts to accommodate themselves to unusual exertion, and which gradually yields to the habit of exertion pushed to the point which the animal can bear without distress. A thick-winded horse may be seriously injured by being pushed beyond his power; the over-laboured cells or passages may burst from the violent influx of the air, or its forcible expulsion and thick-wind may degenerate into broken-wind; but, on the other hand, the horse is sure to suffer from idleness, for the deposition or thickening will become more confirmed, and the parts will acquire an additional, and perhaps an habitual inaptitude for exertion.

Thick-wind depends on Conformation.—Thick-wind, however, is not always the consequence of disease. There are certain cloddy, round-chested horses, that are naturally thick-winded, at least to this extent,—they are capable of that slow exertion for which nature designed them, but they are immediately distressed and blown, if put ever so little out of their usual pace. A circular chest, whether the horse is large or small, indicates thick-wind. The circular chest is a capacious one, and the lungs which fill it are large, and they supply sufficient arterialised blood to produce plenty of flesh and fat, and these horses are always fat. This is the *point of proof* to which we look, when all that we want from the animal is flesh and fat; but the expanding form of the chest is that for which we look in the animal of speed—the deep as well as the broad chest—always capacious for the purpose of muscular strength, and becoming considerably more so when arterialised blood is more rapidly expended in quick progression. We cannot enlarge the capacity of a circle; and if more blood is to be furnished, that which cannot be done by increase of surface, must be accomplished by frequency of action. Therefore it is that all our heavy draught-horses are thick-winded. It is no detriment to them, for their work is slow; or rather it is an advantage to them, for the circular chest, always at its greatest capacity, enables them to acquire that weight which it is so advantageous for them to throw into the collar.

BROKEN-WIND.

Peculiar Breathing.—This is immediately recognisable by the manner of breathing. The inspiration is performed in somewhat less than the natural time, and with an increased degree of labour; but the expiration has a peculiar difficulty accompanying it—it is accomplished by a double effort, in the first of which, as Mr. Blaine has well explained it, "the usual muscles operate, and in the other the auxiliary muscles, particularly the abdominal, are put on the stretch to complete the expulsion more perfectly, and, that being done, the flank falls, or the abdominal muscles relax with a kind of jerk or spasm."

An aggravated Degree of Thick-wind.—This may proceed from unusually extensive induration or hepatisation of the lungs, or thickening of the bronchial membrane, rendering it a work of this labour to accomplish the expulsion of the air, and another set of muscles gradually associated in the act.

Different Theories of Broken-wind.—The most frequent cause or accompaniment of this breathing is an emphysematous state of the lungs. This, although not precisely a doctrine of modern date in veterinary pathology, has only lately been generally received. Some of the Greek Hippiatrists maintained that it proceeded from ulceration of the lungs, which had been obstructed and filled with dust; and others of them, that it was to be attributed to rupture of the lungs from the concussion they experienced in violent cough. Vegetius attributed it to the dryness of the lung, and the diminution of its volume. The Spanish veterinarians believed that it arose from the bad quality of the blood, and the afflux of humours to the lungs, and also a morbid contraction of the bronchial tubes. The Italians seemed to refer it to ulceration and rupture of the lungs. M. Demoussy traced it to diminution of the caliber, both of the air-cells and the bronchial tubes, from an aneurismatic varicose state of the vessels of the lungs. Vitet attributed it to the introduction of air and of blood into the tissue of the lungs; and Bourgelat, to obstruction of the vessels of the chest, and a vitiated state of the lungs.

Among ourselves, old Bracken artfully entrenched himself by saying, that "broken-wind in horses is what in human creatures we call asthmatic and phthisicky." Blunt traced it to the relaxation and thinness and feebleness of the diaphragm, with an enlarged state of the lungs and heart. Bartlet and Gibson likewise traced it to hypertrophy of the heart. One of Gibson's disciples, Mr. Knowlson, fifty-seven years in full business, enters so thoroughly into the opinion of his predecessor, that I cannot refrain from quoting his version.* "It is frequently owing to the hasty or injudicious feeding of young horses for sale, by which means the growth of the lungs, and all the contents of the chest, are so much enlarged, that in a few years the cavity of the chest is not sufficient to contain them, when they expand themselves to perform their proper function. Horses rising eight years are most subject to it, for at six the horse generally finishes his growth in height, and after that he lets down his belly and spreads, and all his parts gain their full size, so that the pressure upon the lungs and midriff is now increased. Dissections of horses that have died broken-winded, sufficiently prove the truth of the above observations, and in some of them the disproportion of them has been so great, that the heart and lungs have been almost twice their natural size." Horses are very differently constructed now, for we do not find this strange disproportionate bulk of the thoracic viscera, but we *do* find that which these excellent morbid anatomists mistook for hypertrophy, an emphysematous state of the lungs, not admitting of that degree of collapse which the pressure of the atmospheric air would otherwise have occasioned. I once saw a case in which the lungs did not collapse at all on the opening of the chest. The horse had had broken-wind to a most annoying degree.

Origin of the present prevailing Opinion.—The majority of veterinary surgeons attribute broken wind to an emphysematous state of the lungs. In the year 1795, Mr. Coleman was dissecting a mare that had been destroyed on account of inveterate broken wind. Mr. Bracy Clark was standing by, with the pupils of the Veterinary College. The lungs were free from inflammation, and white, and it was considered at first that the lungs were not the seat of disorder; until some one observed a small bladder or vesicle on the outside of the lungs, in the external investing pleuritic coat. It was at first mistaken for a tubercle, but Mr. Clark, suspecting from its appearance that it was not solid, punctured it, and it immediately subsided; and it instantly suggested itself to his mind, that the lungs were in a state of emphysema, or that air was contained in a state of extravasation in their substance. Hence Mr. Clark claims the honour of being the father of the modern theory of broken-wind, and which is now adopted by the English and the French schools. I do not know, however, why Mr. Coleman should not participate in the honour, since he is acknowledged to have suggested at the time, that the extravasation of the air into the substance of the lungs was occasioned by a rupture of the air-cells.

Description of the emphysematous State of the Lungs.—In almost every broken-winded horse whose carcase I have examined, I have found these vesicles or vessels (for some of them have been of considerable magnitude) on the surface of the lung beneath the pleura; and in every case I have been enabled to trace them, or a portion of them, into the substance of the lungs, and communicating with the air-cells beneath. I have also succeeded by gentle and continued pressure in returning a portion of the air again into the lungs, but never in diffusing it among the cellular membrane between the pleura and the lung. I was satisfied, therefore, that it was no *extravasation* of air—that it was no gas extricated by any process of decomposition; it was simply dilatation of an air-cell or of some of the air-cells, either appertaining to one lobule or a few contiguous ones. *It was not extravasation into the parenchymatous substance of the lung*, for there was no general pulmonary emphysema; there was no case of emphysema of any other part of the frame; nothing like that inflation of the cellular membrane, more or less extensive, which almost invariably accompanies a puncture into the thorax, wounding the lung, and evidently admitting the air into its parenchymatous substance.

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* Knowlson's Complete Farrier and Horse Doctor, p. 46.

Progress of the Emphysema and Rupture of the Cells.—I have been brought to regard this as the commencement of the morbid change in the air-cells, and taking place at different periods in different parts of the lungs; for I have invariably found some other portions, and more particularly towards the edges of the lobes, where the cells have not been so much dilated, but where they evidently ran into one another. I could there press the air along—no not all over the lobe, but for a little space; sometimes indeed the space of an inch or two, but oftener not beyond one of the lobules into which the lungs are evidently divided; and while I did this there was a crepitating crackling noise, as if I was breaking down the attenuated membrane of other cells. Here we have the true broken cell whence this disease derived its name. This portion of the lung was considerably lighter than the neighbouring ones; and when I cut into it (what I have never been able otherwise to do, although some anatomists speak of it), I have had a distinct view of the separate air-cells, and the termination of the air-tubes, now dilated. If you wish to have proof of this, inflate the lungs of a horse that has had bad broken-wind, and hang them up where there is a thorough draught, so that they may be dried as speedily as possible. Incisions being then made into them, will show the actual state of the cells. I can conceive that broken-wind may proceed from the hepatisation of the lung or thickening of the bronchial membrane, but I have never seen it without this emphysematous state of the lungs, although sometimes existing in so slight a degree that it would have eluded the eye, and was recognizable only by the crackling, when the edges of the lobe were pressed.

Cause.—Broken-wind is preceded or accompanied by cough, a cough perfectly characteristic, and by which the horseman would detect the existence of the disease in the dark. The cough of thick-wind, which I should have noticed before, is a peculiar one; it is short, hollow, yet feeble; the cough of the broken-winded horse is still shorter, cut short, grunting, yet generally followed by a wheeze: and when the animal is suddenly struck or threatened, there is a low grunt, of the same nature, but not so loud as that of roaring. Broken-wind is usually preceded by cough,—that cough becomes chronic — leads on to thick-wind, and there is but a step to broken-wind. It is the consequence of the cough which accompanies catarrh and bronchitis oftener than that or of following pneumonia; and of inflammation and probably thickening of the membrane of the bronchiæ, rather than congestion of the air-cells.

The frequent consequence of Bronchitis.—Laennec, whose illustrations of the diseases of the chest are invaluable to the human surgeon, comes to our assistance, and while describing emphysema of the lungs of the human being, gives us an explication of broken wind, more satisfactory than I have found in any of our veterinary writers. I will endeavour to condense his description. He attributes what he calls dry catarrh "to the partial obstruction of the smaller bronchial tubes, by the swelling of their inner membrane. The muscles of inspiration are numerous and powerful, while expiration is chiefly left to the elasticity of the parts: then it may happen that the air which, during inspiration, had overcome the resistance opposed to its entrance by the tumid state of the membrane, is unable to force its way through the same obstacle during expiration, and remains imprisoned in the cells as it were by a valve. The succeeding inspirations introduce a fresh supply of air, and gradually dilate the cells to a greater or less extent, and if the obstruction is of some continuance, the dilated condition of the cells becomes permanent."

Progress of the Disease.—Then we can easily imagine that they cannot bear any sudden or violent inspiration, or that the continuance of sudden and violent inspirations, and the forcible pressure of more air, will rupture the cells, or that a sudden effort of expiration, as in violent cough, fruitlessly pressing the confined air against the almost obstructed passage, may produce the same effect. A second cell is now involved, and gradually shares in the distention of the first, and becomes ruptured, and the greater part or the whole of the lobule is gradually broken down, and a labyrinth of cells is formed, through which the air can no longer be pressed by the weak mechanism of usual respiration, and scarcely by the aid of the auxiliary muscles of the abdomen causing the second effort.

Peculiarity of Feeding.

Farther Explication of the Disease.—We are now, perhaps, enabled to account for several circumstances attending the disease. The preceding cough, and that of long continuance, is the foundation of the disease, or indicates that irritable state of the bronchial membrane with which chronic tumefaction is almost necessarily associated. Horses that are greedy feeders, that devour large quantities of somewhat nutritious food, or that are worked with a stomach distended by this food, are very subject to broken-wind. There is the pressure of the distended stomach on the diaphragm impeding the motion of the lungs, increasing the stress upon every part, and lacerating the thinned membranes of the distended cells, for the very distention of the cell renders the membrane thin and more easily lacerable. More depends upon the management of the food and exercise than some have been aware of. The post-horse, the coach-horse, and the racer, are comparatively seldom broken-winded; they are fed at stated hours on nutritious food that lies in little compass, and these hours of feeding and of exertion are so arranged, that they seldom work on a full stomach. The agricultural horse is too often fed on the very refuse of the farm, and his hours of feeding and his hours of work are frequently irregular; and the carriage horse, although fed on more nutritious food, is often summoned to work, by his capricious master, the moment his meal is devoured.

A rapid gallop on a full stomach has often produced broken-wind. When the exertion has been rapid and long, I can easily conceive a rupture of the air-cells of the soundest lungs; but I am inclined to believe, that were the history of these cases known, there would be found to have been a gradual preparation for this result; there would have been chronic cough, or more than usually disturbed respiration after exertion, and then it required little more to perfect the mischief. Galloping after drinking has been censured as a cause of broken-wind, yet I cannot think that it is half so dangerous as galloping with a stomach distended by solid food. Old Dr. Grey relates a curious story of a horse having been cured of broken-wind by a brushing gallop after watering. He says that "a certain groom, who had a horse under his care, kept him from drinking for two or three days, giving him all that time as much hay and other provender as he would eat; then he leaped upon his back and rode him to water, where he suffered him to drink his fill: then he clapped spurs to him and galloped full speed till the poor creature fell down for want of breath, and lay some time as if he was dead. As soon as he recovered wind, the groom gave him more water, and galloped him a second time until he fell, and so a third. Well, when the horse had recovered his wind a little, he fell to coughing very much, and by the violence of the shake, cast out of his windpipe and mouth a lump of congealed phlegmatic stuff of a good bigness, after which the gelding was freed from the distemper." The author says he had this from a worthy honest farmer, who had it from the very groom that performed. We, I think, should require better evidence to induce us to believe this story, and many an instance of the success of this treatment to persuade us to adopt it.

Foul Feeding.—It is said that broken-winded horses are foul-feeders, because they devour almost everything that comes in their way, and thus impede the play of the lungs; but there is so much sympathy between the respiratory and digestive systems, that one cannot be much deranged without the other evidently suffering. Flatulence, and a depraved appetite, may be the consequence as well as the cause of broken-wind; and there is no pathological fact of more frequent occurrence than the co-existence of indigestion and flatulence with broken-wind. Flatulence seems so invariable a concomitant of broken-wind, that the old farriers used to think, that by some inexplicable way the air found its way from the lungs to the abdomen, and thence their "holes to let out broken-wind." They used literally to bore a hole near to or above the fundament to give vent to the imprisoned wind. The sphincter muscle was generally divided, and although the trumping ceased, there was such a constant although silent emission of fœtid gas, that the remedy was worse than the disease.

The narrow-chested horse is more subject to broken-wind than the broader and deeper-chested one, for there is not so much room for the lungs to expand when rapid progression requires the full discharge of their function.

Hereditary.—Is broken-wind hereditary? I believe so. I am sure that I have seen cases of it. It may be referred to hereditary conformation, to a narrower chest, and more fragile membrane, and predisposition to take on those inflammatory diseases which end in broken-wind; and the circular chest, which cannot enlarge its capacity when exertion requires it, must render both thick and broken-wind of more probable occurrence.

Treatment.—Is there any cure for broken-wind? What medical skill can repair the broken-down structure of the lungs? Sir John Floyer thought he could cure the *broken-wind* "by a paracentesis in the thorax, for if the external air be admitted, it will compress the flatulent tumour; and, through the same hole, a styptic and carminative hydromel may be injected to restore by its stypticity the tone of the membranes, and discuss by its aromatic acrimony the windy spirits or air retained in the lungs." Of about the same value is the recommendation of Mr. Parkinson to feed with hay and corn wetted with chamber-lye; and to pour shot down the throat, "which will drive the bowels so far from the midriff, that the disease will not be discernible." Linseed oil given in large quantities has been extolled as a cure. It may act as a purgative, and so empty the stomach and bowels, and give temporary relief. Mr. Knowlson says, that "two or three cloves of garlic being given in each feed, or three ounces bruised and boiled in a quart of milk and water, and given every morning for a fortnight, has been found very serviceable. So easy a remedy should never be neglected, *for by warming and stimulating the solids, and at the same time dissolving the tenacious juices which choke up the vessels of the lungs, it greatly relieves this complaint.*" Enough of this foolery!

Food.—If we cannot cure, we may in some degree palliate broken-wind; and first of all we must attend carefully to the feeding. The food should lie in little compass,—plenty of oats and little hay—no chaff. Chaff is particularly objectionable, from the rapidity with which it is devoured, and the stomach distended. Water should be given in moderate quantities, and the horse should not be suffered to drink as much as he likes until the day's work is over. Green meat will always be serviceable. Carrots are particularly useful. They are readily digested, and appear to have a peculiarly beneficial effect on the respiratory system.

It is from the want of proper attention to the feeding that many horses become broken-winded, even in the straw-yard. There is little nutriment in the provender which they there find, and to obtain enough for the support of life, they are compelled to keep the stomach constantly full, and pressing upon the lungs. It has been the same when they have been turned out in coarse and innutritive pasturage. Here the stomach was perpetually gorged, and the habitual pressure on the lungs, cramped and confined their action, and inevitably ruptured the cells, when he gambolled with his companions, or was wantonly driven about.

Exercise.—Next in importance stands exercise. The pursive or broken-winded horse should never stand idle in the stable a single day. It is almost incredible how much may be done by attention to food and exercise. There is scarcely a hunt in which there is not a thick or broken-winded horse, that by judicious feeding and training, is enabled to acquit himself very respectably in the field. The broken-winded horse may thus be rendered comfortable to himself, and no great nuisance to his owner,—but inattention to feeding, or one hard journey—the animal unprepared, and the stomach full.—may bring on inflammation, congestion, and death. Occasional physic, or alterative medicine, will often give considerable relief.

ST. GEORGE'S HOSPITAL.

CLINICAL LECTURE

BY

MR. BRODIE,

Delivered March 24th, 1832.

LIGATURE OF THE EXTERNAL ILIAC.

Pulsating Tumours not always Aneurismal.—Secondary Aneurismal Hemorrhage.

GENTLEMEN,—The case of Batchelor (the man on whom I operated for aneurism on the 1st of this month) is a very interesting one; I shall therefore make a few remarks to you upon it to-day. He was an innkeeper at Windsor, and was sent here by Mr. O'Reilly, who thought the man's case was a formidable one, in which he was perfectly correct. He was admitted with a large pulsating tumour in the groin, in the situation of the line of the femoral artery, and you know that pulsating tumours in the groin usually denote aneurism. There were, however, some circumstances in this case which led me to doubt the fact of the tumour being aneurismal. It had existed for a long time, and fourteen months ago it first began to pulsate. After a time the pulsations ceased, and then they came on again. The history of the case altogether did not resemble one of aneurism; but if it be decided that this pulsating tumour was not an aneurism—why then what was it? Oh, but the cellular membrane around an artery may be inflamed, thickened, and infiltrated, and the pulsations of it may be mistaken for those of an aneurism. There was a case some time ago in this hospital, which was supposed to be an aneurism—there was a large tumour in the thigh; but, on examination, I found that the patient had had disease of the hip-joint, and that the swelling had extended down around the coats of the artery, which had caused this pulsating tumour.

Solid tumours may pulsate sometimes. These are either medullary tumours, or tumours of fungus hematodes; these latter ones you know are vascular, and have much blood running to them, and they pulsate;

and if any very considerable artery supplying these tumours gives way and bursts, there is a cavity, and an aneurismal swelling formed, and, consequently, a pulsating tumour, but by attending carefully to the history of the symptoms, you may often distinguish very clearly between a pulsating tumour which is an aneurism, and one which is not an aneurism. If a tumour exists before pulsation comes on—if you can diminish the size of the tumour, but not completely—if there is no whizzing sound denoting the rush of blood into a cavity, *then it is not an aneurism.* If you apply your ear to a fungous tumour, there is not the same sound given to it as in an aneurism, and in doubtful cases you may learn much by applying the ear, or the stethoscope, over the tumour. I had a patient in this hospital who had a large pulsating tumour of the abdomen, which I and the house-surgeon believed to be an aneurism. We made a very careful examination, and were fully satisfied that it was one, but every one else who examined it differed in opinion from us; the stethoscope had however just at that time been introduced into this country, and we applied it to the patient's abdomen, and no one did for a moment after that doubt the tumour being an aneurismal one. The patient died, and on opening the body after death, a large aneurism of the great iliac artery was discovered. You may sometimes distinguish these real aneurisms from false ones; this, in some cases, is easy; in others, however, it is very difficult and intricate. I knew of a case where the femoral artery was tied in a supposed case of aneurism. The tumour was found to be one of fungus hematodes; the surgeon who operated in this case was nevertheless a very clever and skilful man. A case occurred in the Asylum for the Recovery of Health, where there was a tumour in the upper part of the thigh. Mr. Keate and I examined it very accurately, and I tied the femoral artery; it proved to be a medullary tumour, and not an aneurism. In this case, both Mr. Keate and I had paid the greatest attention to the case; yet we were deceived. A case was sent up to London (which I saw) by a very clever man who was surgeon to a very large hospital in the country. The patient had a pulsating tumour in the neck, which was supposed to be an aneurism of a carotid artery. I saw her, and I pronounced the tumour to be an aneurism. Sir Astley Cooper saw her also, and he decided that it was an aneurism. Every other surgeon in London, however, who saw the case, said that it was not aneurism. The patient very wisely believed them, and would not be operated upon. She left London, and went down again into the country, and this case eventually turned out not to be a case of aneurism. Such cases as these, however, are of rare occurrence. I merely mention them, in order to show that you are not to jump to the conclusion that a tumour is aneurismal, because it pulsates. In this case, whether the tumour was one of aneurism or not, it would bleed; if it were medullary it would bleed, somewhat slower to be sure, but not the less fatally for that. Of whatever nature however the tumour might be, it was highly necessary that some operation or other should be performed upon it. I was doubtful about the tumour, not about the point of practice.

Well, now, of the operation. As the man was placed on the operation table, the thin skin covering the tumour broke, and a jet of scarlet arterial blood issued from the orifice. Mr. Keate held a sponge, dipped in cold water, over the tumour during the performance of the operation, and all further bleeding was thus checked. The tumour was a larger one than I had ever seen in an inguinal aneurism. The trunk of the iliac artery was never tied in this country until the beginning of the present century. Mr. Hunter was the first to propose this operation in this country. Mr. Abernethy was the first who tied the external iliac artery in inguinal aneurism; it was considered a great exploit in surgery. The patient, however, died from some accidental cause not at all connected with the operation. Mr. Abernethy was not, however, disheartened by the result. "Oh," said he, "I will perform the operation again." He did so, and again the patient died. In this latter case there was inflammation of the cellular texture around the artery; and it was difficult, therefore, to apply the ligature. "But the limb was nourished," said Mr. Abernethy, "and the operation will therefore succeed." The third operation which he performed did succeed, and the patient was cured. I have performed the operation now myself three times with success; and in the case before us, I hope I may add with success also. The external iliac artery gives off no branches until it reaches Poupart's ligament, and, therefore, you may tie it with a greater chance of success than you can the femoral artery, where you are not sure but that a small arterial branch may be given off just about the ligature, thus preventing the formation of a long coagulum, which is necessary to perfect the cure of aneurism, and to plug up the artery. Mr. Abernethy's method of performing the operation was, to divide the abdominal muscles high up, and to push the peritoneum inwards from the artery. Sir Astley Cooper, however, divides the muscles lower down, and pushes up the peritoneum. In one patient, on whom I have operated, according to Mr. Abernethy's method, a ventral hernia occurred. Whatever difference of opinion may exist with regard to the two methods of performing the operation in these cases, there was no doubt whatever in this case but that the tumour was situated above Poupart's ligament, and I therefore performed the operation according to Mr. Abernethy's method. I made the incision high up; lest I should have to tie the trunk of the great, as well as the external, iliac. I cut the abdominal muscles singly or doubly, according as they lay; I separated the peritoneum, traced the iliacus internus muscle, up to the psoas magnus, and found the artery pushed down into the pelvis, from the bulk of the tumour and from the skin being so much elevated. I was obliged to make a fresh incision, and carry it inwards over the tumour. I reached the artery, and passed an aneurismal needle under it, drew the ligature tight, and tied the vessel. This part of the operation requires great care, from the neighbourhood of the iliac vein, which, if you were so unfortunate as to wound, would prove fatal to your patient from excessive hemorrhage. In this case the cellular membrane around the artery was not inflamed; and I had not, therefore, the same difficulty to contend with that Mr. Abernethy had, and it only required care and caution. The artery should be detached as little as possible from the surrounding parts, for much of the subsequent danger to the patient depends upon this. Sir Everard Home, who followed Mr. Hunter's method of performing the operation, never separated the artery from the surrounding substance more than he could help, and this plan has been followed universally ever since the publication of Dr. Jones's experiments. If you separate the artery much from the surrounding cellular substance, ulceration and sloughing may come on; and in cases where the old surgeons used to lift up the trunk of the artery on the handle of the scalpel, and show it to the pupils, they, of course, separated the artery very much from the surrounding textures, and ulceration and sloughing were invariably the result. Mr. Cline, senior, used to include a piece of cork in the ligature. Scarpa, I think, recommends a bolster for the same purpose. In the present day, as you know, nothing of this kind is done, for the artery is separated from the surrounding parts as little as possible. The ligature should not be too broad, but of such size and strength as, when drawn tightly, will cut through the inner coat, and you then of course bring together two cut surfaces. The surgical course of lectures which is delivered here in the evening will inform you upon all these points more at length than I can do in the present lecture; and I may also at the same time refer you to Dr. Jones's work on the subject. In this case when the ligature was tied, all pulsation in the aneurismal tumour ceased immediately, and the hemorrhage stopped also. The ligature was tied between the origin of the external iliac and Poupart's ligament. The patient was not purged as he ought to have been before the operation, for he was not in the house long enough.

From the time of the operation the tumour decreased in size; the ligature came away on the sixteenth day. It might have come away sooner, but I did not pull it, as I was fearful of hemorrhage; and when it did come away it required no force to detach it. The only other interesting facts connected with the case are those which occurred to the tumour after the operation. On the 10th March, the coagulum of the tumour turned out, and excessive hemorrhage came on from the lower part of the tumour. I did not think that the anastomosis of the epigastric and circumflexa ilii arteries could bring it on. But was I to apply a ligature in this case? There was a confused mass of bloody slough and hemorrhage; and in the midst of all this, I could not tell where to tie the artery. I thought the hemorrhage might be restrained by pressure. In some private cases which I had, and in which secondary hemorrhage came on, I applied strips of adhesive plaster all round the thigh, with a compress over the surface whence the hemorrhage took place. In this case I began from the knee, and laid on strips of adhesive plaster all round the thigh up to the groin, and placed a bolster over the tumour; this was done ten days ago; no recurrence of the hemorrhage or sloughing has taken place since, and good healthy granulations are now springing up, which I am in hopes will stop up the orifice of the bleeding vessel, and prevent any recurrence of the hemorrhage—at all events it seems now completely under control. For my other patients I had a leather strap made, with a pad affixed to it to press over the tumour which could command and control the hemorrhage in an instant. I have had one applied on this man, but have not had occasion to use it. And here let me observe upon hemorrhage from aneurism, that I have seen many cases of it. In one case I tied the artery above, and still the hemorrhage continued, and the patient died. You will, in general, find that slight pressure will be sufficient to restrain it. In a case of aneurism which occurred in this hospital, Sir Everard Home tied the artery, and secondary hemorrhage came on. However, there was an old nurse at that time in the hospital, who, from long experience, understood these matters, and she tied a piece of tape around the limb, and the hemorrhage completely stopped. In a case of amputation I tied the vessels and cut off the ends of the ligature, and the wound united by the first intention. On the tenth day it burst, and secondary hemorrhage came on. I put on a roller and bandage, and it completely stopped. In this case the hemorrhage completely ceased. The hemorrhage did not arise from the ligature, but from the aneurismal sac. I never knew this to happen but once before, and then there was an abscess in the sac; here perhaps it was better that the hemorrhage took place from the sac (although I should have preferred that it had not taken place at all) than from the ligature, as it would not have been practicable to apply either pressure or a ligature higher up. I hope there will be no further return of the hemorrhage, but still the man is not yet out of the wood,—not totally free from danger.

REMARKS
ON THE
INVESTIGATIONS AND ALLEGED DISCOVERIES
OF
PROFESSOR DELPECH,
IN THE PATHOLOGICAL ANATOMY OF THE
MALIGNANT CHOLERA.

By J. Waters, *Esq., Licentiate of the College of Surgeons in Ireland.*

As there has been a considerable degree of attention excited by the statements lately published by Professor Delpech on the pathological anatomy of cholera, I beg leave to state a few facts connected with his investigations in Glasgow, which I had an opportunity of witnessing.

Shortly after the disease appeared in Glasgow, Professor Delpech arrived, his mind being occupied by a preconcerted ingenious theory, determined to make all appearances in the dead body coincide with his views. Completely blind to any changes of structure which could at all militate against them, he immediately commenced by most ingenious and plausible arguments to convince those with whom he had an opportunity of conversing, of the correctness of his idea of the proximate cause of the disease—viz. that it consisted in an inflammation of the ganglionic portion of the nervous system, producing derangements of the functions of sanguification, nutrition, respiration, and circulation.

I was first inclined to coincide with his view of the disease, supported as it was by all his wonted talent and ingenuity, and corroborated as he said by appearances after death, indicative of such an affection of the ganglia, but unfortunately for the validity of his speculations, an opportunity at the moment existed in the hospital, of referring the matter to the test of inspection.

Upon examining the body of a woman who had died a few hours previous, the following appearances were found:—

On removing the calvarium, the superficial vessels of the brain and its membranes were found quite empty; even at the posterior part they were free from the usual congestion from gravitation. The surface of the serous membrane was quite destitute of moisture throughout its whole extent. The substance of the brain was perfectly healthy. The spinal marrow unaltered.

On examining the ganglionic apparatus, more particularly the semilunar ganglia, solar plexus, and splanchnic nerves, to the eyes of Professor Delpech they presented "an intense degree of vascularity, thickening of their neurilema, and tumidity of their substance," the whole indicating a high degree of inflammation, quite sufficient to account for the death of the patient, "on account of the delicate and easily destructible texture of the organs concerned."

I should feel considerable diffidence in taking upon myself to contradict the statements of an individual so eminent in his profession as Professor Delpech, did I not think myself quite competent to form an opinion as to the state of those parts, from having had for some years very extensive opportunities of pursuing post-mortem investigations, and acquiring a perfect familiarity with the natural appearance of the organs in question. I can therefore with confidence assert, that there was not the slightest deviation from their natural healthy structure. The ganglia presented their usual pale, fleshy, appearance, and on being cut into, no blood or fluid of any kind could be expressed from them. The neurilema was perfectly free from any increased vascularity or thickening, and when wiped with a dry towel, was perfectly white, and destitute of the inflammatory blush ascribed to it by Professor Delpech.

The mucous membrane of the stomach and intestines, however, presented more decided and characteristic appearances. In the stomach the vascularity was not much increased, but the membrane was reduced to a state of pulpy softness, having lost all its usual tenacity. In the duodenum the membrane commenced acquiring an increased degree of vascularity, which gradually became more intense as it proceeded towards the ileum, in which latter situation it presented a dark-red ecchymosed appearance. In the large intestine there were irregular patches of ecchymosis along the course of the muscular fibres. The bladder was empty, and firmly contracted, as is usually found; spleen small, flaccid, and not containing any blood; liver healthy; the large blood-vessels full of dark coagulated blood; gall-bladder full of bile.

On the mucous membrane of the intestine being shown to Professor Delpech, he pushed it from him, remarking that it was a mere complication of the disease, and wholly dependent on the deranged functions of the ganglionic system.

In the other cases enumerated by Professor Delpech, I assisted at all the examinations, and can positively assert that in no case, so far as I could perceive, was there the slightest deviation from the normal appearance and structure of the ganglia and nerves inspected.

The degree of cerebral congestion varied in the different cases, but was not intense in any. In all there was more or less softening, and increased vascularity of the mucous membrane of the stomach and intestines; the bladder firmly contracted and empty; liver healthy, and gall-bladder full of very dark bile. There was also in each case an immense accumulation of blood in the large veins in the neighbourhood of the heart, in the auricle and ventricle of the right side, and extending into the branches of the pulmonary artery. It was coagulated, and closely resembled black-currant jelly, and appeared as if it were moulded to the shape of the parts in which it was lodged.

Since the examinations above alluded to were made, I have had opportunities of pursuing the subject on an extensive scale, and I have not in any one instance found the views of Professor Delpech substantiated.

In all the bodies which were examined at the Glasgow Cholera Hospital, there has been found a most unusual exemption from any chronic morbid alteration of structure in the lungs. In upwards of 80 inspections, there was but one instance found of tubercles, while in the medical and surgical hospitals of that city, on examining the bodies of patients who have died of the different diseases incidental to so large and dense a population, there are found in a very large proportion of cases, tubercles in their different stages of advancement. There was but one case of cholera occurring in a subject of phthisical diathesis, although the latter is particularly prevalent there.

In endeavouring to harmonize (if I may term it) his theory with the progress of the symptoms, he has completely perverted the order of invasion of the latter. He commences by stating, that a fixed pain in the situation of the semilunar ganglia and solar plexus, always precedes the manifestation of other symptoms. The diminution of the circulation next succeeds, no evacuation as yet having taken place, and the blood being still entire, next in order follows, according to his statement, the increased discharge from the stomach and bowels.

That such is not the order of invasion must be obvious to any one who has had an opportunity of observing the disease since its appearance in England.

The disease is invariably preceded by diarrhœa, of certain duration, existing without any pain or uneasiness, being unaccompanied even by the usual griping pain which attends diarrhœa under ordinary circumstances. During this diarrhœa the pulse is not diminished in strength; the respiration is unaffected, and the secretion of urine continues as usual; the temperature of the body is as yet unaltered, there is no spasm, no uneasiness at the epigastrium, as described by Professor Delpech, in fact no pain, or any other symptom which can possibly warrant us in supposing an unnatural condition of the ganglionary apparatus to exist.

We find the blood after death accumulated in the large vessels, to which alone it had found access, and principally in the pulmonary artery and its branches, so far as its viscidity would allow it to advance; and, corroborating this view, we find in the consecutive fever which nature establishes, as it were to rouse the system from the collapsed state, that one of the most marked symptoms is violent and inordinate action of the right side of the heart, evidently owing to its exertions to overcome the obstruction which exists to the passage of the blood, the vessels being clogged by blood previously deprived of its fluidity, and preventing the circulation of that which has now recovered the fluid it had before lost.

The indication of the proper mode of treatment to which this view would lead, seems to be the checking, by means of astringent medicines, the increased discharge from the mucous membrane of the stomach and intestines; at the same time to allow the patient a plentiful supply of fluids, which will greatly alleviate his sufferings as well as contribute towards his recovery. As an astringent in such cases, no medicine can be found more efficacious than sulphate of copper, combined, if necessary, with opium. I have seen it completely put a stop to the discharges after a few doses had been administered, in cases where opium, both alone and in combination with the vegetable astringents, has failed even in checking the evacuations. I have also known it to succeed where acetate of lead had been exhibited without success. It should be given in similar doses to those recommended by Dr. Elliotson in the treatment of chronic diarrhœa. Tepid water acidulated with lemon juice should be allowed the patient, ad libitum. In addition, the usual methods of keeping up the external heat should be resorted to, and particular attention paid to the temperature of the apartment, which should never be allowed to sink below 70° F.

Parsonstown, May 6th, 1832.

SKETCH
OF THE
ORIGIN, PROGRESS, CAUSES, SYMPTOMS, TREATMENT, AND POST-MORTEM APPEARANCES, OF THE
CHOLERA IN PARIS.

To the Editor of THE LANCET.

SIR,—I transmit to you the observations which I have made on cholera since its appearance in this city. I have not advanced a single fact unauthenticated; I have hazarded no theory; I am attached to no particular dogma. I have not, therefore, enlisted physiological or pathological characters in the confirmation of favourite opinions. I simply submit the most remarkable circumstances, and the most striking results of the therapeutic agents employed, in order that medical men, by the comparison, and by the accumulation of proved inductions and general testimony, may exercise their unprejudiced judgment in the employment of the most rational and most efficacious remedies. If you consider that this parallel between the French and English mode of treatment is unnecessary, or that the difference is so slight, so undefined, that the publication of my paper would be productive of no beneficial result, I request you not to occupy the pages of your valuable journal with it. I am, Sir,

JORDAN R. LYNCH, M.D.
64, Rue St. Andre des Arts,
Paris, May, 1832.

Cholera made its first appearance in France, at Paris, on the 26th March, among the poorest and most distressed portion of the inhabitants, none of whom seemed to have had any communication with persons arrived from England, or any other country, as the patients resided in a part of the city little frequented by foreigners.

Notwithstanding that the disease did not appear to be propagated by contagion in the first instance, in no house in which it appeared was it confined to one individual, but in all cases attacked many. It was a fact remarked by all the medical attendants, that when a medical man's services were required to-day, he might rely on having one, two, or three, to-morrow added to his list.

This circumstance led many to infer, that cholera was more or less contagious, but those who entertained a contrary opinion endeavoured to account for it by the moral predisposition of people residing in the same house, who were terrified at the sight of the ghastly disease. I shall cite one striking example, that of a distinguished person, the Neapolitan ambassador, Prince Castaglioni, a bigoted and cruel aristocrat, who was in the habit of daily tracing on the map of Europe the progress of cholera for the last eighteen months, and sent every week for his physician to indicate its march. He was continually employed in calculating on its fearful arrival in France. His guilty conscience saw in it the avenger of the murdered Italian patriots. As soon as it broke out he sent in consternation for his attendant. Next day he was attacked with diarrhœa, which was succeeded by an attack of cholera which it was impossible to arrest. In most cases the exciting causes were moral affections of different kinds, viz. love, passion, dread of cholera, losses in commerce, play, indulgence in venery, too unremitting application to study, of both of which latter instances the French students afforded melancholy examples; also drunkenness, in which cases they were frequently attacked without any precursory symptoms; inflammation and derangements of the intestines; the ascescent, the vegetable, and the weak diet, to which cause I attribute the greater ravages that were witnessed in Paris than in London, which opinion is confirmed by this fact, that many medical students who had resided for two or three years in France, were carried off, while those who had arrived within the last six or eight months from England, Ireland, and Scotland, were not seriously affected.

All these were considered predisposing causes; but, on the other hand, I have seen those attacked who resided in different *étages*, in different families, whose profession in life, circumstances, age, and residence in the same house, were different, and who were uninfluenced by strong moral causes.

The symptoms were, digestive derangement, existing for two or three days, more or less grave, sometimes so slight as not to have fixed attention, and the neglect of which ended in diarrhœa, one time ceasing then recurring, without any other phenomena than general debility, sensation of oppression, nausea, cardialgia, frequent vomitings, evacuations, presenting two different aspects; 1st, a transparent liquid, with slight yellow tint, at the bottom of which gelatino-albuminous flakes; 2nd, a liquid thicker, whiter, tinged with blood or bile; odour not very disagreeable; acescent.

After the invasion the grave symptoms came on more or less rapidly; very variable coldness of the extremities, then of the trunk; the circulation, which from commencement was diminished, became imperceptible; in many cases ceasing altogether, and first of all in the extremities. Action of the heart almost indistinct; the colour of skin in the bilious and dark-complexioned becomes blue; black in the fair; and in the leuco-phlegmatic dark yellow. In the former, or where it is in the worst form, the skin becomes of a leaden hue, then a marked blue; of this description, at the commencement, 10 out of 100 were not saved.

All the above were modified by different degrees of intensity. If not terminated by death, they were followed by the second period, that of reaction, characterized by a return of heat. In the former period there is a suspension of almost all the functions.

The indications that arise from its study are so positive, that there can be no doubt of the diagnostic means to be employed. One hundred cholera patients during the first period of the disease, offer the same principal features in different degrees; whereas in the second period the characteristics are more equivocal, and the shades of diversity more numerous. Sometimes the vomiting ceases suddenly, in general gradually succeeded by frequent emission of gas per anum, and hiccough; the diarrhœa follows the same course, and ends, sometimes, in a constipation, not the effect of the medicines administered. The appearance of the urine is variable, extending from two to five days. It is not in every instance an infallibly happy prognostic.

This is the mildest form of reaction; it has been divided into four kinds, typhoid, ataxic, adynamic, and inflammatory; the last should be placed second, from its relative degree of frequency, from its importance, from the polemical disquisitions to which it has given rise. The study of this stage, I think, most easy. Nature seems as if to throw off the incubus of incipient death; she excites herself; she rallies from the state of general atrophy. At this period all the profundity of the skilful physician is required to adapt his succour to the powers of the constitution; here, specifics, favourite remedies, resolve themselves into charlatanism. This is frequently followed by perspiration, which lasts some days, at the end of which, sometimes, the organs resume their functions; but the termination is rarely so favourable. This, like the others, is subject to the greatest variety; the transition to heat is the leading mark; it is in general gradual, imperceptible in shades, approaching more or less the typhoid character; heat inconsiderable; pulse small, feeble; vomitings tinged with bile; phlogosities of lips and tongue; tenderness of epigastrium; more or less stupor; prostration great; integrity of intelligence. If these continue, life extinguishes like a candle. In other cases, excitement; thirst; violent reaction, too violent for the strength of constitution; inflammatory type, *en resume*.

I will submit, without comment, the comparative efficacy of the remedies adopted, with their results.

At *l'Hotel-Dieu*, entered from 26th March to 23d April, 1920; died 1161; in which hospital the treatment was greatly diversified.

Baron Dupuytren tried, first, superacetate of lead and cautery, without effect; latterly, he used strong tea, ammonia, laudanum, and occasionally bleedings.

MM. Magendie and Petit applied alcohol ʒi; ol. terebinth. ferrum calidum dorso. M. Honore, sulph. æther, malaga wine. Bally, sulph. quinine, ol. tiglium.

At the *Pitie*, MM. Andral, Louis, Lisfranc, and Velpeau, almost the same. Here ipecacuanha, latterly, with benefit. From 29th March to 14th April, entered 592; dead 313.

At *Val de Grace*; under the service of Broussais, who considers it a gastro-enterite, and subjected it accordingly to what he calls physiological treatment, bleeding, antiphlogistics; but he was obliged (to use his own words) to have recourse to the dangerously stimulant treatment. From 29th March to 26th April 127 sick; cured 24; dead 51; remaining 52.

St. Louis. M. Alibert; ipecacuanha; in commencement 18 grains; sulph. quinine; application of leeches to epigastrium; administration of ice during the thirst. From 29th March to 26th April, entered 233; cured 80; died 81; remaining 57.

The same treatment has been approved of in Hungary. Bleeding there, as well as in Podolia, has not answered their expectations. In Hungary, in a population of nine millions and a half, 25,000 attacked, half dead.

In attendance at the arrondissement to which I was attached, when we bled we generally lost our patients. The change was marked by the adoption of that mode of treatment. The *convalescence* was grave; a relapse fatal; progress tedious; orbital excavation remained for at least fifteen days. On the 9th of April, dead in domiciles and hospitals 1020; on 10th, 985 dead; 11th, 850 dead; 12th, 863; 13th, 785. From this time it daily and rapidly decreased. The severity mitigated up to this. They seemed devoted before to certain death. On the 15th, the sources of life seemed less compromised; pulse more perceptible. On the 23d only 105 attacked; on the 24th 81 attacked; 25th 62; 27th 71; and then on, gradually lessening. Galvanism, protoxyde of azote, oxygenated water, powdered charcoal, calomel and opium, arteriotomy, general bleeding, counter-irritation, produced little or no happy effect.

Post mortems.—The inductions from this source have not established any decided fact. The leading characters are the same as lately described by Dr. Green in THE LANCET. Equivocal appearances presented in one case, have not been found in another, and their absence or presence, backed by ingenious conjectures and plausible reasonings, have caused the broaching of many fanciful theories, and the physical cause of the disease rests in the same impenetrable obscurity. I will only notice a few appearances at variance with those generally found. Serous effusion in brain; generally healthy spleen, small, without blood; generally gorged intestines; dark appearance of inflammation, generally congested, without redness; mesenteric ramifications enlarged, in general much smaller. Semilunar ganglia in two or three instances, traces of inflammation; generally natural; glands of Peyer, unenlarged, uninflamed; vascular patches in general developed as in typhus fever about the 10th day. Kidneys natural, in general congested; bladder always contracted.

APPEARANCE OF THE

CHOLERA IN LIVERPOOL.

To the Editor of THE LANCET.

SIR,—The cholera, the malignant, the Asiatic cholera, has now beyond all dispute made its appearance in Liverpool, and our Board of Health, in spite of some misgivings, no longer doubt it; but the evincing of, and the perverse adherence to, a sceptical spirit in the belief of its existence, seems to be inseparable from its presence everywhere; at least such has been the case in all places which have been visited by this pestilence in England. It was not to be supposed that Liverpool would be singular in this; hence, the cry of "*No cholera*," "*We have no cholera*," was to be heard everywhere, from the rising of the sun to the setting thereof; it was raised by the *faculty* and echoed by the populace, and up to the present moment this false, preposterous, absurd, dangerous report, is perseveringly propagated and encouraged.

The following account of five cases of cholera which have occurred in my own practice since Monday last, 14th instant, is condensed from my note-book. The evidence they afford is to the point, and very convincing, and establishes pretty clearly two things, 1st, That the cholera of Asia has commenced its ravages in Liverpool, and, 2dly, That this disease *is* contagious, relatively considered.

CASE 1.—*Samuel Prince*, iron-founder, aged 30 years, rather stout and robust, residing in Union Place, Lower Chisenhale Street, Liverpool, was seized with cramps, vomiting, and purging, at eleven o'clock on Sunday evening, 13th instant, but had been troubled with a diarrhœa the whole of that and the preceding day. Did not see him before nine o'clock next morning; vomiting and purging were then severe and almost incessant; great prostration of strength; countenance fallen, intensely anxious and dejected—a true picture of horror and dismay; pulse slow, and almost imperceptible; skin without the least moisture, but not harsh, soft, cold, conveying to the touch that peculiar feel which is given from a human body some two or three hours after life has become extinct, or in that state when the animal heat is almost entirely confined to the heart and internal parts. Face, neck, and the upper and lower extremities, of a distinct blue colour. Respiration laborious and hurried; tongue covered with a dense white coat, and the breath nearly as cold as the skin; the matter vomited was exactly similar in consistence and colour to that which had passed through the intestinal canal, very thin, and of a palish white, not the least offensive, and nearly inodorous, and on standing a few minutes, deposited a flakey sediment, closely resembling the grounds of white wine posset. Urine suppressed. Complained of acute pain over the region of the liver, and along the margin of the ribs—a feeling of stricture; stomach and bowels slightly tender, and not particularly painful on moderate pressure. Head not materially affected, but he frequently called out for some cotton-wool to be put into his ears; this being done, he observed his hearing was more distinct; the whizzing and clashing sounds subsided immediately; thirst intolerable; preferred cold water as a drink to any other fluid. With such prognosis before me I could not mistake the nature of the disease.

If the treatment of a solitary instance in any disease can be brought forward with recommendations of sufficient moment to entitle it to general adoption, that which was exclusively followed in the above case of cholera, claims no small share of attention. Subm. hydr. gr. x, opii gr. j, and carb. sodæ gr. xx, were administered to him immediately; the dose to be repeated every third hour. The whole body was fomented and well rubbed with hot water, very hot water, saturated with common table salt. Thus treated for a few hours, the patient became sensibly better, and at six o'clock, same evening, nineteen hours from the attack, he was so far recovered as to be apparently out of danger. To-day he is down-stairs, and wishes much to walk into the open air.

Case 2.—*Mary Rennard,* 53 years old, a strong, jolly-looking woman, had been one night and two days in attendance upon Prince; was seized suddenly with severe cramps in her extremities at three p.m. on the 16th inst., attended with vomiting and purging, &c. I saw her at five same afternoon, and had no difficulty in identifying in her the same looks and symptoms which were so peculiar and striking in Prince, only, if possible, in a more aggravated form. She was treated in a similar manner, and with the same doses of medicine, except that in addition she was given brandy and other spirituous liquors in tolerably large quantities; all, however, proved of little or no effect, and this poor woman fell a victim to the disease at four o'clock next morning, having been ill just thirteen hours. Her pulse was imperceptible from the first, and she died without any signs of reaction.

Case 3.—*George Prince,* a fine straight athletic youth of nineteen, son of Samuel Prince, also an iron-founder, and living with his father, was attacked with all the common symptoms of cholera at four o'clock a.m. on the 17th inst., and died at one o'clock next morning, 21 hours from the attack. Much difficulty was experienced in giving this patient his medicine, and, as a necessary consequence, therefore, it was administered at irregular and too long intervals.

Case 4.—*Thomas Prince,* a strong well-made young man, 21 years old, brother of George Prince, worked at the same foundery, and lived also with his father Samuel Prince, was attacked with the cholera at 11 o'clock a. m. on the same day, and only ten hours from the time his brother was taken. The same kind of treatment was adopted in him as in the other cases, and in 28 hours the nature of his complaint had entirely changed. He has quite recovered. This patient took 100 grains of calomel and 10 grains of opium in about 20 hours. His mouth is not the least affected with the mercury; nor was the in any of the other cases.

Case 5.—*Mary Moore,* aged 60, thin, and of emaciated appearance, a charwoman, was hired to clean the house of Rennard, and came from a distance for that purpose on the morning of the 18th, but at 12 o'clock, noon of next day, was attacked with the cholera, and was removed to her own house soon after. The treatment of this case, at my request, was intrusted to other hands. The woman died, however, at four next morning, 16 hours from the attack.

It may be as well to state, that the iron-foundry at which the Princes worked, adjoins the Prince's Dock, and about 150 yards from the river Mersey. The house in which they live is in a dirty low neighbourhood, close to the Liverpool and Leeds canal.

I was much surprised to learn that the two young Princes *only* out of the first four of the above-mentioned cases, were reported as instances of cholera to the Metropolitan Board of Health, and yet it is quite evident that the disease of which they died, was communicated to them from the father. I had this information from our worthy mayor, who is chairman of the Liverpool Medical Board.

I am, Sir, yours,
Thomas Weatherill, M.D.
Liverpool, May 20th, 1832.

P.S. I have just come from seeing another case of cholera. It has occurred in a house adjoining that in which the aforementioned Mary Rennard died of this complaint on the 17th instant.

EFFICIENCY OF CALOMEL
IN THE TREATMENT OF
MALIGNANT CHOLERA.

By Joseph Ayre, *M.D., Hull.*

At the close of my last letter, published in The Lancet of the 28th ult., I declared my belief that the method of treating cholera by small and frequently-renewed doses of calomel, which I recommended, required no auxiliary means to render it efficient; but that, on the contrary, I had reason to fear that the use of them had frequently compromised its success. This view of the subject was only consonant with that which I had taken of the pathology of cholera, and which I was willing to have considered as the test of its correctness. For if the use of emetics, or bleeding, or stimulants, and opiates in large doses, be needed as auxiliaries to the calomel in restoring the secretion of the liver, then my notion of its nature must be unfounded, and especially in respect to the relation which I have assumed it to hold to the cholera of this country, and in which, I well know, no such aids are needed.

The inordinate use of these means in the early cases of the disease, and the seeming success in some of the milder instances of it, led many, no doubt, to believe, that being, as they supposed, beneficial in some cases as the remedy in chief, they must be useful, if not necessary, as auxiliaries in all, from which would result an admixture of other active means with the treatment by small doses of calomel, and a consequent disappointment in the result of it in many cases, and a distrusting of the evidence in others, of the pre-eminent value of that remedy. The mustard, or other active emetic, indeed, if given early, and when the congestion is incipient, may produce, as the spontaneous vomiting generally does in the English disease, a sudden reaction in the secretory system of the liver, and a speedy termination of the complaint. But if this emetic be given, as I saw it in the north and elsewhere, in the stage of extreme collapse, and when no such reaction could be looked for, it can serve but to wring from the exhausted patient whatever of strength he has left him. In the same way bleeding may be employed at the commencement of the disease with an occasional chance of its relieving the congestion, and of thereby becoming beneficial; but when the congestion is fully established with its attendant collapse, the practice is full of danger; for however it may for the moment relieve the overcharged heart, by removing a part of the load which oppresses it, it can in nowise contribute to renew the secretion of the liver which is required, (and which can alone remove the congestion,) whilst it reduces, still further, that vital energy by which the patient is to be supported through the struggle.

That bleeding in the collapse stage is inadmissible, is more than implied by the practice of many who, under precisely the same circumstances, resort to the use of stimulants. And of these it may be observed, that, like emetics in the incipient stage of a mild case of the disease, they may prove occasionally useful, by the stimulus they give to the arterial circle of vessels involved in the congestion, and thus force forward the circulation.

But the power which such forced circulation has to restore the secretion of the liver, is unappreciably slight. When given at the period when the collapsed stage is formed, and the pulse has become extinguished, they will, if freely used, rouse the heart to a more forcible action, and render the pulse perceptible; but, like the other means, they do not thereby remove the proximate cause, and the pulse almost instantly becomes extinguished again. If given in the slighter cases of the complaint, and the patient survives the collapse, and the secretion of the bile is at the same time unrenewed, the consecutive fever will ensue, with its concomitant inflammation in the mucous tissues of the stomach, and they will be then found, by their previous use, to aggravate these states, and materially to increase the danger. In that premonitory stage of the complaint, when the diarrhœa is the prominent affection, the use of opium in small doses is not only admissible, but highly beneficial, by weakening the energies of those morbid sympathies which have their seat and origin in the stomach and intestinal canal, and which appear to lead, as their ultimate results, to that obstruction in the secretion of the liver, and its consequent congestion, which form the essence of the complaint. If given, however, in large doses, whether before or after the congestion is fully formed, they will perhaps be found to do harm by temporarily extinguishing that excitability of the nervous system upon which every remedy is to act, and by which alone indeed the functions of life are carried on.

Such then being the views which I have taken of the principles upon which the foregoing agents act upon cholera, and of the pre-eminent importance of calomel given in small and frequently-renewed doses, as the great instrument for its cure, I decided in the cases which I should have to treat, to limit myself exclusively to this medicine, and to such minute doses of opium united with it as might be necessary to delay its passage through the stomach, and support the energies of the system.

In my last letter I reported two cases of successful treatment in which this plan was strictly adhered to, and one[*] also in which recovery took place under the use of calomel, but given at wider intervals, and not unmixed with other means. These were the only cases that had been treated by small doses of calomel, by myself or others here, up to the period when I wrote, and since that time only two other cases, as far as I can learn, have been treated by the calomel given at the short intervals employed by me, or unmixed with other means. These two cases were both of the severest kind, but being treated according to the mode already adverted to, were cured with a facility which greatly surprised the gentleman who saw the cases with me, and who indeed, and more especially in the second one, had a leading share in the treatment. The following are the two cases as drawn up by Mr. Sharpe :

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[*] In your Journal of last week, a correspondent of the name of Clay has denied the correctness of the account given by me of this case in my last letter, and disclaims my right to quote it. To the first charge, I may reply, that I received the particulars from Dr. Alderson himself, and this, with the small—the *insignificant* difference between my account and his corrected one, is my answer in full. To the second charge, which he grounds upon the assumption that it was a private case, I reply that it was *not a private, but a public one*. Your correspondent is the hired and paid servant of the Board of Health, of which I am a member, and he is under a positive engagement whilst he is so, not to engage in any *private* practice. By his own admission he visited this patient alone, and bled him on his own responsibility, and without consulting any one. I regarded him therefore as the attendant, and his

Case 1.

At half past nine o'clock p.m. on Thursday the 10th of May, I was called to visit Mary Lotherington, a married woman, aged 22, of intemperate habits, residing in Trundle Street. Her former residence was in Green Lane, Sculcoate, in which house three cases of cholera had occurred. She left there on the 7th of May. I found her labouring under the following symptoms:—Pulse imperceptible; extremities cold, and of a livid hue; skin dry; eye sunk; countenance contracted; tongue moist, but colder than natural; thirst excessive; vomiting and purging of a fluid which had the appearance of rice-water; severe pain in the epigastrium; cramps in the upper and lower extremities; urine suppressed. I ascertained that she had been slightly affected with diarrhœa from two a.m., and at eleven a.m. was attacked with severe vomiting, purging, and cramps of the extremities, which continued until I saw her. I immediately gave her two grains of calomel with four drops of the tinct. of opium, to be taken every ten minutes; heated bricks were applied to the feet and sides of the chest.

Half past eleven p.m. I saw her again in conjunction with Dr. Ayre: there was not any alteration of the symptoms. The pills were now given every five minutes during an hour, and afterwards every ten minutes, and a mustard cataplasm was applied to the epigastrium. We remained with her near two hours, during which time the pulse became slightly perceptible, beating 120 in a minute; the cramps less severe; vomiting and purging rather abated. My assistant remained with her during the whole night, and administered the mixture. On leaving her, the pills were ordered to be continued every ten minutes.

Friday, eight a.m. Thirty doses of the calomel and opium had been taken; the surface is warmer; the eye more prominent; countenance more animated; purging and sickness nearly abated, but the evacuations are of the same appearance; has not yet passed any urine; the calomel and opium to be continued every fifteen minutes.

Half past twelve p.m. The symptoms somewhat relieved, excepting the sickness, which has rather increased; has not yet passed any urine. The calomel to be continued without the opium every half hour, with an effervescing draught, and a teaspoonful of brandy with each dose.

Two p.m. Sickness rather abated, but the cramps are more severe; urine still suppressed; pulse 100, and of greater strength. The calomel and opium pills to be taken again every half hour with the effervescing draught.

Five p.m. Cramps abated; sickness not so severe; has not yet passed any urine; pulse 95, and stronger. Calomel pills to be given every half hour without the opium.

Nine p.m. Continues improving, but has not yet passed any urine; has passed some stools slightly tinged with bile. Continue the calomel pills every half hour during the night.

Saturday, seven a.m. The symptoms are much relieved; has passed a little urine for the first time during the last 44 hours. Dr. Ayre had seen her previous to me, and ordered a powder of rhubarb and magnesia, and an effervescing draught to be given occasionally, and the calomel pills to be given every two hours. From this time the patient continued to improve rapidly; the secretions became natural, and yesterday and to-day she has had several dark bilious motions; the pulse is only 76; the tongue is clean and moist; there is no soreness of the mouth, and, excepting for some nausea which still continues, she may be pronounced as convalescent.

R. Sharpe.
Hull, 14th May, 14, Castle Street.

Case 2.

Jane Hodgson, aged 50, a married woman of temperate habits, residing in Caroline Street, Spencer Street, was first seen by me at half past two p.m. on Friday, May 18. The husband informed me that she had been labouring under diarrhœa since Monday, but that at five a.m., May 18, she was attacked with severe vomiting, purging, and cramps. I found her affected with the

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friend as the superintendent of the case, and as the servant of the Board of Health, and acting for it, I referred to the case, being myself a member of that Board. It was not for me to know, nor did it concern me to inquire, whether he or his friend had the larger share in the honour of the case, or which was to the other, being both young men, the premier in the management of it.—J. A.

following symptoms: pulse 125, almost imperceptible; surface dry and cold; hands shrivelled; the lips, chin, hands, and feet, blue; eyes sunk and surrounded with a dark areola; countenance expressive of great anxiety; cramps of the lower extremities; voice feeble; thirst excessive; tongue slightly furred and warm; sickness and purging of a fluid resembling rice-water; urine suppressed. Two grains of calomel, with three drops of tincture of opium, to be taken every five minutes for one hour, and then every ten minutes; hot bricks to be applied to the feet.

Five p.m. The symptoms still continue, but are less severe. One drop and a half of the tincture of opium with the calomel, to be taken every ten minutes for one hour; and afterwards every fifteen minutes.

Eight p.m. Was joined by Dr. Ayre; pulse 100, and stronger; surface warm; blueness disappeared; countenance regained its natural appearance; cramps less severe; purging entirely abated; sickness continues; has not yet passed any urine. Repeat the calomel, with one drop of the tincture every half hour.

Saturday, half past twelve a.m. Sickness nearly abated; thirst not so troublesome; has passed no urine. The pills to be taken every hour.

Seven a.m. Pulse 90 and strong; sickness entirely ceased; no urine yet; has had some sleep during the night. The pills to be continued and taken every two hours.

Two p.m. Has had two liquid stools void of bile; urine still suppressed. Continue the pills as before.

Half past nine p.m. Improved in all her symptoms. Has passed a little urine, after 36 hours suppression. A powder of jalap and rhubarb to be taken immediately, and repeated in three hours.

Sunday, ten a.m. Appears much improved; has just passed another stool, but not altered in appearance. Urine passes freely. The powders of rhubarb and jalap to be repeated.

Nine p.m. Has been rather affected with sickness; in other respects much better; has passed some bilious stools; the mouth slightly affected. An effervescing draught to be taken occasionally. She may be considered as quite convalescent.

R. Sharpe.
Monday, May 21st.

In the cases above given, and in those to which I have referred, as at once illustrating my views of the nature of the disease, and of the treatment demanded for it, I desire not to push my conclusions beyond what the facts will legitimately warrant. The four cases are the only ones in which I knew the practice has been pursued in strict accordance with my views, and without any admixture of other means. They were all severe cases, and they all recovered, and without being followed by any consecutive fever, which happens in so many cases of recovery, but which did not happen here, and could not happen if my views be correct concerning the mode by which the calomel acts upon the disease. The treatment and results of every case I may hereafter have shall be reported, and I hope, in the mean while, that others may be induced to make the same trials, and duly report the issue of them.

Joseph Ayre.

Hull, May 23d, 1832.

MALIGNANT CHOLERA.

DOCUMENTS

COMMUNICATED BY THE

CENTRAL BOARD OF HEALTH, LONDON,

RELATIVE TO THE TREATMENT OF CHOLERA BY THE COPIOUS INJECTION OF AQUEOUS AND SALINE FLUIDS INTO THE VEINS.

No. 1.

Letter from Dr. Latta* *to the Secretary of the Central Board of Health, London, affording a View of the Rationale and Results of his Practice in the Treatment of Cholera by Aqueous and Saline Injections.*

Leith, May 23, 1832.

Sir,—My friend Dr. Lewins has communicated to me your wish for a detailed account of my method of treating cholera by saline injection into the veins, with which I now most willingly comply. My scope for observation, since I commenced this treatment, has been too limited to allow me to be very copious on the subject, but I think I can adduce sufficient proof to the unprejudiced, not only of its safety, but of its unquestionable utility. I have never yet seen one bad symptom attributable to it, and I have no doubt that it will be found, when judiciously applied, to be one of the most powerful, and one of the safest remedies yet used in the second stage of cholera, or that hopeless state of collapse to which the system is reduced.

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* Dr. Latta having signified his wish that this communication should be published in The Lancet, the Central Board of Health have accordingly forwarded it to this Journal.—Ed. L.

Before entering into particulars, I beg leave to premise that the plan which I have put in practice was suggested to me on reading in The Lancet, the review of Dr. O'Shaughnessy's report on the chemical pathology of malignant cholera, by which it appears that in that disease there is a very great deficiency both of the water and saline matter of the blood. On which deficiency, the thick, black, cold state of the vital fluid depends, which evidently produces most of the distressing symptoms of that very fearful complaint, and is, doubtless, often the cause of death. In this opinion I am abundantly borne out by the phenomena produced on repletion by venous injection.

So soon as I learnt the result of Dr. O'Shaughnessy's analysis, I attempted to restore the blood to its natural state, by injecting copiously into the larger intestines warm water, holding in solution the requisite salts, and also administered quantities from time to time by the mouth, trusting that the power of absorption might not be altogether lost, but by these means I produced, in no case, any permanent benefit, but, on the contrary, I thought the tormina, vomiting, and purging, were much aggravated thereby, to the further reduction of the little remaining strength of the patient; finding thus, that such, in common with all the ordinary means in use, was either useless or hurtful, I at length resolved to throw the fluid immediately into the circulation. In this, having no precedent to direct me, I proceeded with much caution. The first subject of experiment was an aged female, on whom all the usual remedies had been fully tried, without producing one good symptom; the disease, uninterrupted, holding steadily on its course. She had apparently reached the last moments of her earthly existence, and now nothing could injure her—indeed, so entirely was she reduced, that I feared I should be unable to get my apparatus ready ere she expired. Having inserted a tube into the basilic vein, cautiously — anxiously, I watched the effects; ounce after ounce was injected, but no visible change was produced. Still persevering, I thought she began to breathe less laboriously, soon the sharpened features, and sunken eye, and fallen jaw, pale and cold, bearing the manifest impress of death's signet, began to glow with returning animation; the pulse, which had long ceased, returned to the wrist; at first small and quick, by degrees it became more and more distinct, fuller, slower, and firmer, and in the short space of half an hour, when six pints had been injected, she expressed in a firm voice that she was free from all uneasiness, actually became jocular, and fancied all she needed was a little sleep; her extremities were warm, and every feature bore the aspect of comfort and health. This being my first case, I fancied my patient secure, and from my great need of a little repose, left her in charge of the hospital surgeon; but I had not been long gone, ere the vomiting and purging recurring, soon reduced her to her former state of debility. I was not apprised of the event, and she sunk in five and a half hours after I left her. As she had previously been of a sound constitution, I have no doubt the case would have issued in complete reaction, had the remedy, which already had produced such effect, been repeated.

Not having by me the Number of The Lancet containing Dr. O'Shaughnessy's analyses, I adopted that of Dr. Marcet, only allowing a smaller proportion of saline ingredients. This I now find to be considerably less than natural, according to the more recent analyses. I dissolved from two to three drachms of muriate of soda and two scruples of the subcarbonate of soda in six pints of water, and injected it at temperature 112° Fah. If the temperature is so low as a hundred, it produces an extreme sense of cold, with rigors; and if it reaches 115°, it suddenly excites the heart, the countenance becomes flushed, and the patient complains of great weakness. At first there is but little felt by the patient, and symptoms continue unaltered, until the blood, mingled with the injected liquid, becomes warm and fluid; the improvement in the pulse and countenance is almost simultaneous, the cadaverous expression gradually gives place to appearances of returning animation, the horrid oppression at the præcordia goes off, the sunken turned-up eye, half covered by the palpebræ, becomes gradually fuller, till it sparkles with the brilliancy of health, the livid hue disappears, the warmth of the body returns, and it regains its natural colour,—words are no more uttered in whispers, the voice first acquires its true cholera tone, and ultimately its wonted energy, and the poor patient, who but a few minutes before was oppressed with sickness, vomiting, and burning thirst, is suddenly relieved from every distressing symptom; blood now drawn exhibits on exposure to air its natural florid hue.

Such symptoms, so gratifying both to the sick and the physician, must never allow the latter to relax in his care—the utmost vigilance is still necessary. At first the change is so great that he may fancy all is accomplished, and leave his post for a while. The diarrhœa recurring, he may find his patient, after the lapse of two or three hours, as low as ever. As soon as reaction by the first injection is produced, mild warm stimulants, such as weak gin-toddy, mixed with some astringent, should be freely and assiduously administered. An attempt should be made to fill the colon with some astringent fluid. That such is requisite, is evident from the watery diarrhœa returning with violence, and if not restrained, death will ultimately make sure of his victim, therefore so soon as the pulse fails, and the features again shrink, the venous injection must be repeated, taking care that the fluid in use retains its proper temperature. The injection should be carried on very slowly, unless the patient is much exhausted, when it may be used more rapidly at first, until a little excitement is produced, after which it should not exceed two or three ounces per minute, and now is the time for the exhibition of astringents by the mouth, which will be retained, for in general the sickness entirely leaves during the operation.

Such remedies must be persisted in, and repeated as symptoms demand, or until reaction is permanently established. I have witnessed no violent symptoms accompanying the rapid injection of the fluid, but I have thought that the hasty repletion of the system was followed by great increase of the evacuations, and consequently a more sudden depression of the powers of life. The quantity to be injected depends on the effect produced, and the repetition on the demands of the system, which generally vary according to the violence of the diarrhœa; the greater the degree of collapse, the greater will be the quantity needed, though not uniformly, for a very slight loss produces much depression in some systems; hence there is often great collapse, without much vomiting, purging, or cutaneous discharge.

Although in every case, even the most desperate, the cholera symptoms were removed, some of my cases failed, which I attributed to one or other of the following causes; either the quantity injected was too small, or its effects were rendered abortive by extensive organic disease, or its application was too late.

I have already given an instance where deficiency in quantity was the cause of failure, which I will now contrast with one in which it was used freely. A female, aged 50, very destitute, but previously in good health, was on the 13th instant, at four a. m., seized with cholera in its most violent form, and by half-past nine was reduced to a most hopeless state. The pulse was quite gone, even in the axilla, and strength so much exhausted, that I had resolved not to try the effects of the injection, conceiving the poor woman's case to be hopeless, and that the failure of the experiment might afford the prejudiced and the illiberal an opportunity to stigmatise the practice; however, I at length thought I would give her a chance, and in the presence of Drs. Lewins and Craigie, and Messrs. Sibson and Paterson, I injected one hundred and twenty ounces, when like the effects of magic, instead of the pallid aspect of one whom death had sealed as his own, the vital tide was restored, and life and vivacity returned; but diarrhœa recurred, and in three hours she again sunk. One hundred and twenty ounces more were injected with the same good effect. In this case 330 ounces were so used in twelve hours, when reaction was completely re-established; and in forty-eight hours she smoked her pipe free from distemper. She was then, for better accommodation, carried to the hospital, where probably, from contagion, slight typhoid symptoms were produced. She is now, however, convalescent.

The second cause of want of success is the presence of organic disease; this, probably, renders the possessor very liable to attacks of cholera; and the latent evil, which previously gave but little uneasiness, suffers aggravation in all its symptoms, more especially after reaction has been produced, and has evidently, in many cases, been the cause of death. A delicate young female, of strumous habit, who had been for some years subject to pectoral complaints, was rescued from a state of collapse by the injection of sixty ounces of the saline fluid, administered in separate portions, within the space of twelve hours. After lingering for ten days she died; the heart was found in a state of atrophy, covered with strong evidence of the existence of ancient disease, and floating in eight ounces of pus. In another case every internal organ was diseased; some of them so much so, that it was astonishing the individual lived so long.

The third cause of the occasional want of success, is the late application of the remedy. Hitherto I have had opportunity of injecting only in extreme cases, after every other means had entirely failed, cases which apparently would have soon proved fatal. Here the obstacles to be overcome have been of no ordinary kind, notwithstanding the result of the practice is of the most encouraging nature, and the number of cases now convalescent or doing well highly gratifying. In every fatal case we have had an opportunity of examining, independent of organic disease, I have found a large quantity of fibrin in the cavities of the heart, especially on the right side, where it had extended from the auricle through the ventricle into the pulmonary artery. Such deposition must have formed a certain obstacle to recovery, and is, no doubt, from the interruption it gives to the pulmonary circulation, the cause of the heavings of the chest, and the inordinate action perceptible in the centre of circulation many hours before death. Now surely it is reasonable to suppose, that if this, the most simple of all remedies, were applied early, before the blood drained of its water has collected in the larger vessels, in fact before such fibrinous depositions have taken place in the cavities of the heart, is it not reasonable to suppose that such would be entirely prevented?

But not only is early injection advisable on this account, not only is stagnation of the blood prevented by it, and the laborious breathing, and the præcordial oppression, the intense sickness, the burning thirst, the extreme depression of the vital powers, and the chances of aggravating chronic disease, or of producing new organic lesion, in a great measure avoided; but it is rational to suppose that the consecutive fever will be rendered much milder, and that this is the case, is supported by my own experience, even though the remedy has not been applied earlier, indeed the fact is very evident. In an ordinary attack of cholera, much fluid is lost; and if the individual is so fortunate as to get out of the stage of collapse, if consecutive fever of typhoid type comes on, the system, left to its own resources to replace the lost serum, must be but ill fitted for the task, for the debility is extreme, absorption goes on slowly, the fever will be much aggravated by the irritation of internal congestion; local inflammation will thereby be produced, and the chance of recovery will be but small. Much of this evil is to be mitigated or entirely avoided by injection into the veins, of which circumstance I can adduce living instances; and where the patient, who had been injected, has sunk under organic disease, the usual marks of congestion are not perceptible.

The apparatus I have used, is Read's patent syringe, having a small silver tube attached to the extremity of the flexible injecting tube. The syringe must be quite perfect, so as to avoid the risk of injecting air; the saline fluid should never be injected oftener than *once* into the same orifice, and the vein should be treated with much delicacy to avoid phlebitis. The wound should be poulticed and carefully watched, if it does not heal by the first intention.

I am, Sir,
Your most obedient servant,
Thomas Latta, M.D.

No. 2.

Letter from Dr. Lewins *to the Secretary of the Central Board of Health.*

Results of the Injection Practice in the Drummond-street Cholera Hospital, Edinburgh.

Sir,—You will receive from Dr. Latta the details of two or three cases treated by saline injections. We have both been so much occupied to-day, that we have not had leisure to get our communications ready to be sent in the same envelope. We steal an hour from the time usually allotted for rest to write to you. In case Dr. Latta should omit to mention the circumstance, I beg to mention that his patient, Cousins, the woman who was injected to the amount of 376 ounces, and who promised to do well, for a considerable time, was a person of very dissipated habits.

In the Drummond-street Hospital six patients have been injected, and three recovered, or are recovering. In the three that died, extensive organic disease was found on dissection; disease that had existed previously to the attack of cholera.

I send herewith the report of two cases, treated by Dr. Craigie of this place, which, at my request, he furnished me to-day for the perusal of the Board.

I intended to have sent an account of an interesting fatal case, the only one in which the venous injection may be said to have fairly failed where it was fairly used; that I shall do to-morrow. I have the honour to be, Sir, your most obedient servant,

Robert Lewins, M.D.
Leith, May 27th.

No. 3.

Details of Two Cases of Malignant Cholera treated by Venous injection, by Dr. Craigie, *of Leith.*

No. 1. *Case successful.* 15 *lbs. injected at intervals in nine hours.*—Martha Smith, aged 38, a noted drunkard, thin and debilitated, in sixth month of pregnancy, admitted into the hospital at 8 p. m., May 16th, 1832.

It appears she had had vomiting and purging since Sunday morning, 12th inst. Cramps came on about four hours ago in both legs; great evacuations both upwards and downwards like dirty water. The countenance is now collapsed; eyes sunk; tongue cold; pulse imperceptible at wrists; very small in brachial artery; 124.

℞ *Muriat. sodæ* ʒiij;
Carbon. sodæ ʒi;
Aq. calid. ℔vj. *solve. Ft. Enema statim injiciend.*

Sinapisms to spine and epigastrium; let her be placed on heated tin mattress.

Nine a.m. Has a good deal of vomiting; is getting warmer; pulse now perceptible in right wrist; tongue warmer; she allows the enema to come away without giving notice to nurse. Saline enema as above, with the addition of white of eggs, to be repeated every half hour.

Ten. Vomiting and purging of watery fluid, with slimy matter in it.

Half-past ten. Cramps have returned severe in left leg; pulse again imperceptible; urgent thirst and constant vomiting. *Rep. enema et pulveres effervescentes.*

Half-past-eleven. Breathing becoming much affected; extreme restlessness; cramps severe in legs, and every symptom of sinking. Let the following saline solution be injected into one of the veins of the arm.

℞ *Muriat. sodæ* ʒi;
Carbon. sodæ gr. x;
Aq. calid. ℔iij, *solve temp.* 105° *Fahr.*

Noon. When about ℔i had been thrown in, the pulse was perceived to flutter at the wrist, and gradually strengthened as the injection was proceeded with. By the time ℔iiiss had been injected, the countenance, which was before quite death-like, now beamed with the appearance of health, and she began to converse freely. Pulse 96, moderate. To have ℥i gin in warm water with sugar.

Half-past one. The gin was immediately rejected. Pulse has again gradually become imperceptible, and respiration quick and laborious.

Two. Let the venous injection be repeated to ℔vij.

The effect of the injection, as formerly, was very striking. To see an individual who seemed *in articulo mortis* brought back, as it were, in so short a space of time to an apparently tolerable state of health could not but astonish the beholder. Before the injection was finished the pulse had returned to a healthy fulness and firmness. Expresses herself much relieved; no purging, but vomits much serous matter.

℞ *Muriat. sodæ* ʒij;
Carbon. sodæ ʒi;
Alcohol. dilut. ℥i;
Aq. calid. ℔iij. *M. ft. enema statim injiciend.*

Four. Enema retained about an hour and a half; surface of body now comfortably warm; she has not passed more fluid by stool than was thrown into the rectum.

Six. Has slept softly for an hour; the first sleep she has had for many days.

℞ *Subm. hydrarg.* gr. v;
Pulv. opii gr. ss. *M. sumat stat. et rep. 3tia quaque hora.*

Nine. Complains much of vomiting and sense of weakness; countenance rather collapsed; breathing difficult.

Let ℥lxxx *be injected into the veins again gradually.*

After the first few ounces were thrown in she complained of an acute pain at the epigastrium, and faintness, probably arising from the fluid being thrown in too fast upon the heart, or from the passing of a bubble or two of air, which may have got in from the inaccuracy of the injecting apparatus used. Be that as it may, the circulating system was so much affected, that the pulse, from being distinct though feeble, became quite imperceptible, but on stopping the injection for a few minutes the pulse gradually returned, and the pain abated. She expressed herself as always getting relief from the operation.

Eleven. Vomiting continues urgent.

App. emp. cantharid. epigast.
Effervescing draughts occasionally.

May 17th. Has passed about ℔j of urine, of natural appearance; this is the first she has made since she was brought in.

From this time she went on gradually to improve, but stomach continued very irritable, and the matter vomited was bilious.

21st. Labour pains came on, and she was delivered of a still-born female child.

22d. Symptoms of phlebitis in right arm came on, proceeding from the wound upwards, but this yielded to the ordinary treatment, and she may now be considered out of all danger, though she is not yet reported cured.

THOMAS CRAIGIE, M.D.
Leith, May 26th, 1832.

No. 2. *Case fatal—appearances on dissection.* — George Cousins, aged 10, was brought into quarantine at nine a.m., 13th May, on account of his mother being ill of cholera. About an hour after admission began to vomit and purge, and it appears he has had diarrhœa severely all the morning. Pulse 102, extremely weak; complains much of sickness; countenance collapsed; areolæ rather dark under the eyes; voice very weak.

He had hot air-bath immediately, and got the following dose:—

℞ *Ol. ricini* ℥ss;
Tinct. opii gtt. xv;
Aq. menth. pip. ℥iss *M. ft. haust.*

Half-past Eleven a. m. Draught retained; sickness has gone off; complains of heat of bath; let it be removed.

Noon. Has vomited some watery matter, with undigested potatoes in it, and again a rice-watery fluid with flocculi. He has now a considerable degree of jactitation ; countenance more sunk, and great desire for cold water. These symptoms went on increasing in severity in spite of sinapisms to spine, effervescing draughts, calomel, and Dover's powder, warm water, enemata, &c., and head symptoms were now making their appearance.

Half-past Two p. m. Pulse quite imperceptible, and has been so for an hour and a half; he lies quiet and drowsy, with eyes turned upwards; face bedewed with cold perspiration; hands and feet cold and very blue.

My colleagues, Drs. Combe and Lewins, saw him with me at this time, and concurred with me in thinking him not only beyond all hope of recovery, but likely to die *within an hour or two.*

From what I had seen of the resuscitating powers of Dr. Latta's treatment on the boy's mother this morning, by venous injection, I determined on giving it a trial, though this was a case rather likely to bring discredit on the remedy than otherwise. The following solution at temperature 102° F. was slowly injected into the median basilic by means of a common silver blowpipe attached to Read's enema syringe.

℞ *Muriat. sodæ* ʒi ;
Carbon. sodæ gr. x ;
Aq. calid. ℔vj, *solve.* T. 102.

Three p. m. A few minutes after the injecting was commenced the pulse returned to the wrists, the blueness and coldness of the extremities gradually wore off ; the countenance was much improved ; and the whole fluid was injected within twenty minutes.

Half-past Three p. m. He has now a healthy blooming appearance ; is sitting up in bed, and looking about him as if awoke out of a dream. Pulse 110, natural ; extremities of good colour and warm ; voice much stronger.

Half-past Four p. m. Pulse has been gradually falling off since last report ; is getting listless, and dislikes to be troubled with questions ; breathing becoming laborious, and head symptoms more marked, with squinting to a slight degree superadded.

Seven p. m. Pulse again imperceptible ; respiration quick and laboured ; countenance collapsed ; tongue and breath cold ; says he is dying.

Let the venous injection be repeated to lbiij.

Half-past Seven p. m. Pulse immediately returned, of natural strength and fulness, and continues so.

Nine p. m. Lies very quiet ; pulse good ; breathing more natural ; surface of body covered with warm perspiration.

Ten p. m. Large watery evacuations from the bowels came on soon after last injection ; the quantity cannot easily be guessed, but must have been considerable, as it is running through the mattress on the floor. Pulse scarcely perceptible ; screams loudly like a child in hydrocephalus.

Eleven p. m. Pulse quite imperceptible ; is sinking fast ; venous injection attempted a third time, but desisted from as it was not productive of the first good effects. Both pupils much dilated. Died at two a.m. 14th.

Dissection fifteen hours after Death.—On exposing the brain and spinal marrow, but before opening their investing membranes, the least pressure with the fingers on the middle of the hemispheres of the brain caused a remarkable undulating down to the middle of the back, showing the existence of a fluid beneath the membranes, and on opening them, about two drachms of pure serum flowed out.

The surface of the brain was rather vascular, and the blood in the most minute vessels particularly bright. A few ecchymosed spots on its surface. All the other viscera were found healthy. The urinary bladder contained about half an ounce of urine.

Leith, 26th May, 1832.

Thos. Craigie, M.D.

No. 4.

Note from Dr. Macintosh *to* Dr. Latta.

My dear Sir,—You will be delighted to see the enclosed. Our old patient up to this hour is doing well. Send this to Dr. Lewins.

Yours, &c.
J. Macintosh.

Dr. Latta.

Note from Mr. Racy *to* Dr. Macintosh.

Drummond-street, 11 o'clock.

Dear Sir,—Another case has just come in, and we have transfused 6lbs of saline mixture in the presence of Dr. Alison at the patient's house with better success than in any of our other cases. The doctor is quite delighted. Yours, &c.

John Racy.

No. 5.

List of Queries addressed to Drs. Lewins *and* Latta, *by the Central Board of Health, London, relative to the preceding cases, &c.*

Queries by the Central Board.

1. Were any of your patients bled previously to, or after, the saline injections into their veins ?

2. Were the evacuations by purging, vomiting, or perspiration, increased by the injections?

3. Did any of the patients submitted to the saline injection plan die; and if examined after death, what were the appearances?

4. Had the pulse at the wrist absolutely ceased, and for how long, or had blueness of the surface taken place, and to what extent, in any of your patients before the injection of the saline fluids; and how many of such patients recovered under that treatment?

5. Had suppression of urine been *perfectly* established, and for how long, in any of your cases previously to the saline injection, and what effect did that practice appear to produce on the urinary secretion?

6. What effect did the injections appear to have on the temperature of the patient?

7. Were the blood and evacuations analysed before and after the injections?

8. Did consecutive fever occur in any, and if so in how many of your cases, whether successful or otherwise?

9. Was the quantity of the evacuations noted before and after the injections in any of your cases?

10. Please to give the details of two or three cases treated by saline injections with age, condition of life, temperament, habits, &c., and particulars of such other treatment as may have been adopted in addition to the saline injections.

Answers by Dr. Lewins.

1. None before. One to the amount of twelve ounces immediately after the first injection.

2. The evacuations by purging and vomiting, in most of the cases continued. In some of them the purging, the discharge from the bowels at least, was increased. Perspiration was increased in all.

3. Yes; no less than ten of the fifteen that have been injected up to the present day; but under such circumstances as do not detract from the general merits of the practice: this will be made evident by the history of the cases that will be sent by to-morrow's post.

4. Yes; even at the axilla in some of the cases, blueness of the surface had taken place to a considerable extent. Five of these patients recovered.

5. Complete suppression, I think, in all except two, and for hours. In all the successful, and in some of the unsuccessful cases, the effects of the injection in restoring the secretion of urine were most evident.

6. The injections raised the temperature of the body; but in all the successful cases where the veins were injected the patients complained of cold soon after the injection.

7. Neither the blood nor the evacuations were analysed, but I sent some of the blood of a patient that had been injected by the veins to Dr. Reed for analysis to-day.

8. The consecutive fever in all the patients who were injected has been slight.

9. No; but they were excessive in most of the cases.

10. Question ten shall be fully answered by to-morrow's post.

ROBERT LEWINS, M.D.

6, Quality-street, May 26, 2 o'clock a.m.

No. 6.

Latest Communication from Dr. LEWINS *to the Secretary of the Central Board of Health.*

Sir,—The urgency of my present private and public duties prevents me from communicating more to you to-night than the following brief particulars of a case that occurred at the Leith Cholera Hospital yesterday:—

A woman of about forty years of age was admitted on Sunday evening at seven o'clock. She was *pulseless* even at the axilla, *sightless*, *cold* and *blue* over almost the whole body. *Respiration* very slow and irregular—in a word, she was all but lifeless. It was feared she would be dead before the operation of injecting could be commenced. Between seven at night and two o'clock next morning, there were thrown in 284 ounces, upwards of twenty-three pounds. The report of her situation at two o'clock on Monday morning, in the hospital book, is as follows:—"A change for the better that appears almost miraculous has taken place. The action of the heart is greatly improved; respiration not in the least laborious, but quicker than natural; pulse 120, small, but distinct. She can articulate distinctly; countenance natural; lips red; tongue moist and warm; she perspires freely; heat over the whole body natural."

A full report of this wonderful case shall be forwarded soon. I remain, Sir, &c.,

ROB. LEWINS.

P.S.—In one the pulse had ceased at the wrist eight hours before the injection. Dr. Alison had seen the patient eight hours before the operation, and the pulse was then imperceptible.

Quality Street, Leith, May 29, 1832,
1 o'clock a.m.

NOTE FROM DR. O'SHAUGHNESSY.

Proposal to combine Stimulants and Astringents with the Aqueous and Saline Injections.

To the Editor of THE LANCET.

SIR,—Having learned from the Central Board of Health the circumstances of Dr. Latta's peculiar practice, I may be perhaps excused for requesting the immediate insertion in your Journal of the following brief observations:—

Although I expected great benefit from the injection of water into the veins, and although I believed that the addition of small quantities of salts to that water would contribute powerfully to the restoration of the blood to its natural state, I beg to state that the results of the practice described by Drs. Latta and Lewins exceed my most sanguine anticipations. When we consider that no practitioner would dare to try so novel an experiment, except in cases beyond hope of relief by any ordinary mode of treatment, and consequently desperate to the last degree, even a solitary instance of recovery affords matter for congratulation.

I write now for the purpose of offering the following suggestions to the gentlemen engaged in these experiments.

Although by the injection of water and salts (in quantities varying according to the previous extent of the evacuations), we may restore the deficient fluids of the body, and bring back the blood to its normal state, thus possibly removing a powerful cause of death in this disease, we must still remember that the unknown *remote* cause, and other agents, may be and still are in operation, and require to be remedied before a perfect cure can be performed. The great debility, the natural effect of the evacuations, will probably require some stronger stimulant than the new and artificial blood will afford. Some means will also have to be taken to prevent the recurrence or continuance of those evacuations, an event of constant occurrence in Dr. Latta's cases.

I would therefore recommend, that in addition to the injection of water, either simultaneously or subsequently, as circumstances would suggest to the operator—*medicinal agents*—of the stimulant or *astringent* classes, for example, *should also be injected in minute doses.*

In selecting the agents employed, it is to be cautiously observed that they must not be chemically incompatible with the blood. They must not coagulate albumen, fibrin, or colouring matter, or darken the colour of the latter. As a general rule they should be soluble in water.

As *stimulants*, minute quantities of *carbonate of ammonia* dissolved in water might be used. Dupuy, of Alfort, has found that this salt may be injected with safety into the veins of horses. Small doses of the *sulphate of quinine* might also be thus administered. Even extremely dilute spirits might be employed.

Weak decoctions, or solutions of the vegetable *astringents*, might also, I conceive, be injected with great advantage. Gaspard's experiments fully warrant me in stating that these cannot induce any *bad* effect, and that they may therefore be tried with perfect safety.

I am, Sir, your obedient servant,
W. B. O'SHAUGHNESSY, M.D.
London, June 2, 1832.

ROYAL COLLEGE OF PHYSICIANS.

May 28th, 1832.

TREATMENT OF CHOLERA AT CONSTANTINOPLE.

SIR HENRY HALFORD, M.D., in the chair. This evening the assembly in the library was less numerous than usual, and with the exception of a few church dignitaries, there were none of the usual aristocrat visitants. The President announced the first paper to be a letter on the successful treatment of cholera, from Dr. Samuel M'Duffal, Physician to the Embassy at Constantinople, to Sir Robert Gordon, late ambassador at that court.

The writer considered the great indication in the treatment of this formidable disease was to restore the lost balance of the circulation; and this was to be done, in their way, by the exhibition of internal stimuli, by free venesection, and by the use of such external applications, as mustard cataplasms, hot-bricks, and various rubefacients. This mode of treatment he had found successful, in the great majority of instances. When a patient was brought to him with the hollow eye, cold surface, leaden hue, and feeble pulse, characteristic of this malady, he immediately administered brandy, in large quantities, qualified with opium. He then would bleed the patient to sixteen or twenty ounces. As the blood flowed, the surface grew gradually warm, the pulse rose, and the eye and countenance regained their tranquillity. The patient after this was put into a warm bed, and treated with rubefacients, and recovered in a few days. In the village of Terapia, in the vicinity of Constantinople, out of 204 patients attacked with cholera, and treated in this manner, 200 recovered; and the proportion in other places was nearly the same. A native merchant of great wealth, affected with the incipient approaches of cholera, would not suffer his barber to bleed him, being buoyed up by the hope that the disease would not run its course. Dr. M'Duffal was sent for, and found him with leaden extremities, cold skin and breath, imperceptible pulse, and the peculiar facial expression of this epidemic. He was bled immediately to sixteen ounces, and the symptoms gradually subsided; stimulants internal as well as external were freely resorted to. About an hour after this a tendency to relapse was observed, when sixteen ounces more blood were abstracted, and the other treatment continued. He ultimately recovered.

An Armenian nun, who had long been subject to frequent attacks of gastro-enteritis, was attacked with cholera to a severe degree; stimulants were freely given, and copious venesection resorted to. The collapse was subdued, but it was followed by a sharp gastro-enteritic affection, which was overcome by leeches, mucilaginous potions, and enemata. Dr. M'Duffal opines, that there is no hope if more than four hours have elapsed before the administration of remedies; and he recommends that the public in general should be made acquainted with the importance of immediate measures being had recourse to. The writer used himself to counsel the people within his sphere of observation, to give freely to the affected person any spirits he might have in their dwelling; to place him in a warm situation, and immediately to send to a barber to bleed him. When these prompt measures were resorted to, the patient, on the arrival of the physician, was in a much more favourable condition for treatment; for the stage of collapse being by those means subdued, the stage of reaction alone remained to be grappled with. When marks of reaction were observed, it became expedient to intermit the stimulants, for in all cases a great tendency to inflammatory affections of the head, chest, and abdomen, existed during the stage of reaction. These affections were managed much in the same manner as in England, by leeches, antiphlogistics alteratives, such as calomel, opium, and others, and epispastics.

Dr. M'Duffal did not dwell much on the rise and causes of the disease; but he was of opinion, that mental depression was a great predisponent. The inhabitants of the Pera suffered most severely from this malady, which he ascribes to chagrin from loss of property, occasioned by the burning of that quarter of the city in the summer of 1831. The blood presented much the same character as it does in such cases in this country, being black and tenacious. The ejected fluids also presented the distinctive aqueous or serous character.

DR. HEBERDEN, THE ELDER, ON PERSPIRATION.

As soon as the foregoing paper was concluded by the Registrar, the President read a letter from Dr. Heberden, containing numerous apologetic expressions relative to an essay "On Perspiration," written about ninety years ago by the elder Heberden his father, and which he presented to the President to be read at the meeting. This document affords a curious commentary on the opinions then entertained on some pathological points. The author stated that the ancients took no note of any less degree of perspiration than sweat. Sanctorinus had the merit of first calling the attention of physicians to the insensible transpiration. He illustrated in his works the nature of this action, and founded some ingenious theories on the facts which he had ascertained, and, like all discoverers, he was so much absorbed by the importance of the subject, that he referred every disease to a derangement of this function. Dr. Heberden gives, in this essay, a tolerably good description of the structures contributing to the perspiratory action. He expressed an opinion that the perspiration was not entirely excrementitious, but possessed some analogy to the bile, saliva, and pancreatic juice. The glands and terminal vessels situate on the surfaces of serous sacs, when occasional pouring out of lubricating fluids takes place, and which fluids are again absorbed, were, he thought, very similar in function with the skin which is bedewed by the oleaginous exudation of the cutaneous glands and follicles. The agency of the omentum majus in the abdomen affords a good example of the use of the skin as a recrementitious organ. This important peritoneal apron has the power of insinuating itself between the folds of the intestines, and exuding copiously its oil to lubricate the peritoneal surfaces, and facilitate their vermicular movements. The synovial glands also fulfil an analogous office in the joints; in fact, throughout the interstices of the body, there exist these oily secretions; and Dr. Heberden very ingeniously endeavoured to explain the indisposition and inability to move observable in fever, by the arrest and deficiency of these secretions, which produce a mechanical impediment to the various evolutions of the body.

The Doctor appeared to maintain an idea that the waste of life may be diminished by the adoption of means to moderate the evaporation attendant upon perspiration, and suggested the propriety of imitating the practice of the ancients in the inunction of the body, which he was convinced was a practice attended with the best effects. The paper was concluded by a curious inquiry relative to the practice of anointing amongst the ancient eastern nations, and especially amongst the Hebrews. Oil was always the vehicle for perfumes before the discovery of spirits of wine. The adoption of this more elegant liquid as a menstruum for perfumes, and the general use of linen in modern times have led to the abandonment of the salubrious custom of anointing. The production was marked throughout by great quaintness of style, and excited a languid interest.

Some very good drawings illustrative of disease of stomach were exhibited. Amongst others we noticed sphacelus of the stomach succeeding carcinoma, enlargement of mucous follicles, hemorrhage from the villous coat, &c.

ROYAL INSTITUTION.

Friday, May 11, 1832.

SPARKS FROM MAGNETS.

Dr. Richie, professor of natural philosophy to the institution, this evening stated that he had followed in the track of Mr. Faraday in his late brilliant discoveries, and was happy to say he had uniformly arrived at the same conclusions. He had also succeeded in making the spark, which had been obtained by Mr. Faraday in breaking the magneto-electric circle, visible to a large assembly. This was done by placing an explosive mixture of oxygen and hydrogen in the course of the spark, which immediately produced a loud report and a flash of light. He employed a horse-shoe magnet, between the ends of which were placed a couple of tubes; a wire was introduced into each, and their connexion maintained by a conducting medium. The wires were connected to the magnet by the folds of copper ribbon. The gas was introduced by a bladder and stopcock. The contact suddenly broke, and the spark was made evident by an explosion. Dr. Richie declared his belief that no spark could be elicited from any but a *temporary* magnet.

At this time Mr. Faraday came forward, and stated he had succeeded in obtaining a spark from a *natural* magnet. Mr. Faraday had borrowed Brown's magnet from the Academy at Woolwich. A small bar of iron, about six inches long, was used in contact with the extremities of the magnet. Two connecting wires were raised from each end of this small bar, and being bent at right angles, overlapped each other. The undermost terminated in a disc about the size of half-a-crown. By a rapid percussion of the bar against the magnet, the disc and wire broke in contact by their elasticity, and a beautiful bluish spark was rendered evident amidst the merited applause of the spectators. During these transactions, as pretty a scene of philosophical coquetry was enacted between the two experimentalists as was ever exhibited on any stage. But the good-natured and discriminating audience would pardon that, and much more, in men of such transcendent merit as Richie and Faraday.

In the library there was nothing worthy of remark, except specimens of a petrified *jungle* of gum, and other trees from the New Hebrides.

Friday, May 18, 1832.

MR. FARADAY ON THE CRISPATIONS OF FLUIDS UPON VIBRATING SURFACES.

On former occasions the lecturer had exhibited experiments showing several various phenomena of vibrating surfaces, by means of sand and other heavy and light powders. With the aid of square glass plates and a violin bow, curious results were obtained. Sand being sprinkled over the surface of the piece of glass, and a certain note produced by the application of the bow against the edge, a certain arrangement of the particles took place, and a regular figure was delineated. These figures were found to vary according to the note produced. It was ascertained that the aggregation of the particles took place on the quiescent or nodal lines of the vibrating surface. An apparent exception to the general law was discovered, and was for some time inexplicable. When a light powder, that of *acopadium* for example, was used, the accumulation did not take place on the nodal points or lines, but on the vibratory ones. This phenomenon Mr. Faraday found to be due to certain atmospheric currents which were excited by the vibratory surface.

Mr. Wheatstone extended his inquiries into the nature of the effects which vibrations might produce on the surfaces of fluids, and he tried with oil, water transparent, and coloured albumen and mercury. The experiments were performed on a variety of scales, and a satisfactory uniformity was observed in the results. A symmetrical elevation of certain points of the surfaces took place when the vibration was transmitted to the fluid. Upon vigilant examination, the tumours, hills, or crispations of fluid, were always square, and were arranged in a checkered fashion, one-half alternately ascending and descending. The effect was most perfectly shown with albumen, and least so with oil. The size of these crispations varied with the height or depth of the sound, for the baser the note, the larger the crispation with the albumen; the effect appeared like a fine web, formed with mathematical precision. From various experiments, Mr. Faraday thought he could establish as a principle, that all fluids were influenced by the same law, and that the intestine motion produced and evidenced by the agitation of the surface was dependent upon the conversion of the perpendicular impulse into a lateral one, so as to occasion the alternate and geometric tumefaction of the surface. This effect has no reference to the depth of the liquid employed.

After the lecture, Mr. Faraday exhibited a portrait of Baron Cuvier, by Pickersgill. He pronounced a eulogy upon that great philosopher and anatomist, who was an honorary member of the institution. Mr. Faraday also announced that the gold medal of the institution had been conferred this year on Gay Lussac for his late discoveries in chemistry.

THE LANCET.

London, Saturday, June 2nd, 1832.

The papers which we publish this day on the effects produced in several desperate cases of cholera by the injection into the veins of water containing the salts of the blood in solution, will be read with the most lively interest and satisfaction.

From the conjoint testimony of the highly-respectable individuals who have conducted the various cases, it appears certain that of fifteen hopeless and abandoned cases, five were rescued from apparently certain death by the treatment adopted, while of those which proved fatal, all but one had been complicated with such extensive organic disease, that no method of medication could do more than postpone a little the fatal event. That reprieve the injection seems to have procured. In short, according to the evidence before us, the method has only failed in one case in which it had been fairly tried—that is, where no organic disease had pre-existed, and where enough of life was left to sanction the least anticipation of success.

The most startling fact connected with the details of the treatment is the great quantity of fluid injected. In one case, nearly 7 lbs. were at once thrown in to the median basilic vein, and in nine hours 15lbs. were infused. But in this very case—a desperate example of the last stage of a protracted attack of cholera, after excessive purging and vomiting had drained the body almost dry—when the pulse had been imperceptible for hours, the skin livid, and the voice lost, *the patient completely recovered*! While the injection was performing, the pulse rose, the heat returned, the lividity disappeared, the countenance became florid and beaming, in short the patient underwent a change more like the workings of a miraculous and supernatural agent, than the effect of the interposition of medical science.

The case thus alluded to is we think one of the most interesting recorded in the annals of our profession. It gives us at least this all-important inference, that the injection of water in these great quantities is not necessarily fatal or even a formidable measure. It further entitles us to presume, that as organic chemistry improves, as our knowledge of the relation between the blood and remedies of different kinds increases, the art of treating diseases in general will be placed on a more rational and certain foundation. It teaches us how boldly we may proceed when certain and scientific data are before us, and it thus dispels the all-but-cowardly timidity which has hitherto prevented the principles of the treatment in question from being carried into effect.

With respect to the quantity of water employed, two circumstances are to be borne in mind; first, that the opinions which rate the quantity of blood in the adult body at the mean of thirty-five pounds only, rest upon no sure or experimental facts; secondly, that the blood forms at most but a small proportion of the fluids which the body generally contains—which facilitate all the movements, in the due perfection of which health and life are essentially concerned, which cannot be diminished without danger, and which cannot be lost without certain death. Now when we contemplate the phenomena of cholera, when we see the plump and vigorous limbs of youth and adolescence withered in a few hours to the shrunk dimensions of emaciated old age, it is impossible not to conclude that not only has the blood lost much of its water, as chemistry has so satisfactorily ascertained, but that all the living solids of the frame, its muscles, nerves, its vascular tunics, and its membranes, have been robbed of the bulk of fluid essential to the due discharge of their functions,—to the preservation of their vital condition. The quantity of water to be replaced is therefore immense, and bears no relation to the presumed quantity of blood which the human body naturally contains. The blood only participates in the general drainage. Unless we limit life to that fluid, unless we renounce the solidist pathology, and unless we overlook all the most obvious features of the disease in question, we cannot arrive at any other conclusion.

A rapid glance at the history of the administration of remedies by the veins may not at this momentbe destitute of value.

The truth of Harvey's doctrines had scarcely been admitted, when the wild enthusiasm of the disciples of the new physiology led them to believe that a path was now opened to the cure of all diseases. The cause of every malady was assigned to the blood. The cure was to be accomplished by substituting for the diseased fluid, blood drawn from a robust and healthy animal. Experiments were accordingly made on animals, and soon repeated upon man. Denys and Emerens, of Paris, were the first who dared the experiment and performed the *transfusion* of the blood of a calf into the veins of an idiot, who seemed to recover his reason. Various accidents, however, soon occurred to excite distrust in the practice, and the Parliament of France and the Sovereign Pontiff almost simultaneously issued edicts prohibiting its repetition. After a long period, however, Magendie demonstrated, by several most conclusive experiments, that transfusion of blood from an animal of the same species, is attended with no danger whatever. With what signal success Dr. Blundell, in this country, applied the principle to the treatment of uterine hemorrhage, it is needless for us to remark.

The idea of giving medicinal remedies by the veins is of comparatively modern date. Encouraged by Magendie's experiments on animals, Percy and Laurent injected, with success, the decoction of stramonium into the veins of a soldier affected with tetanus. M. Coindet, of Geneva, performed a similar operation with the solution of opium in an analogous case, and with instant advantage, so that the spasms which had resisted all other remedies immediately subsided, and the patient recovered. Several other cases of this description are on record, to which we need not more particularly allude. In the treatment of the diseases of animals, again, the practice was so extensively followed, that at the veterinary infirmary of Copenhagen, no other mode of administration of medicines was resorted to, it being found that injection by the veins rendered success more certain, and economized the consumption of drugs.

Previous to the proper experimental investigation of the chemical pathology of cholera, numerous speculations were made by different writers on its treatment by venous medication. In Germany transfusion of blood was tried by Scoutteten and Dieffenbach under the impression (founded on Hermann's erroneous statement) that the blood in cholera had lost its globular structure. The experiments failed remarkably, and one patient died almost instantly after the transfusion had been performed. Again, several writers in England who had never witnessed the disease themselves, reasoning on the "blackness" of the blood so prominently described by the Indian and Russian authors, suggested different means of restoring that fluid to the bright and florid colour which they took for the test of its healthy condition. With this view, one proposed the injection of the oxygenated salts, another of the muriate of soda, both being solely intended to redden the blood, and having no reference to its state of fluidity.

At length Dr. O'Shaughnessy's experiments having shown the deficiency of water, and occasional diminution of the salts, in cholera blood—having proved that its anatomical structure was preserved, and its capacity for aëration maintained, a more rational principle of treatment was pointed out. It was a necessary inference from these experiments that water *essentially*, and salts *contingently*, should be added to the blood before it could again discharge its functions. It was evident, as stated in Dr. O'Shaughnessy's report of his experiments, that water could only be restored in two ways, by absorption or venous injection, and that in the cases in which the power of absorption was irretrievably lost, venous injection left the only hope of a cure. Dr. Latta, of Leith, has now, for the first time, carried this suggestion into effect. We thank him for the intrepidity, scientific zeal, and assiduity, he has displayed. His example has been followed by Drs. Craigie and Macintosh, of Edinburgh, and we entertain no doubt that the practice will be repeated all over Great Britain, as a last resource in the desperate cases which have baffled the ordinary methods of treatment, and which would otherwise be abandoned to inevitable death.

We append to the Leith and Edinburgh papers a note we have received from Dr. O'Shaughnessy, containing additional suggestions on the subject.

ST. BARTHOLOMEW'S HOSPITAL.

CANCEROUS BREAST.—EXTIRPATION.

Eliz. Bridnell, of a dark olive complexion, is a native of Buckinghamshire, her father being a black. She is forty-two years of age, and of a spare habit. Her general health has not been interfered with till about two years ago, when she first took cognizance of "something wrong" in her left breast, discovering there a small lump. This gradually increased to its present size, its growth being accompanied by pain, sometimes acute, but not generally distressing till very lately. The swelling is situated just beneath the nipple, and is moveable upon the subjacent muscle. Disease does not appear to involve the axillary glands, and the other mamma is unaffected. She is the mother of four children, all of whom have received their lacteal nutrition from herself, and the youngest, nine months old, was suckled by her at both breasts till within the last six weeks.

Operation being prescribed, and acceded to by the patient, was resorted to on Saturday last, Mr. Stanley officiating *vice* Mr. Vincent. The patient being in the recumbent position, two semilunar incisions were made in an oblique direction, embracing the nipple, and meeting at their extremities. An oval piece of integument being thus demarcated, the diseased structure was by a free dissection separated from the cellular substance, and from the fibres of the great pectoral muscle, which appeared not to be intermeddled with. During this process the long thoracic artery was divided, whilst a pretty copious effusion took place from the mammary branches. A ligature was applied, and a search then made for any remnants of the cancerous structure. Mr. S. took out a small remaining portion, and likewise a gland from the axilla, after which the edges were approximated, the wound was covered with strappings, surmounted by a simple dressing, and the patient removed from the operating table.

There was no retraction of the nipple, which appeared perfectly uninfluenced by the progress of the disease. A section of the tumour exhibited a clear development of the characteristic morbid appearances. There was the hard firm *nidus*, from which shot off the white bands forming the medium of connexion with the surrounding parts. The tumour was about the size of a hen's-egg.

27. Report very favourable. The wound is healing rapidly and the general health indicates a speedy recovery, unless there should recur one of those sudden reversions

which are so characteristic of these cases, and so often mock the efforts of the operator.

Three other operations were performed on the same day—two by Mr. Lawrence, for congenital cataract in two children, brothers; and the other by Mr. Lloyd, for the removal of an adipose tumour, of twelve months duration, from the shoulder of a girl. Of the *former* we say nothing, because we were in the same predicament as most of the class—that is, we could see nothing but *heads*.

The case of *nævus*, mentioned the week before last, is doing well. The morbid growth quickly mortified, and on Tuesday, 16th, Mr. Lawrence detached it from its adherence. The sloughing has caused a rather extensive wound, from the minute vessels of which, blood issues whenever the child cries violently. A poultice is applied.

CASE OF LEPRA VULGARIS.

Mary Young, ætat. 30, was admitted into this hospital in the beginning of April, under the care of Mr. Lawrence. Her case exhibited a well-marked specimen of *Lepra*. She states that she is a married woman, and the eruption first made its appearance six or seven months ago, when she had been suckling twin children. Her circumstances in life are comfortable; her constitution she considers naturally strong, but within the last five years a change for the worse has taken place in her health, which during that period has been constantly very delicate. Previous to her admission here, she had been attending Mr. Lawrence as a private patient; but entered this institution at his suggestion, that gentleman being desirous, no doubt, of trying the effect of the regularity and certainty of hospital life. When we first saw the patient (on April 21st), the eruption was existing over the whole body save the face, prevailing particularly on the arms and legs. The spots were of a circular form where isolated, but gradually became confluent, forming clusters, and losing the original characteristic. The interspace between the white squamæ had the usual shining blush. About the elbow the affection assumed a thick rugose appearance, corrugating the integuments upon any movement of the joint. There were also the small origins of fresh maculæ springing up. The treatment adopted had been light diet, *calomel* and *pil. aloes c. myrrha*, grs. x, every other night. She was ordered to use the warm-bath, and *Ung. hydrarg. nitrat. Ol. olivæ, Cerat. cetacei*, in equal parts, as a daily application to the affected spots.

25. No alteration in the state of the eruption. There is much constitutional derangement. The patient throws her head from side to side, complaining of heat, pain, and a sense of unusual fulness in the cerebral region; the pulse is not high or strong. Twenty leeches to be applied to the temples. *Mist. ammon. acet.* ℥iss, every four hours.

27. The leeches were used yesterday, and seem to have afforded great relief. Improvement is observable.

May 1. Relapse; troubled with much heaviness; pain in the head, and lowness of spirits. *Vin. antim.* ʒss to be added to the *mist. am. acet.*

2. Rather worse; pain in the chest; pulse very weak; slight shivering. *Spirit. æth. nitr.* ʒss. *tinct. amant.* ʒj. *mist. camph.* ʒx. The draught to be taken every four hours.

5. Some improvement; bowels open. *Pulv. Ipecac. comp.* grs. viij, to be taken every night.

11. To-day Mr. Lloyd saw the patient for Mr. Lawrence, and ordered a resumption of the warm-bath, which he found had been discontinued on the ground of its disagreeing with her. Ordered also *Liq. Ant. Tart.* ♏ xxx, *Magn. Sulph.* ʒss., *Mist. Salin.* ℥iss. A draught to be made and taken twice daily. For the *Ung. Hyd. Nitrat.*, the *Ung. Hyd. rubrum* is also substituted.

All this while the eruption has remained nearly stationary. Her health is now beginning to improve, but to-day, the 14th, fresh spots appear; some on the nose. She is to rub in on *one* arm the *Ung. picis liquid.*

This was followed up, and the diseased action appeared to give way in a day or two. The little squamæ fell off, and the skin became softer. Not much difference was apparent in the effects of the two applications, but the general health was uncommonly improved, appetite was regained, and the heavy look left the countenance.

22. To-day the patient left the hospital in a state of convalescence.

LONDON HOSPITAL.

NÆVUS MATERNUS.

On the 4th of May, a little child, about eighteen months old, was brought to this hospital with nævus maternus, situated on the forehead a little above the left superciliary ridge. The swelling is of a blueish colour, about one inch in diameter, and when the child cries violently, the tumour becomes very much swollen.

Mr. Scott performed the operation by ligature, in preference to the knife, in order to avoid troublesome hemorrhage. Speaking on the subject, he observed that he had known many instances in which the parents would not allow of excision by the knife, when they had no objection to the performance by ligature, because, in the latter case, the operation was bloodless. The mode Mr. Scott adopted was that of transfixing the tumour with two pins, as used in the operation for harelip, thus dividing the nævus into four portions, and then drawing a strong ligature as tightly as possible several times around the swelling, until the tumour became exceedingly tense, by this process entirely cutting off all supply of blood to the part. The patient did not appear to suffer very great pain during its performance, as its cries ceased almost directly it was restored to its mother's arms.

22. The child was brought to the hospital again to-day. The forehead now looks very well; the part has begun to cicatrise.

HYDROPS ARTICULI.

There is now a young woman in this hospital whose knee is affected with this disease.

The patient (F. M.) is 24 years of age; she attributes the origin of this disease to cold, caught by kneeling on damp bricks in her occupation as a servant. The disease first made its appearance four months previous to her admission here, with pain in the joint, and weakness, followed by swelling, for which the only remedy she used was rubbing it with soap liniment. After a time she applied to a medical man who prescribed a blister to the knee, with continued rest, and gave her a letter of admission here. Her general health has always been pretty good; catamenia regular.

The appearances which the part presented on her admission (about four weeks since) were,—joint very much distended, the enlargement principally on the fore-part and sides, extending some distance up the thigh; the tumour soft, elastic, giving way to pressure, but the indentation filled up instantly on that being removed; fluctuation very distinct. When the limb is in the straight position, the enlargement appears to be principally on the inner side of the joint. When the knee is bent, the fluctuation is not so evident, as the swelling is then of course tense. There is no very considerable impediment to the motions of the joint.

The treatment adopted on her admission was, blisters to the knee, and the blistered surface dressed with the unguent. hydriodat. potassæ. Took *tinct. ferri concentrat.* ♏x, *ter die*, which did not appear to be of much benefit, and consequently was discontinued. The knee was then dressed with *unguentum hydr. c. camphora*, spread thickly on lint; next, strips of adhesive plaster, over which the *emplast. saponis*, spread on leather, thin rolled; after which, pasteboard splints, and over the whole another roller. With this plan of treatment the fluid has been in a great measure absorbed.

22. The knee was dressed again to-day in the same manner, omitting the pasteboard splints. The swelling is materially reduced, the knee being now nearly of the same size as the other, fluctuation much less distinct, and the patient shortly intends asking permission to leave the hospital.

Mr. Thackrah, of Leeds, is preparing a new and enlarged edition of his work on employments as affecting health and longevity, extending the subject of his inquiry to the general arts, trades, and professions of England. His first treatise had a particular reference to the employments of a clothing district only.

CORRESPONDENTS, &c.

The M.S. List forwarded by *XX* was at variance with a printed list which we also received, and being unable to decide which was correct, we published neither. If we receive another list from XX, it must be written in the clearest manner. He may receive both on application at the Office.

The "brief statement" will not read well in print.

ERRATA.

We are requested to make the following erratum in Dr. O'Shaughnessy's paper on the Chemical Pathology of Cholera:—

In page 226, 2nd column, 3rd table, for

	No. 1.		No. 3.
Water	8.54	....	8.66
Albumen	1.33	....	1.24
Salts	5.80	....	2.92
Urea	40.—	....	— —
Fatty matter......	1.43	....	1.28

Read

	No. 1.		No. 3.
Water	854.—	....	866.—
Albumen	133.—	....	124.—
Salts............	5.80	....	2.92
Urea	— 40	....	— —
Fatty matter	1.43	....	1.28

In Mr. Salmon's lecture, page 241, col. 2, line 2, for "*consistent* with birth," read "*coexistent* with birth." Line 6, for "resembles *his* parents," read "resembles *its* parents." Lines 11 and 12,—in the sentence "the same similarity may occur internally, *and* is visible externally," for *and* substitute *as*.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JUNE 9. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXII.

THE CRANIUM OF THE HORSE—THE FRONTAL BONES—SINUSES.—INFLAMMATION.—PERFORATION OF THE FRONTAL BONES.

Difficulty of the Subject.—Gentlemen, I approach the next, or that which ought to have been the first division of our lectures, with mingled pleasure and fear. I cannot forget the difficulties and perplexities with which I was environed, oppressed, dismayed, when, like you, I began to study the anatomy and the functions of these organs by which animals, biped and quadruped, are rendered conscious of surrounding objects, and enabled to move and to act among them. There was an intricacy, a mysterious complexity about every part of the nervous system, that long baffled my most anxious efforts to unravel or to understand. Can I then hope to render that plain to you which was so incomprehensible to me?—hence my fear. But some master minds have been at work since that time; the magic wand of classification has been waved over the confused and puzzling mass; and order, harmony, and beauty, have sprung into view—hence the pleasure with which I invite you to the consideration of the sensorial system.

The first and the most important division of our subject, was into the systems of animal and of organic life—the first including the mechanism by which external impressions are conveyed to the brain, and sensations are there produced, and the materials of intelligence furnished; and by which also volitions are conveyed from the brain to the organs of voluntary motion, and we are enabled to connect ourselves with, and to profit by, those objects of the existence of which the sensitive nerves had rendered us conscious;—the second confined to circulation, respiration, digestion, secretion, and the other simple functions of life itself:—these systems proceeding from different sources, or referable to different centres:—in a manner independent of each other, yet continually anastomosing for their mutual benefit.

Division of the Animal System of Nerves.—This plain and simple division removed, or materially lessened many of our difficulties—but still, when we entered on the consideration of any portion of the animal system of nerves, there was perplexity enough and to spare; and to trace a single nerve from its origin to its ultimate destination, unravelling all its intricacies, and comprehending the meaning of them all, afforded labour for weeks to the most diligent and talented student. Then came the subdivision, little inferior in importance to the first, and immortalising the name of Bell—the divisions of the spinal chord—the six columns referable to distinct functions—the central ones of the superior surface devoted to sensation—the corresponding ones on the inferior surface to volition—the track of medullary matter belonging to these columns distinctly traced up into the brain, and the functions of each referable to different portions of the brain—the singularly different manner in which the nervous fibriculi spring from the chord, and approximated to each other, and become enclosed in one common sheath, yet intermingled not in their substance or their power, but constituting a compound nerve of sensation and volition:—the satisfactory experiments by which this was proved; the facility with which by cutting or tying either of its roots it could be deprived at our pleasure of its motor or its sensitive power,—here was a clue afforded to all the labyrinthian anastomoses of these nerves—the union of their motor fibrils to produce the consentaneous action of numerous muscles, for the power of many is combined in the performance of the slightest motion; while the sensitive fibrils probably pursue their course, with little intermingling, to limit the region of pain, and to secure the simplicity and perfection of the sensation.

The Lateral Columns.—The discovery of the lateral columns of the spinal chord, also contributed materially to the simplification of the nervous system generally, and to the rapid improvement of physiology—not to be traced into the brain—proceeding from those nuclei of medullary matter—those little brains, the corpora olivaria—placed between the sensitive and the motor columns—possessing the motor origin and the sensitive ganglion—combining the power of the two, yet in a singular way combining it, and evidently for a peculiar purpose—being, as it were, the motor and the sensitive nerve, not of the respiratory alone, but of the organic system generally, for it goes to all the thoracic and abdominal viscera and leaving to the ganglial nerves proceeding from the superior cervical ganglion, or those radiations of organic influence from the semilunar ganglion—leaving, I say, to them the chemical or the vital power of secretion and nutrition.

Such is an outline of the path we have to travel for a few lectures—interesting, and not I trust so mysterious as the nervous system was once supposed to be.

The Brain the Common Sensorium. — I will commence with the nerves of animal life. I will not enter into the dispute, whether the nerves are prolongations of the brain, or the mere conductors of impressions to the brain and of the power of motion to the muscles, but I will consider in popular language the brain as the common sensorium of animal life, every sensation being referred to it, and the power of voluntary motion being derived from it, or its prolongation in the spinal canal.

The Cranial Cavity.—I find the brain in a bony cavity situated in the postero-superior part of the head of the quadruped. This cavity is rounded above, and flattened below—it is broader anteriorly than posteriorly—longer than it is wide in the proportion of about seven to four, and wider than it is high. It is composed of several bones. The *frontals* form the anterior portion of the roof, and the superior and anterior portion of the side walls; the parietals constitute the central and greater part of the roof, and the central and superior division of the sides; the *temporals* occupy the lower portion of the sides; the *occipital* the postero-superior part of the roof—the whole of the back wall, and the posterior division of the floor; while the anterior portion of the floor, the antero-inferior one of the sides, is made up of the ethmoid bone, and in the centre of the base is found the sphenoid bone.

Why so many Bones?—So many bones enter into the construction of the cranium,—not that, imperfectly formed at the time of birth, they may overlap each other, and enable the head to pass more easily and with greater safety through the pelvis, because, from the horizontal position of the body of the quadruped, the pelvic cavity may be considerably enlarged without fear of abortion; but the young quadruped is soon upon his legs and exposed to danger, and these numerous bones form so many nuclei or centres of ossification, by means of which the skull is more rapidly formed, and the brain securely defended.

The Frontal Bones in the Horse.—The frontals occupy that portion of the skull which we term the forehead. They are very irregularly formed and difficult to describe. Their external surface is somewhat convex in the young animal, and also in the cart-horse breed, but it becomes flatter in the adult subject, and always flat in the thorough-bred horse, and even sometimes depressed over the frontal sinuses. They are united together by a straight suture, a prolongation of the nasal one. The construction of this suture deserves attention. Superiorly it is dove-tailed, intricately so, but as we trace it downward it becomes less complicated, and at length is little more than a mere approximation of the bones. Above, the frontals have to protect the brain, but lower down the frontal sinuses alone are found. Nature proportions her care to the importance of the object. This is one of those instances of contrivance which it will be my duty to point out to you, and which I trust you will form the habit of observing and contemplating with interest and advantage.

Nasal and Lachrymal Connexion.—At the lower part of this suture, small processes of the frontal bones are received between the nasals. We will pursue the frontal on either side, now receiving within its anterior concave border the posterior convex extremity of the nasal bones, and there we find a suture denticulated and squamous—a suture of peculiar strength. The anterior apices of the nasal bones are separated from and unsupported by the anterior maxillary bones, except by a small interposed process. The effect of a blow falling there, can be easily calculated on the principle of the lever, and there must be a considerable weight or firmness of attachment at the other extremity, to prevent its being wrested from its situation.

We now arrive at the os unguis, the suture between which and the frontal, takes a direction upwards and outwards and then downwards, including a rounded process of the lachrymal bone, and bringing us to the inner canthus, or angle of the eye, where again is a beautiful suture, dove-tailed and squamous, to give strength to the orbit.

The Supra-Orbitar Foramen.—We now enter the orbit of the eye, the roof of which is formed entirely by the frontal bone; and in that roof you will observe a foramen, the *foramen supra-orbitarium*, through which passes the *supra-orbitar artery*, a branch of the internal maxillary artery, which comes from the bottom of the orbit, and ramifies upon the skin and cellular membrane of the forehead, and, anastomosing with the anterior auricular and temporal arteries, forms a communication between the vessels within and without the orbit. The *supra-orbitar nerve* likewise escapes through the same foramen. It is a branch of the ophthalmic, given off as soon as it enters the orbit, and after climbing up to and crossing the roof, and giving ramifications to the fatty matter of the eye, and to the upper lid, finally expanding itself on the integument of the forehead.

The External Orbitar Process.—The projecting edge of the frontal bone, which thus forms the roof of the orbit, presents a peculiarly roughened appearance; it is formed by numerous minute bloodvessels passing into the bone for its nourishment, and some of them passing through the bone. A process is now observed, completing the superior and posterior part of the orbit, and resting upon the zygomatic arch, to which it gives additional strength. The process itself is exceedingly strong; it presents a sharpened edge anteriorly, a rounded one posteriorly, and terminates in a kind of tubercle on the zygomatic arch. The whole of this part of the frontal bone is exposed to considerable danger. Many a heavy blow falls upon it, and if we only considered the brutal way in which a tight collar is sometimes forced on, we should see sufficient necessity for strength here.

Fracture of it.—Mr. Pritchard had a case in which it was split through longitudinally from the foramen to the zygomatic arch: it was the consequence of brutal violence. Two detached portions of bone overhung the eye, and interrupted vision, and the integument was extensively lacerated. He carefully replaced the separated pieces of bone; brought the integument over them, and kept the edges in contact by means of silver pins, and in a fortnight the bones had united, the wound had healed, and the eye was uninjured.

The Orbit.—We now trace the frontal bone deep into the orbit. It becomes all at once very thin, for it is out of the way of danger, and nature adapts the strength of every part to the function it has to discharge, or its degree of exposure to injury. This plate is called the *internal orbital process*, and it occupies a considerable portion of the orbit. It is in a manner divided into two by one of the wings of the ethmoid bone, which is received into it as into a kind of notch, and projects far up the orbit opposite to the external orbital process.

The Temporal Fossa.—Following the posterior division of the internal orbitar process, we arrive at the *temporal fossa*—the pit or hollow above the eye by the depth of which we sometimes guess at the age of the horse. There is a great deal of fatty matter placed at the back of the eye to facilitate its motion. In illness, attended by loss of condition, and as old age comes on, a portion of this fat is absorbed; the eye sinks in the orbit, and the pit above it deepens, and by the degree in which this takes place we rudely guess at the age of the horse. It is said that some low dealers, who are capable of every species of cruelty as well as knavery, attempt to obviate this in some measure. If they have an old horse that does not much show his age in other ways, they puncture the skin at about the centre of the depression, and, introducing a small hollow tube, or even a tobacco-pipe, they blow into the orifice until so much air is forced into the cellular texture, that the hollow nearly disappears. This, assisted perchance by some *bishopped* teeth, may impose upon the unwary; but the deception may be easily detected by merely pressing upon the part, when the unnatural yielding and sinking and the escape of the air cannot be mistaken. In this fossa plays the coracoid process of the lower jaw, around which the temporal muscle is wrapped. The consideration of this belongs to the digestive system. More inferiorly in the fossa, the frontal bone comes in contact with the sphenoid, and at their point of union, or that of the ethmoid, the sphenoid and the frontal, is the *internal orbitar foramen*, through which escapes the lateral nasal nerve.

Leaving the ethmoid bone, we find the frontal in union with the squamous portion of the temporal bone. From the nature of the suture, this portion of the temporal derives its name; it is squamous, one bone overlaps the other like a shell. The temporal overlaps the frontal;—it is that additional strength which we should place on the side of the arch and near its base, to prevent its spurring out.

The Coronal Suture.—Emerging from the temporal fossa, we meet the parietal bone, which is united to the frontal by a suture, partly squamous and partly dove-tailed, and of great strength, for the brain lies immediately underneath. The suture first takes a direction upwards and forwards, and then somewhat posteriorly, and in a curved direction across the upper part of the forehead, including at the angle at the base of the orbitar process, a portion of the parietal between the expansions of the frontal: and if we pursue it across the forehead, a convex projection of the frontals is received between the concave anterior extremities of the parietals.

The Levator Palpebræ Superioris.—There is one muscle only belonging to these bones on either side, the *levator palpebræ superioris*, or elevator of the upper eyelid. It arises by an aponeurotic expansion from the forehead; narrows, becomes pyramidical, and is inserted into the superior part of the upper eyelid, nearer the inner than the outer angle. It elevates the lid, and, as you will observe when you look at the horse, more towards the inner than the outer side. It unites itself with, or in a manner blends with, the orbicularis muscle, and is an antagonist to it; but more of this when we have to treat of the eye.

Phrenology as applied to the Frontal Bones of the Horse.—Of the truth of the science of phrenology as founded on the study of the human skull, it becomes not me to speak; but I confess that I can derive little illustration of it from the cranium of the horse. There is certainly a material difference in the form of the frontal bones in different breeds of horses. The convex and narrow frontal of the cart-horse, forms a striking contrast with the flat and broad forehead of the thorough-bred horse. These two crania will give you a tolerable idea of this; but you must study it in the living animal to comprehend its full effect: the broad and angular forehead of the one, the prominence of the eye consequent upon this, and the face gradually tapering from the forehead to the muzzle, are beautifully contrasted with the long and large face of the dray-horse, scarcely tapering at all downwards, and the forehead but little wider than the face:—the fire and intelligence of the one, with the quietness, if not the stupidity, of the other; but this belongs to physiognomy, and not to phrenology. No irregularity of surface, or development of any portion of the brain, is concerned here, no portion of the cranial covering is visible here; but the frontal sinuses, or the diploe continued above them, interpose between the brain and that portion of the frontal bone which covers it, and still higher up the temporal muscle enveloping the proper skull-cap, would bury from observation any irregularity of external surface, which the irregularities on the surface of the brain might occasion.

The Frontal Sinuses.—The frontal bone is composed of two tables or plates, which recede considerably from each other, and form large cavities, called *the frontal sinuses.* They extend, anteriorly—posteriorly, from an imaginary line drawn from the inner angle of one eye to that of the other, to another imaginary line drawn across the forehead at the middle of the orbitar fossæ. Laterally they reach almost from one supraorbitar foramen to the other, and posteriorly a little further, for they penetrate along the rounded edge of the orbitar process. They are perfectly divided from each other by what seems to be a bony septum, continuous with the cartilaginous one of the nostrils; or rather by a double septum, for the plate belonging to each side is sufficiently evident in the macerated head. Is this separation between the frontal sinuses, like the perfect interposition of the septum in the nose of the horse, connected with that fatal disease of the Schneiderian membrane, to which it was foreseen the horse would be so liable? Was it designed to prevent the spread of mischief by cutting off all communication with the neighbouring parts?

Divided into Cells.—The sinus on each side is divided into various cells, but the septa here are imperfect; they all communicate; and they communicate also with the maxillary sinus, and with the ethmoidal and sphenoidal sinuses, and also by means of the valve under the superior turbinated bone, which I have described in former lectures, they communicate with the nasal cavity.

Extent.—The extent of the frontal sinuses varies in different subjects. In the fœtal colt they are scarcely discernible; in the colt of a twelvemonth old they are fairly developed, but they increase with the years of the animal, and in the aged horse they reach not only through the whole of the frontals, but penetrate far between the plates of the parietal bones.

Lining Membrane.—They are lined by a prolongation of the Schneiderian membrane, —thinner, as vascular, and highly sensitive. On this account it soon sympathises with and shares in the maladies of the Schneiderian membrane. In severe catarrh the horse expresses much pain when gently tapped on the forehead; and he rarely dies of glanders without the membrane of the sinuses being ulcerated, and the cavities filled with pus.

Inflammation of the Membrane.—The frontal sinuses are occasionally the seat of intense inflammation unconnected with any general affection of the Schneiderian membrane. The cause can rarely be traced. The horse is suddenly attacked; he refuses his food, he hangs his head, appears to be stupid, half unconscious; the mouth is hot, and the pulse hard and quick—it is like the beginning of staggers, and in its second stage it more resembles that disease, for a state of the highest excitation succeeds, and that as sudden as was the first attack: the pulse becomes quicker and the artery throbs more violently; the nose and conjunctiva are now intensely red; the eye is prominent and glaring—a strange disposition to mischief is displayed; the horse kicks and plunges, and more particularly attempts to bite. It is now phrenitis; the inflammation has been communicated by contiguity to the membranes of the brain, yet the symptoms do not quite correspond with those of pure phrenitis. There is a half consciousness which is not observed in staggers, an intention to do mischief; and on the other hand, the incomplete consciousness distinguishes it from rabies, under which the horse knows perfectly what he is about, and artfully, as well as in the most determined way, seeks mischief for its own sake. The evident heat on the forehead, and the shrinking of the horse when struck on it, would characterise this disease in its early stage. The early progress of the inflammation may be arrested, but when the brain has become affected the case is usually desperate. For the best account of this disease, fortunately of rare occurrence, we are indebted to Mr. John Field, jun. It used to be confounded with phrenitis or rabies, and the animal was either destroyed or the proper measures were neglected.

Treatment.—Of course we should bleed, and that from the jugular, and copiously, and repeatedly. Cold applications, of which ice is the most convenient as well as effectual, should be made to the forehead, succeeded by blisters over the frontal sinuses. A strong dose of physic should be immediately administered. The trephine has been spoken of—we should certainly have the advantage of local bleeding, but by our laceration of the membrane we should probably increase the local irritation.

Post-mortem Appearances.—The membrane will always be found in the highest state of inflammation and injection, and the sinuses generally filled with clots of blood. The brain or its membranes will usually exhibit inflammation of the intensest character.

Its Chronic Form.—In a few cases it will readily yield to decided treatment; in others it assumes a more chronic form, recognised by a purulent and sometimes bloody discharge from the nostrils; unaccompanied by cough, and distinguished from glanders, the want of viscidity in the discharge by there being no ulceration in the nostril or enlargement of the glands, yet easily mistaken for glanders since it usually proceeds from one nostril alone.

Tumours on the Frontal Bones.—Small fluctuating tumours are sometimes observed on the frontal, or nasal, or malar bones, varying from the size of a marble to that of a large nutmeg, and oftener in the half-bred horse with much loose cellular substance under the integument than in the thorough-bred one. They are little cysts, with parietes of considerable thickness, and filled with a glairy, yellow, or limpid fluid. They constitute a little dissight, but there is no danger about them; I never knew one proceed to ulceration. In an early stage they may be readily dissected, or rather turned out; but when they have existed for some weeks or months, it will be prudent to let them quite alone. The long pressure of the dense cyst upon the bone beneath will frequently produce a partial or total absorption of it, and nothing will remain but the periosteum. I was once desired to open a large one on the frontal bone. I had not thought of this effect of continued pressure, and my lancet penetrated into the frontal sinus. Two months passed before I could heal the wound, and then it was accomplished by granulations which sprung up from the very bottom of the sinus.

Use of the Frontal Sinuses.—Of the use or function of the frontal sinuses, I have little to say—they may render the voice more sonorous—they may add to the exquisiteness of the scent, by the odoriferous effluvia loitering in the labyrinth of these cells—or this portion of the skull may have been left hollow, that the weight of the head might not be unnecessarily increased, the parts beneath being sufficiently secured by the arched form of the floor of the sinuses.

Trephined for the Discovery of Glanders.—There are few situations of greater perplexity and annoyance, than when we are called upon to decide on the existence of glanders in an incipient stage. You will please to recollect what I have said in former lectures of the difficulties with which we are here environed. I will suppose that there may be no visible ulceration in the nose—no adhesion of the gland, or even enlargement of it, but a slight discharge, and that not viscid, yet too constant. Experience has forced upon me the great but too often unsuspected danger of contagion. Thousands of horses are lost every year when no one dreamed of mischief. I must give a prompt and decided opinion upon the case—the convenience—the interest of my employer—my own reputation, depend upon it. Inoculation would be the most satisfactory test; but I may not have a condemned horse or a poor ass at hand, or I may a little doubt my right to experiment upon them. Can I detect the existence of ulceration in the upper portion of the nasal cavities? It has been thought that I can, by opening the frontal sinuses. I have spoken of their communication with the nasal cavity. I know precisely their situation. I feel for one of the supra-orbitar foramina—I trace an imaginary line across the forehead to the other, and that line will mark the deepest part of the sinuses. I have them there more than an inch in depth, and a little on either side of the frontal suture, I can perforate with safety. I do not like the *spill-gimblet;* it is a mere carpenter's instrument, and does not look well in the hand of a veterinary surgeon. I use a small trephine, and then injecting a little tepid water into the sinus, it runs through all or the greater part of the cells, it enters the maxillary sinus, and perhaps the ethmoidal and sphenoidal cells, and finds its way into the nasal cavity, through the valve under the superior turbinated bone. If it is discharged from the nostril mingled with pus, I have satisfactory proof that there is ulceration somewhere in these cavities, and probably the ulceration of glanders; but if it is merely tinged with blood, or thickened with mucus, am I to conclude that the horse is not glandered? No, for I must remind you that by this operation you do not reach the superior meatus of the nose at all; and that the upper and most suspicious part of the central meatus, and where ulceration is oftenest and soonest found, is not even approached. You will also remember that glanders may long exist in an insidious yet highly contagious form without the slightest ulceration. You will then see the value—the little value of this operation. You may now and then detect the existence of glanders, but you can never be assured that a horse is not glandered.

Perforation of the Frontal Bones in the Treatment of Glanders.—It was formerly the custom to inject many liquids into the nostrils in this way, some of them harmless enough, but others most cruelly acrid. I have sufficiently reprobated this practice when speaking of glanders. It betrays alike unpardonable ignorance of anatomy, and of the nature of the disease. Never disgrace yourselves by having recourse to it, or to the equally objectionable practice of injecting caustic or irritating liquids up the nostrils.

Inner Surface of the Frontals.—The inner plate of the frontal bone is divided into two unequal parts by an irregularly-formed lateral septum, as you will see in this section. The superior portion forms the superior and posterior part of the floor of the sinus, and the anterior and inferior part of the roof of the cranium. The inferior portion forms the superior part of the roof of the nasal cavity, and the anterior and inferior part of the floor of the sinus. The cranial portion of the frontal bone covers the anterior lobes of the brain, and there it is thickest and strongest, and especially thickest where strength is most wanted—namely, at the centre or roof of the arch, to the security of which, next to that of the base, the attention of the builder is directed. We there find a considerable crest or spine extending from the crista galli of the ethmoid bone, along the frontal and parietal bones to the tentorium. In the centre of this ridge is a slight groove for the reception of the longitudinal sinus. The inner plate of the bone on either side of the ridge is studded with depressions, corresponding with irregularities on the surface of the brain.

ESSAYS
ON
ANIMAL ACTION.

By William Dobson, *Esq., Surgeon.*

No. V.

RESPIRATION AN INVOLUNTARY ACT.

Is respiration a voluntary or an involuntary action? The mind is conscious of the existence of the respiratory process during the waking hours only, and when the attention is applied to the organs of breathing. When the intellectual powers are engrossed with an interesting topic, as when occupied in solving a mathematical problem, or when we are listening to the effusions of an orator and a genius, or when witnessing a dramatic representation, breathing is repressed often for a considerable period, yet, even on those occasions, the phenomenon can scarcely be attributed to a voluntary act. The individual is unconscious of the condition of his respiratory organs. His mind is abstracted from every other object; he listens, he thinks, and he admires. But no sooner is the attraction removed, than breathing resumes its accustomed play. During sleep, when the intellect has ceased its operations, and when the senses are no longer perceiving surrounding impressions, and when every acknowledged voluntary action is totally at rest, respiration still goes on. The mind is now passive, it issues no mandates, all its operations are suspended, and it is therefore illogical and unfair to presume that so important an action as breathing can be executed constantly by mental dictates. That it can be accelerated or retarded at pleasure no one will dare to deny, and that we can maintain by an effort of the will, an increase or diminution of the chest's velocity, is equally tenable; but ought we thence to infer that volition is ever the exciting cause of the motions of the chest? That this can be imagined for a moment to be the case, argues a false conception, or a perverted imagination, in him who thinks so. Does not voluntary motion imply the existence of volition? And does not volition equally imply the entity of mind? And if mind during sleep be annulled, or the material organs through which mind is exhibited be inapt for the exhibition, how can the effects be produced, or the ordinary consecutive phenomena occur? For voluntary motion to result, or motion to be effected with a determinate object, the will must have a specific intention in view; and where mind is not operative, it is impossible for a voluntary act to be induced. But metaphysical disquisitions will not solve the problem. Rational deductions from acknowledged data are what is intended. Then the conclusion which is inferred from these premises, and from a careful balance of the arguments adduced in favour of each doctrine, is, that *respiration* is most essentially, and most indisputably, an *involuntary operation*. Conceding that volition can exert a material influence over the movements of breathing, that it can regulate and subject them to its dominion for a limited duration, yet ultimately they emerge from its control, and play, even amidst the mental ruin to which the mind is liable; respiration will exist, when, to all evident appearances, every spark of mind, and every insignia of its essence, are annihilated. Need we refer for other illustration than to the apoplectic man, whose every animal action is abolished, and who merely vegetates like a plant which has a stationary existence? He respires, he survives. Motion to him is nothing, existence is nothing; he knows not of either; he is dead to every surrounding thing, incapable of appreciating or of influencing external bodies—unable to govern his material being—unconscious that he lives.

The influence of the cerebro-spinal system over the respiratory and circulatory functions has not been satisfactorily established. However zealously the subject may have been investigated by experimental physiologists, more especially by Le Gallois, and our talented countryman Dr. Wilson Philip, it is still concealed by the veil of obscurity. Two palpable errors appear to pervade the whole of the inquiries which have been instituted.

1st. When such extensive lacerations and sections, and destructions of the large nervous masses, are effected, it is both physically and morally impossible that the machine can perform its normal actions. If the functions do continue, they must necessarily be abnormal; if they cease, the inflictions ought *not* to be considered as the immediate cause or the proximate antecedence.

2dly. Numerous links in the chain of reasoning, numerous circumstances of vital importance, are entirely omitted. They perform an experiment and observe a certain effect, and infer that the effect is the *immediate* consequence of the experiment, whereas it is easy to perceive that intervening causes may exist.

Does it imply, that when the spinal marrow is divided above the origin of the phrenic nerves, and the diaphragm ceases to act, that the action of this organ is necessarily dependent on the brain or the spinal marrow? No; it evinces, that the entireness of the physical conjunction of two or more organs, is requisite for the maintenance of their individual functions in a state of integrity. Yet it develops the independency of each organ, and it displays the beautiful contrivance of a pre-established harmony in the works of nature, where numberless and varied operations are carried on simultaneously, and yet entirely independent of each other.

That the physical destruction of one portion of the nervous system impairs the faculties of another, whether it be contiguous or remote, can be readily understood, and is by every one admitted. But to say, when the spinal marrow is destroyed, either wholly or partially, and respiration is suppressed, that the functional power of the respiratory organs was derived from the mutilated part, is a heresy in reasoning. What are the intervening links which may be presumed as *causes* inciting the respiratory function to act? I answer, the *nerves*! I affirm that the phrenic nerves *themselves* are adequate to excite the diaphragm to constant motion, and capable of maintaining this motion from the period of birth to the dissolution of the frame: That the phrenic nerves develope a principle, a force, which impels the muscles of the diaphragm to contraction and relaxation. Whatever this principle may be we know not, and it will probably ever remain inscrutable. Whether it be a mechanical, or a chemical, or a galvanic power, or depend on the supposed agency termed life, we need not inquire, for the inquiry would be vain.

The nerves must be in a healthy condition, have a constant supply of scarlet blood, and be conjoined with the surrounding nervous textures, that harmony of their functions may result.

In apoplexy, even in the most aggravated cases, a man can swallow. This circumstance has been adduced to support the idea of breathing being a *voluntary* operation. But it is only a negative argument, and it can be applied as a strong corroborative of the contrary notion. When any substance is received into the mouth of an apoplectic patient, it is swallowed; and the query naturally results, What directs the muscles to contract? The phenomenon of deglutition implies the existence of two distinct agencies,—sensation and motive power, both of which are conceived to reside in the nervous system. Now in apoplexy, animal sensation, or rather cerebral sensation, is abolished, and its motor influence is never exerted; we may therefore presume, that impressions on the fauces are not perceived by the brain. Then we ask the question, By *what* are they perceived? I answer, *by the nerves of the fauces.* Reason and investigation concur to prove their ample sufficiency; and not only to perceive the impression, but to incite the deglutory muscles into action. Thus, when compressed by food, or when placed in contact, a mandate emanates from the nerves, and impels the muscles to contraction. The nerves are the doorkeepers to the alimentary canal, and they are empowered to fulfil their functionary duties, when the governing being is in a state of nullity, either during sleep, or in the apoplectic condition.

Now by the same parity of reasoning, which proves that "swallowing" in the apoplectic is involuntary, we may establish the position, and on a most undeniable basis, that *breathing* is ever an *involuntary* act.

When the body is healthy and vigorous, respiration is slow and easy; when disordered and debilitated, generally rapid and painful, and the will can effect no regulation of its movements. There are times when respiration is like the fiery steed, which has bounded from the hand of its rider, and though he may be able to check its course for a moment, it immediately regains its former velocity.

LITHOTRIPSY.

To the Editor of The Lancet.

Sir,—I beg to transmit for publication an epitome of a memoir on the evacuating catheter lately read (in conjunction with the memoir upon the means of destroying stone by percussion, before the Academie Royale). Both papers are offered in competition for the Montlyon prize, which is periodically awarded by that institution.

Baron Heuteloup apprehends I was not sufficiently explicit in stating his reasons for changing the name of the operation from *lithotrity* to *lithotripsy*. He justly thinks, that the name of an art or science should not be lightly altered. But in the case of lithotrity he is sanguine the profession will agree with him, that a term which expresses only one of the operations resorted to, that of boring, which is the least efficient, is very inapplicable, and inadequate to represent the diversified resources which this art now possesses. The Baron has ventured to adopt the term *lithotripsy*, because it implies simply and generally the destruction of the calculus. In the case No. IV., of Mr. G., ætat. 62, related in the last memoir, I observe an erratum, whether clerical or typographical I know not; the stone is stated to be *ten* instead of *twenty* lines in diameter, a material circumstance, as there were *six* applications of the "percuteur." I have one or two cases illustrative of the new system nearly ready for printing, and which I shall send in a day or two.

I am, Sir, your obedient servant,
D. O. Edwards.

A MEMOIR

ON LITHOCENOSIS;*

OR THE ART OF BRINGING AWAY THE

FRAGMENTS OF STONE

PRODUCED BY THE ACTION OF THE

LITHOTRIPTIC INSTRUMENTS.

Lithotripsy presents two principal problems for the surgeon to solve. The first is to break down the calculus, and the second to bring away the broken particles.

The efforts of surgeons hitherto have been exclusively directed to the solution of the first problem, which naturally demanded the more immediate attention, not only as being the most important part of lithotripsy, but because the necessity of the second was a consequence of the solution of the first.

It was always of the greatest importance to bring away the detritus in cases where the contractile power of the bladder was not sufficient to expel them, for if the fragments remain in the organ, the comminution of the stone would of course be useless.

There are many patients too, who, from a morbid or perverted sensibility, or from natural or acquired anomalies in the conformation of the parts, prevent the stone, when reduced, from being ejected. In such cases the hope of recovery is slight. Many instances might be given of patients being unable to void the fragments on account of the neck of the bladder being obstructed by fungous prominences or swollen veins. Others from an enlargement of the prostate gland compressing the neck of the bladder, and just allowing the urine to escape without any prosilient force. The same effect may be produced by the tumefaction of the third lobe of the same gland; and, finally, in other instances, the free issue of the detritus, by an irregularity in the irritability of the bladder, causing the muscles of retention, and those of expulsion, to contract at the same time, and thus obstruct the flow of urine, which can only be full and strong when the contractile agents proceed with a synchronous action. In fact, to support such a stream of urine as is adequate to carry away the calculous fragments, the muscles of retention must relax, whilst those of expulsion contract.

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* λιθος, κενωσις evacuatio.

If we reflect on the numberless causes which may oppose the natural ejection of the broken portions of stone, we must admit the urgent necessity of inventing suitable means for their removal, and may feel surprise that lithotritic surgeons should have considered their art complete whilst such palpable deficiencies existed. Having met with many cases in which the necessity for contrivance for the abstraction of the retained particles was manifested, I have endeavoured to supply such a desideratum. At the commencement of my practice, I was grieved to meet with several patients whose condition, from causes already described, was such as to preclude all hope of recovery or relief. But more recently I have experienced a satisfaction proportionate to my former chagrin, in the successful application of an instrument which seems equal to all the wants that existed.

Before I present this instrument to the Institute, it may perhaps be advisable to give a general idea of the means which, even before the invention of lithotrity, were employed to bring away gravel, or the fragments which were occasionally produced by the spontaneous division of stones in the bladder. These means were not only feeble, but extremely dangerous in their application.

The first instrument chosen for this purpose was merely a gum-elastic catheter, which was introduced into the bladder in order that the debris should be carried through it with the urine, and that the larger pieces should be extracted by becoming entangled in the eyes or terminal apertures of the canula.

The second instrument was one proposed by myself. It was a metallic catheter, of which the stylet was to facilitate the carrying away of the sabulum, by allowing frequent injections of water. This stylet was also intended to serve in the comminution of such particles as might become entangled in the eyes, and project too much to admit of the withdrawal of the catheter.

It may easily be conceived, that the first of these means afforded very insufficient assistance, and must occasionally have subjected the patient to such serious mishaps as the laceration of the urethra by the spiculæ of the projecting fragments; and that the second, although a little more efficient perhaps, exposed the surgeon in an equal, or even in a greater degree, to the danger of lacerating the urethra. The metallic catheter being larger than the gum-elastic one, and consequently admitting the entanglement of larger pieces of calculus, such as the stylet had not power to break, and being but imperfectly constructed, proved, ultimately, but of little use. When a large piece of stone became fixed in the eye of either of these instruments, it occasionally became impossible to disengage it, and being drawn into the urethra, the surgeon has found himself in the dangerous dilemma, of either forcibly pushing the instrument back into the bladder, which is often impracticable through the barbed position of the fragment, or, which is sometimes the only alternative, by dragging it out with violence. No wonder can be expressed that laceration of the urethra, and sometimes infiltration of urine, should occur.

Unfortunately I have myself to deplore a similar unfortunate case. An old man of 82 years, upon whom I had operated successfully, was unable to expel the detritus. In order to help him, I employed the metallic catheter just mentioned. At first a considerable quantity of the powder came away, but a fragment having lodged in the eyes, and the instrument being drawn into the urethra, I could not by the most persevering endeavours rid myself of it. The only alternative left was to draw the instrument towards me; notwithstanding all the care and precaution with which I did this, the urethral lining was torn, and an infiltration was the consequence. As several fragments remained in the bladder, this was a state of things which required that lithotomy should be forthwith performed, and this was accordingly done.*

Struck from this moment with the fearful consequences of an accident arising from the inability of certain patients to evacuate the reduced portions of stone, I undertook with the greatest fear and unwillingness, any cases in which I could not foresee almost a certainty of the fragments being readily expelled by the spontaneous efforts of the bladder, and I even declined operating upon several, from these just apprehensions. Amongst these were a considerable number of patients advanced in years, and whom I considered fit for lithotripsy, from their frequently possessing, in addition to some of the usual causes which impede the expulsion of the urine, so little contractile power of bladder, as to render their recovery, especially in cases of large calculi, at all times tedious, and sometimes hopeless.

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* Vide postscript.

The only way in which the beneficial effects of this important operation could be extended to old men as well as to adults, under these unfavourable circumstances, was to construct an instrument presenting large openings, and a capacious tube for the easy passage of the fragments, and possessing at the same time the indispensable property of preventing in every case the laceration of the urethra, whilst the surgeon is invested with absolute power over the fragments.

Such are the properties possessed by the *evacuating catheter*, which constitutes the subject of this paper. This instrument is composed of two different parts, the tube through which the fragments are expelled, and that part which serves to comminute those pieces of stone which are too large to pass through the tube.

The first is simply a steel catheter, varying in size according to the capacity of the urethra, it being a point exactly to fill that canal. It presents nearly the same curve as the common sound, and may be distinguished into a vesical and extra-vesical extremity. At the vesical extremity is a sort of cap, about *five* or *six* lines long. It is screwed to the body of the catheter, and forms its term; at an inch from the end are placed two large oval apertures, or eyes with smooth round edges. These openings are exactly opposite each other.

The extra-vesical extremity is quite straight. On this is fixed a stopcock, communicating with which is a cavity lined with cork, for the purpose of receiving the pipe of the syringe with which the bladder is injected. Opposed to the stopcock is a ring for the more secure holding of the instrument.

The other part of the instrument which I have called stylet brisè, or pointed stylet, is a solid piece of steel, presenting also a vesical and extra-vesical extremity. The former is composed of a succession of pieces of steel, so hinged together as to combine the properties of flexibility and strength. It also forms a definite curve, corresponding to the curve of the canula. At the extra-vesical extremity there is a large flattened portion, which may be grasped readily and firmly.

It may easily be conceived, that when this instrument is passed into the bladder, and an injection of water made, all the small particles are washed away by the returning stream, but fragments three or four lines in diameter less frequently pass through the catheter, and generally lodge in its vesical apertures, which, being of considerable size, and placed precisely opposite each other, are well adapted to entangle these large fragments. This circumstance, which was embarrassing when occurring during the employment of the instruments first invented, may be considered favourable, now that we possess the means of turning it to advantage.

When a fragment of considerable dimensions has fixed itself in the eyes of the catheter, and the flexible stylet just described is introduced, it is prevented from reaching the end of the tube by the fragment, which is broken by an impulse communicated to the stylet. The resulting particles are either repelled into the bladder or carried into that part of the cavity of the catheter which is situated beyond the eyes, a part about an inch in length, and which has been named "magazin." This process of catching and reducing the larger fragments may be carried on until the whole of the "magazin" is filled.

When this is done, the instrument is withdrawn, the top unscrewed, and the *debris* set at liberty. If necessary, the catheter may again be conveniently introduced. These fragments, which may have fallen from the eyes into the bladder, will easily be washed away by the injected water.

There is nothing to be feared, even if the canula with a piece entangled be drawn into the urethra, for the fragments would be immediately broken and the tube withdrawn, leaving the fractured particles in the urethral canal to be washed away with the first discharge of urine. This circumstance of a fragment being drawn into the urethra, certainly the most disadvantageous that can occur, is unattended with danger, and can indeed hardly ever happen, because the "eyes" being opposite to each other, the arrest of the fragment is indicated to the surgeon by a stoppage in the stream of water the moment these apertures are drawn to the neck of the bladder, and as it is at this spot the fragments enter the holes, it is never necessary to retract the instrument further at the urethra, and the possibility of laceration is avoided.

Thus the evacuating catheter is not only adapted to bring away the lapidous fragments, but also to their comminution, a property which entitles it to the appellation of lithotriptic.

The instrument may be straight or curved, according to circumstances; for there are cases in which sometimes one form, and sometimes the other, is objectionable. When the straight tube can be employed, it affords the surgeon the advantage of turning it on its own axis, and thus varying the position of the eyes, and directing them to different parts of the bladder. This advantage is important, and I have observed in general, that a larger quantity of detritus is more quickly obtained with this form when it is applicable; but it is more irritating to the patient, and more difficultly managed.

I have successfully employed the evacuating catheter where the patients were incapable of voiding a single fragment, and would consequently have derived no benefit from having undergone the lithotriptic process. This is the instrument, with the aid of which Admiral C——, and Mr. O——, both of whom had enlarged prostate, were delivered of the detritus. It was employed in the case of Mr. Messenger, an old man of 72, who, although his urethra was capacious, could not evacuate a single fragment, either on account of a fungous state of the canal, or from the want of contractile power in the bladder. The application of this means was found indispensable in the case of Mr. S——, whose urethra was compressed by a scrotal hernia, and equally in that of Mr. K——, of Liverpool, in whose bladder there existed, opposite the neck, a tumour, which I believe arose from an enlargement of the third lobe of the prostate.

It was with the evacuating catheter alone I was enabled to rid Mr. G—— of his fragments, for in his case the contractions of the sides of the organ did not take place synchronously. In the case of Mr. Archer, the neck of the bladder was surrounded by varicose veins, and of course no force of stream could be produced. Indeed so successful have been the applications of this instrument, that I do not know whether it would not be advantageous to employ it even in cases where the patient has naturally the power of expelling the powder.

I have made use of this instrument before several of the most eminent medical men in London, and amongst others, Sir A. Cooper, Sir A. Carlisle, Mr. Brodie, Mr. Green, Mr. Travers, Mr. A. White, Mr. Aston Key, &c.

If the members of the Academie are of opinion that the "sonde evacuatrice" brings within the limits of lithotrity the numerous cases in which, besides the stone, there exist either natural or acquired obstacles to the free expulsion of the urine, and consequently to that of the fragments, these gentlemen will allow, that, as the inventor of the instrument, I am authorised to present myself as a competitor for the Montlyon prize. Consonant with the expressed wish of the Academy,—1st, It is totally different from the evacuating catheters hitherto employed, both as regards its manner of acting, its construction, and, above all, in the importance of the ends which it enables us to attain; and, 2nd, It is a progressive step towards perfection, by extending the sphere of lithotriptic utility.

Postscript.—It cannot be expected that complete success should attend the first applications of a new instrument. It is by meeting with difficulties that the inventor is enabled to improve upon his first design. This is the light in which we should view the present subject. The patient in whose case Baron Heurteloup employed the evacuating catheter with so unfortunate a result, was an instance in point, and may be associated in this view with another unfortunate case of recent occurrence. The patient had a very large stone in the bladder, and was operated upon by the new instrument acting by percussion. The "*percuteur*" was in a much less perfect state then than it is now. At that time only three or four of these instruments had been constructed, but now upwards of *sixty* have been made, and each one has in some respect improved upon its predecessor.

In the case to which I allude, the two beaks of the instrument which seized the stone, instead of coming close together, gave way, and remained about three lines apart, which prevented the instrument being entirely withdrawn from the urethra. Baron Heurteloup, rather than employ force to extract it, thought it safer to make a small incision in the urethra, such as are frequently made when small calculi are lodged there, in order to pass out the extremity of the percuteur, and unite the expanded branches by means of a pair of pincers. This he easily executed, and the instrument was safely withdrawn. The Baron, considering the advanced age of the patient and aware that the stone was broken in pieces, and that the incision into the urethra, though trifling, would render difficult to the surgeon and painful to the patient the manœuvres necessary for removing the fragments from the neck of the bladder, should they become entangled there, recommended the immediate performance of lithotomy. The operation was performed by Mr. Brodie, and the fragments extracted with more facility than the entire stone, which was six inches in circumference, could have been.

It is this circumstance which has given rise to the false report *of an instrument having been broken in the bladder, of lithotomy being performed to extract it, and of the patient dying immediately after the operation*, whilst, in truth, lithotomy was performed, not because what had been previously done rendered it absolutely necessary, but because it was deemed more advantageous to the patient to operate immediately, than to wait for the healing up of the small incision. The patient did not die immediately, but lived *nine* weeks after the operation, and would probably have recovered, were it not for his great age and feebleness, for the wound nearly healed, and the urine flowed freely through the urethra.

June, 1832. D. O. E.

CASES AND REMARKS
ILLUSTRATING THE EFFICACY OF
ALKALINE SALTS
IN ALLAYING
GASTRIC IRRITATION.

To the Editor of THE LANCET.

SIR,—Should you conceive the following illustrations of the beneficial use of certain *alkaline salts* will, at the present juncture, be interesting to your readers, I beg to place them at your disposal. The following sketch of three cases that have occurred in my practice within the last week, are only so far detailed as to establish the fact I have in view. Your most obedient servant,

PAUL SLADE KNIGHT.

Connaught Terrace, May 29th, 1832.

CASE I.

The first case was developed in a hale lady, remarkable for health and activity, aged 84. She was attacked shortly after dinner, on the evening of the 15th inst., first with faintness, and then with sickness, an excruciating pain and spasm of the stomach, and copious vomiting of all the ingestia, and then of a semi-transparent fluid, (serum and mucus mixed?) so as to excite the astonishment of herself and the bystanders, as they could not possibly conceive whence it could all come. The bowels were actively moved, but not with much pain, nor could I learn anything satisfactory as to the quality. I found her in bed; pulse weak, but sufficiently distinct, and about 80; this was two hours after the attack. She had taken some brandy and water, but it immediately returned. She had cramp in the legs, which yielded to friction with a warm hand, but her chief complaint was of a general and intolerable uneasiness, with extreme thirst, probably from the more fluid parts of the blood being greatly exhausted, for the spasm of the stomach and the vomiting were at the moment subsided, though her feelings led her to apprehend a return. I immediately gave her the following draught and pill.

℞ *Carbon. sodæ* ʒj;
Tinct. card. comp. ʒi;
Aqua ʒvi, *M. f. haust.*

℞ *Opii pur.* gr. j;
Subm. hydr. gr. iij;
Conf. rosæ q. s. ft. pil.

And to take *carbon. sodæ* ʒj, *carbon. am.* gr. iv, every two, three, or four hours, according as she should be awake, or affected with sickness or spasm.

16. She passed a good night on the whole; neither the sickness nor spasms had returned, and the thirst gradually abated; and with the aid of a mild aperient of pulv. rhæi she recovered.

CASE II.

On the 18th I was requested to see the lady's maid in the same house, a young, delicate, and ailing woman, about eighteen years old. On entering the street door I heard her piteous moans, although she was in bed in the attic. I found her suffering from extreme pain and spasms of the stomach, and in the right side, and cramps of the lower extremities, with a small pulse at 120; countenance very much sunken. When she had first complained of being ill, two grains of ant. tart. were given to her; and afterwards, when spasm of the stomach came on, the tin vessel, used on such occasions, was applied unremittingly, filled with hot water. She had vomited a large quantity of fluid, which I was told contained bile, as also did the stools, one of which was remaining, and which was of the usual cholera character, being in quantity from a quart to three pints, of semi-transparent fluid serum, with a large quantity of opaque white flakes at the bottom, of boiled rice and water appearance. I believe there was no bile whatever, nor were there any fæces. To take immediately.

Tinct. opii ʒss;
Lavend. f. ʒi;
Sodæ carb. ʒi;
Aq. menth. p. q. f. ℥iss. *M. ft. haust.*

℞ *Sodæ carb.* ℈j;
Pulv. arom. gr. iv. *M. ft. pulv.*
2da. q. q. hora sumend.

She retained the draught, and the spasm left her in half an hour, and never returned. She continued the alkaline salt three days, when all cholera symptoms having remained absent, other urgent ailments were attended to.

CASE III.

On the 20th instant, at four in the morning, I was called to Mrs. ——, aged about 50, a strong active woman. She had been attacked at eight o'clock the preceding night with violent cramp, pains in the stomach, and vomiting, to which, in a much less degree, she had been subject, and for which she took, during the night, seventy drops of tincture of opium, and hot brandy and water, neither of which afforded relief. I found her in bed on her left side, very much bent, and her head nearly resting on her bosom, shivering with cold, though hot bottles and hot flannels had been applied to her stomach, and her limbs had been rubbed; her countenance was very much sunken, and, as well as her hands, were of a pale, dirty, or dark hue, but not blue; her pulse was very weak, though sufficiently distinct to be counted at about 80; tongue pale and flabby, and she complained much of its sticking to her mouth, but it did not occur to me to ascertain its temperature; that of the limbs was icy cold, and they were also moist, much colder than I have witnessed in some fatal cholera cases. She spoke in a whisper, although, if she exerted herself, she could speak louder and more distinctly, yet her aspect was healthy. She had vomited an immense quantity of fluid, so as to astonish her and the attendant, about three pints of which remained in the basin, and exactly resembled that of No. 1. The bowels had been only moved once, though great pain had been experienced in them. I wished to place her more comfortably in bed, but she declined, dreading that any motion would bring on the cramps.

Treatment.—Hot bottles to the stomach, and friction with the hottest flannels to the limbs.

℞ *Sodæ carb.* ʒj;
Pulv. arom. gr. iv, *statim sumend.*

This remained, and in half an hour I repeated the dose; and in another half hour I had the satisfaction to see that reaction had commenced. I then left her with directions to take a similar powder every hour. About two hours after I had left her, finding she remained free from sickness, she took some tea, which she soon brought up; after that she refrained, and confined herself to water, as I had directed, and she never had a return of vomiting, or of spasm, or of pain of the stomach or side.

At two a.m., I found her on her right side, free from all pain, looking cheerful; eyes bright and face flushed; pulse 80, and firm; body and limbs warm; and she complained of heat in the spinal canal. Twenty ounces of blood were taken with four cupping-glasses placed at equal distances on the vertebral column, the blood was florid, and firm, and inflamed, exhibiting very little serum.

℞ *Sodæ carbon.;*
Potassæ carb. a a. ʒiss;
Spt. æth. f. ℥ss:
Aq. menth. p. hss. M. ft. cap. cochl. j,
ampl. 3â q. q. horâ.

On examining the state of the blood, however, I directed the mixture to be taken only two or three times a day, and thus the cure ended in convalescence; and in two days she walked round one of the squares; but says she has an indescribably unpleasant feeling, and that *her veins look blacker than usual.*

Here, Sir, we have three cases, all attended by some of the most serious precursory symptoms of cholera asphyxia, all suffering excruciating pains from cramps in the stomach and limbs, accompanied by violent vomiting, and a great discharge of fluids, &c., and one case fast verging to a state of absolute collapse, and all *instantly* relieved by ʒi doses of carb. sodæ, and by the same medicine, restored from all the cholera symptoms. The same thing has occurred in the practice of some of my medical friends in this neighbourhood, to whom, as a member of the Paddington Board of Health, I took the liberty to explain my views some time last winter; the pages too, Sir, of your Journal, bear ample evidence to the same fact—that is, the extraordinary power of the carbon. sodæ to allay *gastric irritation,* in which, I believe there is sufficient evidence to show, chiefly consists the main secret of curing this, as well as many other diseases of a febrile and spasmodic character, as I have, in a former communication, explained. Who but must attribute the success of the practice in the Cold-bath Fields prison to this principle, affected wholly by the alkaline salts there used, and I doubt not that ʒi doses of the carbon. sodæ given every one, two, three, or four hours, according to the urgency of the symptoms, with such additions of stimulants or opiates as the peculiarity of the case might indicate, would have answered fully as well—much better than the formula used. We are, I fear, Sir, too little acquainted with the chemical changes effected in the stomach of man, under the million of circumstances that influence its actions, to justify us in adopting theoretical views, involving nice and ill-understood distinctions. An acid of a peculiarly virulent quality seems the basis of all the mischief in cholera asphyxia, as I had surmised in a published letter this time last year, and ʒi doses of the carbonate of soda, is its appropriate and speedy remedy, as I also stated in the same letter. This method of cure has the advantage of the greatest simplicity, and may be safely trusted in the hands of any intelligent person, to be used the instant any spasm of the stomach comes on, and repeated every hour till the attendance of a medical person can be obtained, which, of course, in a case of this nature, will be with the least possible delay. Is it too much now to say that thousands would have been rescued from a premature death and horrible tortures, had the views which I published in *The Times* paper of June 21, 1831, been duly appreciated?

(See note at the end of this Number.)

THE SULPHATE OF INDIGO

AS A TEST FOR

NITRIC ACID.

To the Editor of THE LANCET.

SIR,—Having lately gone over a number of experiments with the sulphate of indigo, whose claim to be considered a test for nitric acid has latterly become a disputed point, perhaps you may think a brief statement of the results obtained, will perhaps tend to set the question at rest, and therefore not unworthy of occupying a column in your valuable pages.

In THE LANCET, of the 24th of March last, is an excellent paper on this subject by Mr. J. S. Hiley. With most of the observations made by that gentleman I entirely concur, and particularly can I bear testimony to the justness of his strictures on the experiments of Dr. O'Shaughnessy; but, in the meantime, he himself has fallen into some slight error. He states, as the result of an experiment, a solitary one I suspect, that hydrochloric acid does not completely discharge the blue tint of the sulphate of indigo. This is certainly a mistake, since, in repeated trials, I invariably found that the liquid was decolorised by that acid quite as effectually as by the nitric.

Having subjected the solution of indigo to the action of nearly every acid known to chemists, I can state with tolerable certainty, that three only have the power of dispelling the blue tint; these are, the nitric, the hydrochloric, and the chloric; and that but two classes of salts possess it, viz. the nitrates and the chlorates. The addition of a drop of sulphuric acid, which Dr. Liebig recommends to be made in every experiment, but which Mr. Hiley considers in every case superfluous, I have found to be useless when operating with the uncombined acids, but a necessary part of the process in all cases of testing salts. Since muriatic acid and the chlorates may readily enough be detected by appropriate tests, it follows that Dr. Liebig's process has still claims to be ranked among the best of those for the detection of nitric acid and its salts. It is true that several deoxydising agents, as the protoxide of iron, the protomuriate of tin, sulphuretted hydrogen, and chlorine, destroy the blue colour, but all these, not even chlorine excepted, leave the solution of a yellow, or greenish yellow colour, whereas nitric acid and the nitrates either render it completely colourless, or of a very faint straw hue. The best method of conducting the process is to pour a little of the suspected liquid into a small test-tube, then to tinge it *perceptibly blue* with the indigo solution, and having added three or four drops of sulphuric acid, to hold the tube over the flame of a spirit-lamp until ebullition has commenced, when the colour will have vanished, if nitric acid, or any of its salts, be present. I remain, Sir, your obedient servant,

R. W. THACKER.*

Elford, May 27, 1832.

DR. O'SHAUGHNESSY'S

REMARKS ON THE PRECEDING LETTER.

To the Editor of THE LANCET.

SIR,—I have to acknowledge your kindness in permitting me to peruse the preceding letter previous to its publication, and I am further indebted to its author for recalling my attention to Mr. Hiley's paper in THE LANCET of March 24.

I believe I shall have little difficulty in showing, that on the chemical facts alluded to by both writers, the most perfect harmony exists between these gentlemen and myself. As to the medico-legal deductions from these facts, I believe that I can demonstrate that my opinions on the utter inapplicability of the sulphate-of-indigo test to *toxicological* chemistry, remain completely unrefuted.

In my first paper on nitric acid, published in your Journal on May 29th, 1830, I communicated the fact that I had discovered and demonstrated to my pupils in Edinburgh, that muriatic and sulphuric acid destroyed the colour of the fluid tinted slightly with sulphate of indigo, and that a mixture of sulphuric acid and muriate of soda produced the same effect. I described the mode in which the experiments were conducted. They were witnessed by numerous pupils who can bear me out in my statements. From these facts I concluded that, individually, the sulphate of indigo could no longer be regarded as a sufficient test for dilute nitric acid even in abstract chemical inquiries, while in *toxicological* chemistry it should be at once discarded, owing to the natural existence of *muriatic* acid in the contents of the intestinal canal.

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* Several letters on the subject which has occupied the attention of Mr. Thacker, Mr. Hiley, and Dr. O'Shaughnessy, have reached us within the last few weeks. We select that of Mr. Thacker for publication. but in order to save continuous reference to the question in dispute, we have forwarded it, before insertion, to Dr. O'Shaughnessy, out of whose paper, published in THE LANCET, May, 1830, the discussion has arisen, and now append the letter we have received in reply from him, by this means closing the argument in one Number.—ED. L.

In the interval which elapsed between the transmission and publication of my first paper, I pursued the inquiry still further. My stock of mineral acid having been accidentally exhausted, a new supply was obtained from a different establishment, before the second set of experiments were commenced. To my surprise the new sulphuric acid no longer decolorised the fluid, while the muriatic acid was increased in power. Following up this hint, I found that different specimens of acids acted so variously on the sulphate-of-indigo test, that one might be a perfect bleacher of the fluid, and a second be altogether destitute of this action. I further found that the chlorates, per-muriate of iron, proto-muriate of tin, sulphuretted hydrogen, and many other substances, discharged the colour of the reputed test for nitric acid, and the majority under parallel circumstances.

These results I published in a second paper which appeared in THE LANCET on the 19th of June, 1830. This paper appears to have escaped the notice both of Mr. Hiley and Mr. Thacker, whose experiments I may now illustrate by its assistance.

Mr. Hiley, on repeating the experiments performed before my class, described in my first paper, found that the sulphuric acid did not bleach the fluid, but that with the muriatic acid it " lost a little of its tint." Mr. Thacker, again, questions Mr. Hiley's correctness relative to the muriatic acid, and states " that in repeated trials he invariably found that the liquid was decolorised by muriatic acid quite as effectually as by the nitric." The truth is, that both gentlemen observed correctly. Different specimens of the same kind of acid possess different degrees of decolorising power, as I have stated in my second paper. The sulphuric acid, too, is that which most rarely bleaches the liquid. Of 15 specimens in my possession only two succeed, and these are impregnated slightly with sulphurous acid. Of 15 specimens of muriatic acid, eleven bleach the solution. One affects the colour slightly, and three are inert. On the cause of this difference I have not had leisure to make inquiries, and I will consequently offer no speculations about it.

We can now understand why the three observers differed so materially in their statements. I have also a word to offer on the medico-legal inferences which the subject suggests.

It is sufficiently proved by my experiments, confirmed by those of Mr. Thacker, that *muriatic* acid bleaches the test. Now muriatic acid existing in the stomach in the free state, and often in considerable quantities, could the medico-legal chemist presume to swear that he had detected nitric acid, if the indigo test had been bleached by the contents of the stomach? The answer is obvious. The test is therefore utterly useless in the medico-legal examination of any mixture having an acid reaction, and containing muriatic acid, or any of its compounds. Again, as to the applicability of the sulphate of indigo to the examination of the pure or diluted acid, we have so many preferable reagents, that it is absolutely not worth notice, while, for the nitrates, the morphia which I first pointed out, affords us decisive, simple, and infallible evidence.

Both the gentlemen to my surprise speak of " the difference of effect in point of time," and the " difference of tint between a yellow or greenish yellow, and a straw colour," being sufficient distinction between the effects of nitric acid, muriatic acid, the chlorates, &c. It is evident that these gentlemen forgot that, in the practice of medico-legal chemistry, we have usually to deal with turbid and discoloured fluids, in which these tints could not be distinguished. Even were the fluids for analyses sufficiently devoid of alien colour, I for one would not presume to give positive evidence on the strength of a shade of colour.

So much for the sulphate of indigo. If the subject really deserved debate, I might extend the argument much further. I shall only add that Dr. Christison, in his second edition, just published, fully admits the validity of the several positions I have advanced, and emphatically states that Liebig's test must be " abandoned altogether." —Treatise on Poisons, 2nd Ed. p. 145.

With respect to Mr. Hiley's remarks on two processes suggested in my first paper for detecting nitric acid—viz. one by the formation of the detonating cyanate, and the second by the formation of the nitrate of urea—I can only say that I fully agree with him in believing that neither deserves the least confidence, or is at all applicable to *medico-legal* analysis. They are both fallacious and difficult of manipulation. I cannot, however, give Mr. Hiley the merit of pointing out their defects, as they were pretty severely handled by myself in one of the commentaries to which Mr. Hiley alludes, and in which the comparative merits of the several processes for detecting nitric acid were freely considered.

I am, Sir, your obedient servant,

W. B. O'SHAUGHNESSY, M.D.

June 1, 1832.

GANGRENE OF THE MOUTH

FROM MERCURY.

To the Editor of The Lancet.

Sir,—There is a very interesting case of gangrene of the mouth, produced by the violent action of mercury, related in the last Number of The Lancet. This is an accident which will sometimes arise, even when the medicine has not been exhibited in excessive doses. In some persons a small quantity of this metal will produce almost immediate and most violent effects, but we are not in possession of a knowledge of the peculiarity of constitution upon which this susceptibility to its action depends. Your correspondent states that there is scarcely a case of gangrene in children, arising from this cause, to be found on record. At page 295 of my Compendium of Medical Practice, he will find a case, and the cause adverted to in the following terms:—"I have one remark to add with respect to the use of mercury. Children will take an immense quantity of it, without the salivary glands becoming affected; we ought not, however, on this account to give it incautiously, for it will sometimes happen that without salivation the parotid duct will be excited by it into violent inflammatory action, in which the parotid gland and the whole cheek will be speedily involved, and sphacelus, or a rapid ulceration, will be the consequence. I have seen both cheeks entirely removed by this process. Nothing with which I am acquainted has the slightest tendency to check its progress."

In this case very minute doses of hydr. c. cretâ (not more than a grain) in combination with bark, had been prescribed and taken twice a day for three weeks, when the child complained of soreness of the gums, and ulceration upon the inside of the cheeks. The medicine was immediately laid aside, but, on the following day, the ulceration had spread over the whole of the membrane lining the inside of the cheeks, and two small livid spots were to be distinguished externally directly over the terminations of the parotid ducts. These spots rapidly extended, and in seven days the whole of the integuments, and muscles of both the upper and under jaw, sloughed away, presenting the most ghastly and pitiable object I ever beheld. The patient was nine years of age.

Yours, very respectfully,
E. Bedingfield.

Medical School, Stowmarket,
May 1, 1832.

JUVENILE APPOINTMENTS TO THE

DUBLIN CHOLERA HOSPITAL.

To the Editor of The Lancet.

Sir,—Through the medium of your very valuable Journal, may I beg to take notice of a species of injustice, (not uncommon in the present day,) practised towards their junior brethren, by some who are, or imagine themselves, at the head of the profession?

I refer, Sir, to what has occurred in the appointments that have recently taken place for assistants to one of the Cholera Hospitals in Dublin. Immediately on the appearance of the disease in this city, a very extensive hospital was prepared, and it being necessary that medical men should be appointed who might be constantly on the spot, the surgeons of Stephen's and the Richmond hospital, having, by what right it is not easy to say, the disposal of these situations, immediately appointed a number of their own apprentices. The consequence is, that young men, some of them mere boys, not more than two or three years at the profession, are intrusted with the care and treatment of cholera patients, whilst others may be seen driving about the city and its suburbs to determine whether cases are cholera or not. Now, Sir, what I would complain of is, that whilst there are many licentiates, and even members of the college, who would have been glad to have held the situations, professional men are to be found who should so far forget the interests of the college, and of the profession at large, as, through selfish and interested motives, to place their own apprentices over the heads of regular and qualified surgeons. It may be said that those persons were appointed because others more eligible did not come forward; the fact is, that those situations, though refused by some of the pupils, were not offered to the licentiates of the college, nor was it known that there were to be such, until after the appointment had taken place, though I must here add, that there are one or two of these situations held by gentlemen who are in every way qualified to undertake them. I should be glad to ask those gentlemen who have acted in this manner, what is the use of having a degree from a college, where we see the very persons who are appointed to give that degree, declaring to the public by their acts the worthlessness of their diploma, and that whenever situations are to be held which require the practical surgeon, or to which emoluments may be attached, their own apprentices are in every way as capable and as well qualified to hold them, as the possessors of diplomas, which have cost them so much labour and expense to obtain, and from which they are led to expect some profit.

It is a fact well known, that at no period has the Dublin College of Surgeons been in a less thriving state than at present; as a proof of this, I will merely mention the fact, that there have not been more than three or four applications for examinations since the commencement of the year; this, Sir, together with the want of that support which the senior members of the profession should grant to the juniors, arises in a great measure from a deficiency of that *esprit du corps*, which in no profession is more remarkable than in the medical.

In conclusion, Sir, I might hope, that in these days of reform, the medical profession will also undergo a renovation, and that it will first commence, where it is most wanting, in the minds of its members, and that they will lay aside all underhand kind of work, and that desire of raising themselves to the injury and injustice of their brethren.

By giving the above a place in the pages of The Lancet, you will oblige,

Your very humble Servant,

A Member of the Profession.

Dublin, May 21, 1832.

PUNCTURED WOUND
BY A
BAYONET.

To the Editor of The Lancet.

Sir,—I shall feel obliged by your giving an early insertion to the following case of punctured wound, terminating in death in ten hours, in your valuable weekly journal. I am, Sir, your obedient servant,

James Mash, House-Surg.
General Infirmary, Northampton.

William Marriott, ætat. 35, received a wound from a bayonet on the right temple on Saturday evening, March 17th ult., about half-past nine o'clock, and was brought to the infirmary two hours after the accident. On examination I found a punctured wound of a triangular shape on the right temple, extending through the temporal bone to the brain. He was quite insensible, and the pupil of the right eye considerably dilated, and insensible to the stimulus of light; the pupil of the left eye was more than naturally contracted, and also insensible. The pulse was weak and very slow; extremities cold. Wine, &c., were immediately given, and the pulse became stronger. He lingered in the same state until half-past eight o'clock the following morning, and expired, never having spoken from the commencement.

Post-mortem examination of the body, three hours after death.

On examining the external wound, I found the temporal artery divided, and the squamous portion of the temporal bone perforated, evidently by an instrument of a triangular figure. On opening the head, and opposite the wound, the dura mater and middle lobe of the cerebrum were lacerated, extending into the right lateral ventricle of the brain. On removing the dura mater, considerable venous congestion was observed between the tunica arachnoidea and pia mater, and on opening the right ventricle, considerable extravasation of venous blood was found in that cavity. The other parts of the brain presented no unnatural appearances.

April, 1832.

P.S. I was informed by the surgeon who was first called to the case, that he lost a good deal of blood from the temporal artery, but he had no hemorrhage after he came into the Infirmary, except a little oozing before death from the wound.

APPLICATION OF
GASTRIC JUICE
TO THE PURPOSES OF MEDICINE.

To the Editor of The Lancet.

Sir,—The observations I am about submitting to your consideration, first originated by my perusing in your Journal a short time since some very trite remarks on the gastric juice, and the application of the gastric juice of animals in the cure of certain diseases of the stomach, such diseases being supposed to owe their origin to a scarcity of the necessary juice for the usual digestive process. I well remember your correspondent cited some very satisfactory results having arisen from the employment of the animal juice.

After I had attentively considered the subject, the thought struck me, that as gastric juice was a liquid, which it was difficult if not impossible to procure, some other preparation may in its stead be instituted; but as it was an animal production, I was on the point of giving up the subject, not being possessed of chemical apparatus sufficient to moot the matter to the base, when happening to dine that day with some professional friends, the subject of the formation of cheese was brought on the carpet, as well as on the table; one idea led to another, and after returning to my study, I briefly recapitulated the substance of our conversation. One observation of my friend at the time particularly struck me; he stated, that "a liquid called rennet (which was formed from what are commonly called Irish vells, such articles being nothing more or less than the stomachs of calves salted) was dropt into milk, which immediately from its acidity produced a decomposition of its parts; the thicker was precipitated, commonly called curds, and a thin serous fluid formed on its surface denominated whey; he further stated, that it was a generally-received thing among men of agricultural research, that the gastric juice of the vell which produced the decomposition." Now, Sir, my hypothesis is of a very simple nature; we all know albumen is decomposed by acids, therefore what can produce the decomposition in milk if there be not an acid present? And what can that acid be, but the gastric juice contained within rennet? We all know that gastric juice is acetous in a high degree, which proves at once that it can be no other than the real gastric juice of the calf which constitutes the active effect to be found in rennet. I have not, Sir, since forming the above conclusion had a case under care in which I could employ the rennet, but I do not hesitate a moment in giving my opinion, that in those diseases of the stomach in which there is a deficiency of the gastric juice (which frequently occurs in dyspepsia), the employment of rennet would be of material advantage; at least I speak from supposition. Amongst the ranks of the profession, if any gentleman be found who will give his especial attention to this subject, he will merit the thanks of the lovers of science I am well assured. And now, Sir, I must apologise for having so long trespassed on your favour; and having introduced this subject to the attention of the profession, I beg leave to bequeath it to abler and more efficient hands; and believe me, Sir, to remain your obliged humble servant,

G. J. P.

Hants, May 16th, 1832.

ROYAL INSTITUTION.

MR. FARADAY ON THE ELECTRO-TONIC STATE.

WHILST the wire is subject to either volta-electric or magneto-electric induction, it appears to be in a peculiar state, for it resists the formation of an electrical current in it, whereas if it were in its common condition, such a current would be produced; and when left uninfluenced, it has the power of originating a current, a power which the wire does not possess under common circumstances. This electrical condition has not hitherto been recognised, but it probably exerts a very important influence in many, if not most, of the phenomena produced by currents of electricity.

This peculiar condition shows no known electrical effects whilst it continues; nor have I yet been able to discover any peculiar powers exerted or properties possessed by matter whilst retained in this state.

It shows no reaction by attractive or repulsive powers. The various experiments which have been made, with powerful magnets upon such metals as copper, silver, &c. prove this point; for these substances, if electrical conductors, must have acquired this state, and yet no evidence of attractive or repulsive power has been observed. I have placed copper and silver discs, very delicately suspended on torsion balances in vacuo, near to the poles of very powerful magnets, yet have not been able to observe the least attractive or repulsive force.

I have also arranged a fine slip of gold leaf very near to a bar of copper, the two being in metallic contact, by mercury, at their extremities. These have been placed in vacuo, so that metal rods connected with the extremities of the arrangement should pass through the sides of the vessel into the air. I have then moved powerful magnetic poles by this arrangement, in various directions, the metallic circuit on the outside being sometimes completed by wires and sometimes broken. But I never could obtain any sensible motion of the gold leaf, either directed to the magnet or towards the collateral bar of copper, which, as far as induction was concerned, must have been in a similar state to itself.

In some cases it has been supposed, that under such circumstances attractive and repulsive forces have been exhibited. But the phenomena just described, in conjunction with the confidence we may reasonably repose in M. Ampere's theory of magnetism, tend to throw doubt on such cases, and it is far more probable, that the extremely feeble permanent effects observed, have been due to traces of iron, or some other unrecognised cause not magnetic.

This peculiar condition exerts no retarding or accelerating power upon electrical currents passing through metals thus circumstanced. Neither could such power on the inducing current itself be detected, for when masses of metal, wires, helices, &c. were arranged in all possible ways by the side of a wire or helix, carrying a current measured by the galvanometer, not the slightest permanent change in the indication of the instrument could be perceived. Metal in the supposed peculiar state, therefore, conducts electricity in all directions with its ordinary facility, or in other words, its conducting power is not sensibly altered by it.

All metals take on the peculiar state, as is proved by experiments I have shown in a former lecture. With regard to iron, the experiments prove the thorough independence of these phenomena of induction in regard to the ordinary magnetical appearances of that metal.

This state is altogether the effect of the induction exerted, and ceases as soon as the inductive force is removed. It is the same state, whether produced by the collateral passage of voltaic currents, or the formation of a magnet, or the mere approximation of a magnet; and is a strong proof of the identity of the agents concerned in these several operations.

The state appears to be instantly assumed, requiring hardly a sensible portion of time for that purpose. The difference of time between volta-electric and magneto-electric induction, rendered evident by the galvanometer, may probably be thus explained. When a voltaic current is sent through one of two parallel wires, as those of the hollow helix, a current is produced in the other wire, as brief in its continuance as the time required for a single action of this kind, and which by experiment is found to be inappreciably small. The action will seem still more instantaneous, because as there is an accumulation of power in the poles of the battery before contact, the first rush of electricity in the wire of communication is greater than that sustained after the contact is completed; the wire of induction becomes at the moment electro-tonic in an equal degree, which the moment after sinks to the state in which the continuous current can sustain it, and, in sinking, causes an opposite induced current to that at first produced. The consequence is, that the first wave of electricity more resembles that from the discharge of an electric jar than it otherwise would do.

But when the iron cylinder is put into the same helix, previous to the connexion being made with the battery, then the current from the latter may be considered as active in inducing innumerable currents of a similar kind to itself in the iron, rendering it a magnet. This is known by experiment to occupy time, for a magnet so formed, even of soft iron, does not rise even to its fullest intensity in an instant, and it may be because the currents within the iron are successive in their formation or arrangement. But as the magnet can induce, as well as the battery current, the combined action of the two continues to evolve induced electricity until their joint effect is a maximum, and thus the existence of the deflecting force is prolonged sufficiently to overcome the inertia of the galvanometer needle.

In all these cases where the helices or wires are advanced towards or taken from the magnet, the direct or inverted current of induced electricity continues for the time occupied in the advance or recession; for the electro-tonic state is rising to a higher or falling to a lower degree during that time, and the change is accompanied by its corresponding evolution of electricity; but these form no objections to the opinion that the electro-tonic state is instantly assumed.

This peculiar state appears to be a state of tension, and may be considered as equivalent to a current of electricity. The current evolved however, first or last, is not to be considered a measure of the degree of tension to which the electro-tonic state has risen; for as the metal retains its conducting powers unimpaired, and as the electricity evolved is but for a moment, the electricity which may be led away by long wire conductors (offering obstruction in their substance proportionate to their small lateral and extensive linear dimensions) can be but a very small portion of that really evolved within the mass at the moment it assumes this condition.

The tension of this state may be comparatively very great. But whether great or small, it is hardly conceivable that it should exist without exerting a reaction upon the original inducing current, and producing equilibrium of some kind. I have not been able to ascertain that this gives rise to a retardation of the original current, neither have I in any other way as yet been able to distinguish effects attributable to such a reaction.

All the results favour the notion, that the electro tonic state relates to the particles, not to the mass, of the wire or substance under induction, being in that respect different to the induction exerted by electricity of tension. The current of electricity which induces the electro-tonic state in a neighbouring wire, probably induces that state also in its own wire; for when by a current in one wire a collateral wire is made electro-tonic, the latter state is not rendered any way incompatible, or interfering, with a current of electricity passing through it. If, therefore, the current were sent through the second wire instead of the first, it does not seem probable that its inducing action upon the second would be less, but on the contrary more, because the distance between the agent and the matter acted upon would be greatly lessened.

No difficulty arises in considering the wire rendered electro-tonic by its own current, more than by any external current, especially when the apparent non-interference of that state with currents is considered. The simultaneous existence of the conducting and electro-tonic states, finds, an analogy in the manner in which electrical currents can be passed through magnets where it is found, that both the currents passed, and those of the magnets, preserve all their properties distinct from each other, and exert their mutual actions.

The reason given with regard, to metals, extends also to fluids and all other conductors, and leads to the conclusion that when electric currents are passed through them, they also assume the electro-tonic state. Should that prove to be the case, its influence in voltaic decomposition, and the transference of the elements to the poles, can hardly be doubted. In the electro-tonic state the homogeneous particles of matter appear to have assumed a regular but forced electrical arrangement in the direction of the current, which, if the matter be undecomposable, produces, when relieved, a return current; but in decomposable matter, this forced state may be sufficient to make an elementary particle leave its companion, and associate with the neighbouring similar particle in relation to which it is in a more natural condition, the forced electrical arrangement being itself discharged, or relieved, at the same time, as effectually as if it had been freed from induction. But as the original voltaic current is continued, the electro-tonic state may be instantly renewed, producing the forced arrangement of the compound particles, to be as instantly discharged by a transference of the elementary particles of the opposite kind in the opposite directions, but parallel to the currents. Marianini has discovered and described a peculiar affection of the surfaces of metallic discs when being in contact with humid conductors; a current of electricity is passed through them; they are then capable of producing a reverse current of electricity, and Marianini has well applied the effect in explanation of the phenomena of Ritter's piles. M. A. de la Rive has described a peculiar property acquired by metallic conductors, when, being immersed in liquid as poles, they have completed for some time the voltaic circuit, in consequence of which, when separated from the battery and plunged in the same fluid, they themselves produce an electric current. M. A. Van Beek has detailed cases in which the electrical relation of one metal in contact with another has been preserved after separation, and accompanied by its corresponding chemical effects. These states and results appear to differ from the electro-tonic state and its phenomena; but the true relation of the former to the latter can only be decided when our knowledge of all their phenomena has been enlarged.

The momentary existence of the phenomena of induction now described, is sufficient to furnish abundant reasons for the uncertainty or failure of the experiments hitherto made to obtain electricity from magnets, or to effect chemical decomposition or arrangement by these means. It also appears capable of explaining fully the remarkable phenomena observed by M. Arago between metals and magnets when either are moving, as well as most of the results obtained by Sir John Herschel, Messrs. Babbage, Harris, and others, in repeating his experiments.

THE LANCET.

London, Saturday, June 9th, 1832.

In turning attention from the Asiatic cholera to the tyranny of Rhubarb Hall, we merely direct our view from one plague to another—the first, the dread of the world; the second, the curse of English medical students. An active temperament proves a bane instead of a blessing, if it be not directed by sound judgment; thus little-minded men, who have scarcely aught to distinguish them other than expertness of movement, inflict tortures upon all persons who may happen to fall within the sphere of their influence; and the mischief worked by them is in exact proportion to the degree of their elevation in the scale of society. Hitherto the framers of the laws of this country have exercised little or no discretion in the selection of individuals upon whom has been made to devolve the execution of important trusts. Imperfect in the first choice, the original defectiveness has been ever rendered still more defective by allowing deformity to make choice of deformity, until a monster has been called forth too hideous for human endurance. The parent evil has now, however, received a shock which must be speedily communicated to every member of her corrupt offspring. The "Colleges" and "Companies" established under the reign of misrule, have received in their "corner-stones" a blow which has shaken the rotten fabrics to their centres. Where are now the revilers of the LONDON COLLEGE OF MEDICINE? Where are the dim-sighted mortals who predicted the failure of that institution in the failure of the measure for *parliamentary* regeneration? Under any circumstances, in a nation manifesting so much intelligence as the people of England, nothing could prevent the final and permanent success of such an establishment as the London College of Medicine—owing its origin, as it does, to the purest motives, and based, as it is, upon the noble principle of science, and the just one of public utility. But now that we are to enjoy, comparatively speaking, an *independent* legislature, the *legal* establishment of this institution—by act of parliament or by charter—is as inevitable as it must be speedy. The cry of "no innovation," set up by knaves, fools, and bigots, has been exchanged for—"March on in the road of improvement." The senators and philosophers of the present period, who may be directing their researches into the dark channels of medical history, and who institute a comparison between the atrocities which have been perpetrated by the charter-cemented medical colleges and companies, and the great and glorious advantages which must result from such a really liberal institution as the London College of Medicine, resemble a man making the irrational attempt to harmonise the justice and independence of trial by jury, with the cruel barbarities enacted by the Star-Chamber. We are induced to advert to the savage tyranny manifested by the old medical corporations, because it has of late increased not only in the intensity of its malignity, but in the frequency of its manifestations.

The members of our profession do not now require to be told that there is an *Apothecaries' Act*, which was passed in 1815, nor do they require to be informed that, under the provisions of that act, the Judges have decided that neither Surgeons nor Physicians can prepare or administer their own medicines, in cases which are considered "*medical*;" but it is *not* generally known, that under the sanction of that iniquitous measure, medical students are plundered at the various "recognised" establishments, and that they are insulted, harassed, and degraded by the very individuals whom the legislature intended should be at once their guides and protectors. Never have we hesitated to condemn, in the strongest possible terms, all *private* examinations into alleged professional ability. Never have we omitted to hold up such inquisitions, as repressive of professional ardour, as repulsive to professional honour, as injurious to the public welfare. If, however, our voice has been heard, it has been heard in vain, as regards not only the actual state of the law, but as relates to the system still in vogue at the two chartered "Colleges," and the Act-of-Parliament Medical "Company" still in existence in this metropolis. But in a demand upon the energies and discernment of the profession for the creation of a new institution, claiming support by the just and scientific character of its regulations, we have *not* spoken in vain—our breath has not been "wasted upon the desert air." Infatuated by the bedizening influence of office, or bewildered by an overwhelming feeling of self-conceit, the very men who contemplate the existence of the London College of Medicine with apprehension and dismay, labour zealously in the course of its success; but, it must be admitted, with no good intentions. Boasting of the power which the law has conferred upon them, they exercise that power in a manner the most sure for effecting their own overthrow. Detesting an institution which opens wide its portals to talent of every nation and every clime, they labour to furnish materials for a comparison which cannot fail to expose them to their professional brethren in all the hideousness of unshielded guilt. Not a week passes but communications are forwarded to us, in which the most bitter complaints are urged against the examiners at Apothecaries' Hall. If any reliance may be placed on the statements of our correspondents, this insignificant knot of contemptible personages seem to delight in distracting minds which they are only empowered by law to patiently and impartially examine. Screened from public view, and engaged in their inquiries with closed doors, under the protection of a beadle's mace, they indulge in all the capricious whims peculiar to imbecility, and breathe forth the petulant mandates of pedagogues, when their language and demeanour should indicate a love of science, and the calm spirit of honourable inquiry. Candidates for the license are rejected under the most flimsy pretexts; judicious replies throughout three-fourths of a probationary hour are no security against disgrace for the commission of two or three technical errors in the last few minutes of examination. It is even said, that some of the scrutators pride themselves upon the ingenuity, the delusive tendency of their "*catch*" questions. Thus a candidate is frequently rejected—not for misrepresenting that which is really in existence, but for giving some erroneous definition of a fabrication which holds its seat exclusively in the almost brainless head of his inquirer. Placed within the field of medical science, an idiot might, by directing his attention to a thousand points, ask for information from the wisest man in the profession without obtaining it. An examination to be useful, should be practical; to be efficient, it should embrace what is demonstrable; to be just, it should be impartial. Had the pupils the opportunity of catechizing the Court, how many questions would the *examiners* answer correctly, even if the subjects of them embraced matters of a purely elementary character? But, in turning back candidates upon the meanest quibbles, upon the veriest subterfuges of heartless task-masters, the Court bestows no consideration on the sacrifices of time and money—no feeling of compassion for the unhappy victim of disappointment and disgrace. One thing, however, they doubtless *do* consider—that a temporary refusal to receive the fee for the license does not amount to a permanent loss of that fee, for the Act of Parliament is imperative, and the judges have shown that a practitioner cannot act as an apothecary, if he have not the license of this Company, without subjecting himself to a penalty of 20*l.* for every offence. Hence, the gratification of a narrow-minded or malignant feeling is not attended with a tax upon the pocket. The worthy Court may be prodigal in "rejections" without any attendant risk of pecuniary loss. A fee is recoverable, if the student's peace of mind be not; sacrifice of honour to the candidate brings not the inconvenience of empty coffers. How different would be the proceedings of the worshipful Court if the examinations were conducted *in public*, in conformity with the regulations of the London College of Medicine? Standing in his own right before competent querists, and an impartial jury of medical men, what student possessing the requisites for the successful practice of his profession would hesitate to place himself in the presence of so honourable a tribunal?

Although the powers of the miserable Company are rapidly approaching their end, we have felt ourselves called upon to advert to the cruel character of some of the recent rejections. It is possible that this public notice may check the current of abuse. That it will remove it we cannot venture to hope. If, however, the examiners possess any of those qualities which should characterise the heads and hearts of the members of a liberal and learned profession, we would appeal to them, and inquire whether the display of garrulous inquisitiveness—whether the exhibition of an overbearing, intolerant spirit of seeming superiority—whether wily, deep-laid, and mystically-woven questions to entrap, can serve one good purpose, either as concerns the examiners or the examined; and whether, on the other hand, conduct so disgraceful on the part of the Court does not often displace feelings of respect by those of disgust, and convert the fondest anticipations of youthful minds into the rancorous feelings of disappointment and revenge.

On the Nature and Cure of Glandular Diseases, especially those denominated Cancer, with the mode of Treatment, and on the too frequent use of Mercury, &c. By Sir Charles Aldis, Surgeon and Accoucheur, M.R.C.S. Lond. Highley. 1832; 8vo. pp. 116.

If we analyse the motives which generally induce authors to commit their lucubrations to print, we shall find that a few only are influenced by a pure desire to benefit mankind; a large number are stimulated by the less worthy, though often laudable anxiety, of acquiring notoriety, and posthumous honours. A very large number aim not at utility, and are incapable of estimating even the value of reputation. These are possessed by the spirit so well denominated by Horace as the "cacoethes scribendi." Under a third class are to be arranged advertisers in general, who adopt the ingenious expedient of authorship, and under covert of advancing some public object, or a particular science, by professedly publishing a treatise on it, let the public know what they have to sell, or that they have changed their place of abode, and migrated from this part of the town to that. Amongst the class of works produced by this latter kind, must the treatise of the present knight-accoucheur be ranked.

We need not remind our readers how important a question is that of cancer, and what a responsibility that man takes upon himself who declares in positive terms that he can cure this obstinate and formidable disease without the use of the knife. Nothing but the clearest and most unimpeachable testimony is admissible in support of such an assertion, supported by equally unimpeachable cases. But for such, notwithstanding the declarations of the author, the reader may in vain search the present volume. The first page is garnished with a drawing more worthy of the "Masterpiece" than a medical volume: the preface consists of a treatise on cholera; the body of the work is filled with cases, the majority of which, at the least, were *not* cancerous; we have, then, an episode upon small-pox and Dr. Curry; and, at last, lest the bantling might be classed with the Cerberus breed, we have an appendix (to this work on *cancer*!) in the shape of a case of *fractured skull*, published nine-and-twenty years ago, with one of pericarditis equally stale and inappropriate!

The object of the treatise is palpable enough to any one who may inspect it.

Practical Examinations on the immediate Treatment of the principal Emergencies that occur in Surgery and Midwifery, systematically arranged. Intended to serve as an Exercise for the Student, and a brief Work of Reference for the General Practitioner. Part I. By W. S. Oke, M.D. London: Longman and Co. 1831. 8vo. pp. 250.

We feel particular pleasure in being able to recommend this volume to medical students, for it is a very good work, and one to which they may refer with advantage. It contains much valuable information on the leading doctrines of surgery and midwifery, ably and tersely conveyed. The miserable productions usually consulted by beginners, are rather calculated to impede than to promote medical education. Nearly the whole of them, in fact, are occupied by matters of minor importance, or whenever they do treat of essentials, are filled in so wretched a manner,—their contents are so ill arranged, or their matter is so unintelligibly written, that a general destruction of the mass would be in no respect a loss to the sciences of which they profess to treat, but would prevent students from wasting much valuable time and labour upon points of no practical value. From such objections as these, the work of Dr. Oke is eminently free; the questions follow one another in well-arranged series, and the answers given at sufficient length to convey precise information on every subject—are generally taken from the works of the best writers on each topic.

Both the plan and the execution of the work entitle it to our praise, which should have been sooner bestowed on it, had we known the second part would have been delayed for so long an interval.

THE PRESENT DEGRADING CLASSIFICATION OF

NAVAL SURGEONS.

To the Editor of THE LANCET.

MR. EDITOR,—You have lent a helping hand to naval surgeons, for which I hope they are not ungrateful. I am certain, anyhow, that you deserve well of them; for when as gentlemen and members of a liberal profession they were treated with great indignity, you, and some others, alive to the insult so wantonly, if not intentionally, heaped upon them, and, through them, upon the profession, boldly and successfully advocated their cause. In the hope that you will still do so upon all occasions when they have right on their side, I beg to call your attention, and through the medium of your valuable publication that of the corps, to the circumstance of their still remaining classified with warrant officers, viz. boatswains, gunners, and carpenters! It was this *untoward* arrangement which was pleaded in justification of " the exclusion order," which so much galled nearly one thousand meritorious public servants. And as this classification still in various ways subjects naval surgeons to treatment to which the medical men of no other department of the public service are liable, and thereby tends to degrade them in the service to which they belong, and also in that of the sister service, they will have *no more* than justice when they are ranked with commissioned officers, and treated accordingly. There are many other invidious distinctions between the medical men of the two services very grievous and mortifying, but to them I will not further advert at present. But to the distinction of rank, and consequent consideration in the eyes of the public, I do earnestly solicit your attention and that of the aggrieved parties. It is but a small matter, and one unattended by expense or trouble, that I ask for men who are every way worthy of a better consideration. It is only this—that they be removed from the class of *warrant* to that of *commissioned* officers. There are naval surgeons in high station in London, who, if they care not for this themselves, will, I hope, use their influence to have the distinction, so amazingly hurtful to the feelings of the body to which they still belong, removed. I call also on the corps, individually and collectively, to use their endeavours for the same end, viz. the respectability, and consequent usefulness, of the naval surgeons. Without this, the fine laced uniform now to be worn is only a piece of mockery.

I am, Mr. Editor,
ONE OF THE AGGRIEVED.
London, June 2, 1832.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

BY

MR. BRODIE,

Delivered May 14th, 1832.

NEW TREATMENT OF RANULA.

THERE is a case of this nature now in the hospital, which Mr. Brodie is treating by passing a *silver ring* through the cyst. Remarking on it, he said, " This is a new method which I am trying in the treatment of this disease. I have employed a silken thread seton, but the rough surface of the silk irritates too much, and I am now trying what this smooth silver ring will do. I saw a few days ago an infant three days old, who had congenital ranula, which I punctured with a sharp needle."

18th. The silver ring has come out, and the fluid is again collecting.

INFLAMED BURSÆ.

"There are two species of this; you may have the bursa between the ligament of the patella and the skin, inflamed. This is very common, and also very apparent. You may also have the bursa inflamed, which is situated under the ligament, between it and the bone. This is as common as the other, but is more painful, from being in closer contact with the bone beneath."

WOUNDS ON DISSECTION.

"In some of these cases you will find the wound heal, and there will be no swelling of the arm whatever, either in the course of the veins or absorbents; but you will in a short time find an abscess presenting itself under the great pectoral muscle of the same side, which will be attended with acute and intolerable pain. I used formerly to think that the glands in the axilla were affected in these cases, but I once had an opportunity (the only one I ever had, for nine times out of ten these cases recover) of examining a case of this kind after death, and I then found that the axillary glands were scarcely affected at all, but that the cause of this dreadful pain arose from the abscess being situated amongst the axillary plexus of nerves."

TREATMENT OF DISEASED BONE.

"You must not suppose that when we treat diseased bones by putting them up in pasteboard splints, it will cure the disease. They merely keep the limb quiet and at rest, and thus prevent the disease extending to any of the neighbouring articulating surfaces. Even with this care the disease is frequently a very long time before it leaves the patient, depending entirely upon what species of disease it is."

ACID SALIVA.

The case is that of a patient who had had a venereal attack a twelvemonth previously, and who now has slight erythematous pustules over his body. On visiting him today he complained of having pains in all his joints; he has also an ulcerating sore on the posterior and upper part of the right thigh. Mr. Brodie tested his saliva, and found it acid. "It ought to have been alkaline," Mr. Brodie observed. "It is a very singular fact, but Dr. Prout informs me that he has met with a great many persons who have acid saliva, and since he has named it to me I have remarked the same circumstance myself in many of my own private patients. Now this is a very extraordinary fact, and should only be met with in cases where there is very great disorder in the system." On our visiting this patient a few days afterwards, we found this acid state of the saliva to be no longer present.

PULSATING TUMOUR IN THE NECK.

In a case where a patient has a pulsating tumour situated on the right side of the neck, Mr. Brodie has ordered him to be repeatedly bled in small quantities, and ever since the man has been in the hospital, the tumour (in Mr. Brodie's opinion) has gradually decreased in size. Mr. Brodie considers the case to be one of inflammation of the cellular texture around the artery (thus causing a thickening and swelling which pulsates with the artery), rather than an aneurismal tumour. "Such cases as these, Gentlemen," remarked Mr. Brodie, "are not of uncommon occurrence. I had a case somewhat similar in this hospital, in which there was a pulsating tumour in the groin, and every one, upon superficially observing it, pronounced it to be an aneurism; but upon inquiring more particularly into the case, I discovered that the patient had some affection of the hip-joint, which had extended to the inguinal glands and cellular membrane around the trunk of the artery, and consequently formed a pulsating swelling. There was a case also admitted under the care of the late Mr. Ewbank; the patient had a longitudinal pulsating swelling in the thigh, which every one who saw it supposed arose from dilatation or ossification of the coats of the artery. An operation was performed upon this patient, and when the artery was discovered, to our surprise it was of its natural diameter, but the parts around were very much thickened."

With regard to the case which called forth these remarks, Dr. Hamson supposed it to be one of aneurism, from the noise that was communicated to the ear when it was examined by the stethoscope; but Mr. Brodie in our opinion pointed out a surer method of diagnosis,—the want of a well-marked boundary line around the tumour. The man was ordered to be kept perfectly quiet; his right arm to be supported in a sling.

18th. Since we last saw him he has had eighteen leeches applied over the tumour. Mr. Brodie believes to-day that by pressure he can empty it like an aneurismal tumour; but if it be aneurismal, it is so situated as to be entirely beyond the reach of the knife or the ligature. He was ordered,

Magnesiæ sulphat. ʒij;
Infus. rosæ ℥ij; *omne primo mane sumend.*

25. Mr. Brodie believes the tumour, if an

aneurism, to be one of the arteria innominata. Dr. Hamson is still of opinion that the tumour is aneurismal, from the *clacking sound* which is perceptible to the ear on examining the tumour with the stethoscope. The *bruit de soufflet* is also heard in the tumour after the man has been walking about the ward.

ENLARGEMENT OF THE THYROID GLAND.

This was a case which had been treated with calomel and opium. Mr. Brodie remarked, that calomel and opium only did good by removing that from the body on which the inflammation or enlargement depended; "and indeed I suppose," added Mr. Brodie, "that most of our medicines act in this way; but in an organic inflammation of this kind, I doubt whether calomel and opium will be of any service at all. I once tied the thyroid artery in a case similar to this, and I thought too with some degree of benefit; but I shall not give a further opinion upon this case until I have consulted the written authorities upon the subject." Since this man has been in the hospital, 56 leeches have been applied to the tumour, and ℥xl of blood have been taken from him in small quantities at a time. The blood has been buffed and cupped. The man spits up a thick greenish frothy mucus; and percussion detects hepatisation of the apex of the left lung.

May 18th, 1832.

DISEASE OF THE BONES OF THE ANKLE.

In a case of enlargement and swelling around the ankle-joint, plainly indicating disease of the bones, Mr. Brodie remarked, that from the circumstance of the man not having had much pain at the commencement of the disease, which is now about five years since, he was led to believe that the disease (which he believed to be scrofulous) first commenced in the bones, and had now extended to the cartilages. "The appearance of the joint also," continued Mr. Brodie, "resembles very much that of a scrofulous one. I have opened those parts of the tumour which seemed to point most, and nothing is to be done except keeping the man quiet. Indeed I think it will be best to amputate the limb (for there is unfortunately no other resource left) whilst the man's health is sufficiently good to bear the shock of the operation. In cases like these, where they occur in young persons, you may sometimes save the limb—but in adults I think you only increase the mischief by delaying the amputation. What is the man's opinion of his leg?" asked Mr. Brodie of Mr. Fernandez. "I really do not know," answered Mr. Fernandez. "I have not spoken to him about it." "You had better sound him on the subject then," said Mr. Brodie, "and tell him I have a very bad opinion of it."

25. There has been some slight hemorrhage from the wounds made in the tumour, which has been stopped by pressure. In other respects the man is not altered for the worse.

FISTULA IN ANO.

"What chance is there of a fistula in ano healing spontaneously?" asked Mr. Brodie of Mr. Babington. Mr. Babington said, that it would depend entirely upon the length of time that the fistula had existed, and upon how deeply the sinus had penetrated into the substance of the sphincter muscle. Mr. Brodie said, that in the case to which he referred, the probe went nearly the whole length of the index finger into the sinus, and that he believed the disease had existed for upwards of six months at least. "Then in that case," said Mr. Babington, "I should say, that the chance of its healing spontaneously is none." "I quite agree with you in opinion," said Mr. Brodie.

DIVISION OF THE SPHINCTER ANI.

"In fistula in ano you will often find, that the sinus in the sphincter ani muscle will admit of a silver probe, whilst it will not admit of the probe-pointed end of a bistoury.* In these cases, I make use of a pair of knife-edged, probe-pointed scissors (Mr. Brodie here exhibited a pair), which answer the purpose exceedingly well, and do not give the patient half the pain occasioned by the division with the bistoury."

STRICTURE.

"In this case you see, Gentlemen, that the temporary contraction of the stricture is completely overcome; but dilating a stricture, only, is not sufficient to cure it. The disposition to contract still continues; and the dilatation of the stricture, to be of any service to the patient, must be kept up permanently, for if a relapse of the stricture occur, it is in general much more difficult to overcome than the first attack. I shall not let this man leave the hospital till he can easily pass a sound for himself."

DISEASED KNEE-JOINT.

In a case of ulceration of the cartilage of the knee-joint, which has been in the hospital for some time, and has much improved under the use of repeated blisters, bandages, and pasteboard splints, Mr. Fernandez remarked, that as the man had been in the hospital for so long a time, and was getting so much better, he thought he might go out. "By no means," said Mr. Brodie. "In cases of abscesses in the loins, or diseases of the vertebræ, the country air is of great benefit; but in a case of this kind, by sending the man out of the hospital you bring back again all the original mischief in an aggravated form, and the man would return to the hospital worse than ever."

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* We are happy to state, that owing to the remarks which we made on this subject a few weeks since, a case of probe-pointed bistouries is now to be found among the other surgical instruments of the hospital.

Inflammation of the Testicle.—In the case of chronic inflammation of the testicle, which was formerly reported, the calomel was discontinued on the 18th, and the patient was ordered to continue the fomentations and poultices to the parts, and to take

℞ *Decoct. sarsæ* oj;
Extract. sarsæ ʒj;
Liq. potassæ ʒiss *quotidie.*

WESTMINSTER HOSPITAL.

VASCULAR SARCOMA OF THE PENIS. — AMPUTATION OF THE PENIS. — FAILURE OF HALMA-GRAND'S SECRET STYPTIC.

Charles Hunt, 59 years old, a gardener residing at Battersea, was admitted under Mr. White on the 18th of January last, with a vascular sarcoma of the penis. He was a man of middle stature, and apparently of good constitution, having what is commonly called a sanguineous temperament. He had enjoyed good health all his life, having never been annoyed with more than occasional sore throat and coryza. He had been married many years, had led a regular life, and had never had any venereal malady. About two years ago, he first perceived a little hard tumour about the size of a pea on the right side of his prepuce. This continued to enlarge, varying considerably in its rate of growth, and growing fastest in the summer season, or when he felt out of health. It progressively involved the whole prepuce and adjacent integuments, and extended into the substance of the penis. On admission, the anterior two-thirds of the penis consisted of this sarcomatous mass, showing an appearance of large flabby granulations, covered with a rather offensive pus. The man's health was unaffected, and his appetite good. He was subjected for a month to an ineffective treatment by external applications, when his health showing signs of deterioration, it was determined to amputate the penis. On Saturday, 18th February, Mr. White took off this organ as high up as he could. Both the profunda and dorsalis arteries bled freely. Mr. White did not apply a ligature to either, but permitted Mons. Halma-Grand to apply his styptic, which was done in this manner. Dossils of lint were dipped in the styptic, and successively applied in the manner of compresses on the incised surface of the penis. The wound was entirely filled with compresses before the hemorrhage ceased. The patient complained of being "smarted and burnt" by the application. Over all, pressure by compress and bandage was made. No hemorrhage took place for about *twelve* hours, but *then* it occurred most profusely, the patient losing at least thirty ounces of blood. One of the resident surgeons immediately removed all the styptic paraphernalia, and washed the wound with warm water, the bleeding soon ceased, cold cloths were applied, and the patient kept cool and quiet. No farther hemorrhage took place, neither was the styptic again applied, and the wound has gradually gone on healing. The mouth of the urethra has however with difficulty been kept open, a great contraction or puckering of the surrounding tegument having taken place during the cicatrization; bougie No. 9 will now however pass without difficulty, and the urine flows in a good stream. His health this day (20th May) is perfectly good, and he is noted for discharge.

LONDON HOSPITAL.

BURN.

About nine o'clock on the evening of the 20th, a little child, one year and three quarters old, was brought to this hospital with the back part of the head, neck, and arms, very severely burned, from the clothes having caught fire. Wine and laudanum were assiduously administered, and linseed oil and turpentine were applied to the parts, but reaction was not restored. The little sufferer lingered till about three o'clock the following afternoon, when it died. The nurse says that the parents of this child lost another in the same ward about five months since by the same means. Let us hope this is the last of the offspring whose safety appears to be so grossly neglected.

AMPUTATION OF THE TOE.

On Friday the 18th instant, Mr. Adams amputated the great-toe of the right foot of W. W., ætat. 17, of florid complexion, for a fungoid tumour which had formed there.

The tumour is of about the size of a small egg, of a blueish appearance, and cartilaginous hardness. There has been at times considerable hemorrhage from the tumour. He states, that about twelve months since, a "black blister" came on the toe, succeeded by this tumour, which has gradually increased in size. About two months since it was opened with a lancet, when violent hemorrhage took place. Has occasionally suffered severe lancinating pains in the tumour. The operation was very neatly performed. Two ligatures were applied, and the edges were brought together, and retained in approximation by a suture and strips of adhesive plaster, after which a roller was applied. The patient bore the operation without uttering a syllable expressive of pain. The toe was examined. The tumour, on being cut into, presented a uniform cartilaginous structure, with bands radiating from the centre. None of that soft substance so frequently found in scirrhous tumours could be squeezed out.

22. The foot was dressed to-day, looks very well, there is not much discharge; both the ligatures came away to-day. In every respect doing well.

INJURY TO THE THIGH.

Emma Durbridge, ætat. 4, was admitted on the afternoon of the 23d May, in consequence of injuries arising from the wheel of a waggon having passed over her thigh. The wheel had lacerated the integuments of the thigh, from about two inches above the knee-joint to within an inch of the spine of the ilium, completely separating the integuments from the muscles, so that the fingers could be passed from the inner to the outer side. The femur was not fractured, and the artery had escaped injury. There was very trifling hemorrhage, but the nervous system had sustained a very severe shock. The pulse was small and extremely quick; the skin cold; respiration hurried; the child restless, and throwing its arms about, but it did not appear to be suffering much pain. Fifteen drops of laudanum were given immediately on admission, and the edges of the wound were brought together by sutures, several of which were applied, and then strips of adhesive plaster, over which a roller was passed. Ordered a tablespoonful of wine every hour.

May 24. Had dozed continually during the night; is evidently sinking; extremely restless, throwing its arms about; respiration laborious and hurried; pulse extremely rapid; face livid; skin cold. At seven p. m. died.

STRANGULATED FEMORAL HERNIA.

A. L., a woman, ætat. 67, was brought into the hospital about eleven o'clock of the night of the 22d May, labouring under symptoms of strangulated hernia of the right side. While in the act of reaching to tie up a line, she had felt something give way, and a swelling formed in the groin, about the size of a duck's egg. Several attempts at reduction had been unsuccessful, and the patient was sent hither for operation. The warm-bath and taxis again failed, and as the symptoms were urgent, the intestine having been strangulated for nearly five days, it was determined to perform the operation without further delay.

On cutting into the hernial sac, Mr. Luke found very great caution necessary, as the sac was adhering intimately to the intestine, there not being a drop of fluid in the sac. The intestine was separated from its adhesions, the stricture was divided, and the edges of the wound were brought together, and retained by one suture, over which a compress was placed.

23. Passed a tolerable night; complains of pain in the abdomen; pulse 80, but not very full or hard; tongue furred; no stool since the operation.

℞ *Magnes. sulph.* ʒij;
Aq. m. pip., ℥viij; *capiat ægra cochl.* ij *secunda quaque hora*;
Pil. opii, gr. j, *nocte sumend.*

24. Passed a tolerably comfortable night, but complains of griping pain, and tenderness in the abdomen; bowels opened twice; pulse 74, quiet; tongue still furred. Is allowed beef-tea and arrow-root. In the evening, her bowels becoming very much relaxed, she was ordered to discontinue the mixture, and take *tinct. opii* ♏xx; pulse quick and small; tongue very much furred; dry skin, cool; tenderness of the abdomen not so great as in the morning.

25. Slept very well during the night; bowels not relieved since eight o'clock last evening; pulse 100, small; skin hot; thirsty; tongue dry, and very much furred. To take the mixture as prescribed on the 23rd.

29. From last Saturday the patient has gone on very well; the bowels continue open, the tongue clean, and a daily amendment leads us to expect a favourable termination.

FRACTURED TIBIA AND FIBULA.

T. S. ætat. 23, was admitted on the night of the 7th April, with injury to the left leg, caused by the wheels of a hackney-coach during intoxication. Prior to his coming in, a medical man had pronounced the fibula to be fractured, but no fracture could be detected in the hospital. There was very little swelling, but the patient complained of severe pain, for which leeches and lotions were applied; bowels confined; took a dose of house medicine. The case continued in this state until the 17th April, when an abscess formed on the back part of the leg, opposite the seat of fracture, and on lifting the leg for the purpose of examining this abscess, the fracture of both bones was instantly detected. A poultice was ordered to be applied, and the leg placed in splints. Another abscess formed immediately over the fracture, which was opened with a lancet on the 23rd, when about two ounces of pus were discharged. The leg continued without union, discharging daily a quantity of pus, until the evening of the 4th of May, when sudden and profuse hemorrhage came on, which was by the dresser controlled by pressure on the artery in the groin, until the tourniquet could be procured. Mr. Hamilton came down that night, and saw the patient, and determined to defer the operation of amputation until the next day, the patient being extremely exhausted. Pulse scarcely to be counted; and was ordered wine and brandy every ten minutes; upwards of half a pint of which was given him. The tourniquet was kept on the limb during the night. The patient lost about two pints of blood in about two minutes.

5th May. This morning the leg was removed about four inches below the knee. Very little blood was lost during the operation. Six ligatures were applied; the operation was performed neatly enough by Mr. Hamilton, one of the assistant-surgeons. The hemorrhage was found to have come from the posterior tibial artery, which was in a state of sloughing. Took tinct. opii M. XL; and was ordered four ounces of wine and a pint of porter daily.

6. Passed a very restless night; pulse 130, weak; countenance anxious; tongue furred; appetite bad; skin profusely covered with perspiration; complains of the limb starting very much, which wakes him out of his sleep.

8. The stump was dressed to-day, and looks very well; bowels open three times; skin cool and moist; pulse 110; small and weak; countenance anxious; feels very drowsy. To discontinue the porter.

10. The whole of the ligatures came away to-day; the stump looks very well; union by adhesion having taken place throughout the greater portion, the discharge is not considerable. Bowels open; appetite bad; tongue clean; passed a restless night; starts and talks very incoherently in his sleep.

12. Complained of having passed a bad night, although he took 40 drops of laudanum. Sickness of stomach; has no relish for food; profuse sweats, particularly at night; bowels have not been opened since the 10th. Pulse 112, weak; to take a large teaspoonful of castor oil, and continue the saline effervescing mixture, with ten drops of the tinct. opii.

13. Slept tolerably well last night; bowels opened twice by the castor oil; tongue rather furred; skin covered by a cold perspiration; countenance anxious; sickness of stomach very distressing if he omits the mixture; appetite bad.

15. Stump looks extremely well; the lower portion seems to have a disposition to suppurate; discharge healthy, but rather increased in quantity; his general health appears improving; bowels open; appetite better; takes a chop and porter daily.

18. Sleeps tolerably well; spirits and appetite improved; bowels regular; skin warm and moist; tongue clean; complains of considerable pain in the stump when dressed; discharge considerably increased in quantity.

21. Much the same as on the 18th.

24. The stump still continues extremely painful when dressed, particularly on the inner side; there is a discharge of a considerable quantity of pus daily; pulse 90, small; tongue slightly furred; bowels confined unless he takes the castor oil; appetite very indifferent; still complains of the sickness of the stomach if he omits the mixture, but which he takes much less frequently.

27. Complains of being very weak and low; appetite bad; bowels have not been relieved these three days; stump still continues extremely painful, and the discharge is considerable. To have one grain of calomel with ten grains of rhubarb; and a mixture of the compound infusion of gentian, with tincture of columba.

(*To be continued.*)

INJURY TO THE HEAD.

On the 30th of April, G. C., a strong healthy man, ætat. 46, was admitted under the following circumstances. From the account given by his friends, it appeared he had been quarrelling with another man, by whom he had been forcibly thrown down; and while on the ground had received a violent kick on the upper part of the head, which had caused a lacerated wound of the scalp upwards of five inches in length. He was insensible for a few minutes, and afterwards vomited the contents of his stomach. At the period of his arrival here, he was tolerably sensible; his pupils were natural; pulse small but weak; his head was partially shaved; the edges of the wound brought together by strips of adhesive plaster, and the patient ordered to be kept quiet in bed; to take a dose of house medicine.

May 1. Complains of constant pain in the head, extending across the forehead; pulse 78, full. To take *hydr. submur.* gr. ij *omnis 6tis horis*. In the afternoon considerable hemorrhage took place from the wound in the scalp, which was allowed to continue; he lost about 16 ounces of blood in this manner, which appears to have had a beneficial effect in diminishing the pain in the head.

2. Pulse 68, full; pupils natural; skin moist; bowels open; pain in the head diminished; is rather thirsty. To continue the calomel; to have the strapping removed and a poultice applied; and to be bled to 14 or 16 ounces.

3. Pain in the head much less severe; pulse 76, moderately full; bowels open; skin natural; tongue clean; complains of his mouth being rather sore; the blood drawn does not exhibit the buffy coat.

6. Pain in the head very trifling; complains of his mouth having been made very sore by the pills, which have been discontinued; pulse about 70 and quiet; skin cool; tongue clean; bowels open; the wound in the scalp looks clean, and discharges a small quantity of healthy pus.

10. Pulse 64, rather full; bowels have been much relaxed, but are not so much so now; tongue clean; skin natural; is free from pain in the head, excepting in the wound of the scalp, which is rather painful when dressed; is a good deal disturbed by his dreams, which are at times rather frightful; the soreness of the mouth is much less.

From this time he has continued without any bad symptoms.

REMOVAL OF THE TESTICLE.

On Wednesday, May 23rd, Mr. Luke removed the right testicle of F. H., ætat. 23, for a scrofulous disease of that organ. There was very little blood lost during the operation, and upwards of eighteen ligatures were applied.

The patient states the testicle first began to swell about nine months since, which he attributes to his having received a blow on that part. He had been attending this hospital for some time as an out-patient previous to his admission, during which time leeches and mercurial ointment were the remedies employed. About four months ago had hydrocele of that side, which was tapped, and about six ounces of fluid evacuated; the hydrocele was not injected. About a fortnight previous to his admission, the scrotum began to ulcerate, which soon increased in size, and the slough is now about the size of a crown piece, the surface covered with fungous granulations and unhealthy-looking pus, and the testicle much enlarged.

The testicle was not examined after the operation during the time we remained in the theatre. Cold lotion was ordered to be kept constantly applied to the part, and twenty drops of the tincture of opium to be taken at night.

24. Passed a very restless night, having been much disturbed by a noise made by a patient in a state of delirium. Bowels not relieved; skin warm; pulse 108; tongue clean; spirits good; took a dose of house medicine.

25. Slept well; bowels open; tongue rather furred; pulse 94, moderately full; skin hot; is rather thirsty; free from pain; keeps the cold lotion constantly applied to the scrotum.

26. Passed a restless night; bowels open; skin hot, but which he says is generally the case; tongue clean; pulse rather quick; spirits very good.

27. The wound was dressed to-day, and looks very well; there is very little suppuration; several of the ligatures came away to-day.

30. Several of the remaining ligatures came away in the dressing to-day; the wound looks well; the patient is in every respect doing well.

TREATMENT OF FRACTURES OF THE ARM AT

ST. BARTHOLOMEW'S HOSPITAL.

To the Editor of The Lancet.

Sir,—Your humble servant, a constant reader from the first Number of The Lancet to the last, feels entitled to address you upon the two cases of amputation of the arm which are reported at page 223 of The Lancet of Saturday the 19th inst.

Is it necessary to amputate arms, without giving nature the smallest chance of interfering, purposely to enable hospital surgeons to keep their hands in practice?

Case 1, *boy aged* 13.—A lacerated wound (not contused); ulna fractured in a very comminuted manner. Now, Sir, there is an opinion, that had the case been properly managed, the boy's arm admitted of a fair chance of being saved.

Case 2. *Contusion*,—and for that reason being worse than the other, there should have been a consultation. But no; the surgeon was fearful he would lose his cut! Never thought about saving the arm, but whipped it off ere Mr. Lloyd could be informed of it. The head of the radius broken; elbow-joint injured very little;

compound fracture of both the bones of the fore-arm ; not comminuted.

Now these two cases occur in young and healthy youths, and the chances of restoration and recovery in them, compared with a man of 30, is as 20 to 1 in favour of youth.

A skilful surgeon, John Bell, managed the following case, and I have seen worse accidents recover without amputation, than the above cases.

A slater fell from the top of a house ; had his life saved by alighting upon his *right hand.* Luxation of one rib. The radius was terribly shattered. The wrist-joint was burst with great violence ; the hand hung on one side, and turned, as it were, upon a point ; the small end of the ulna projected through the wound, and the hand looked as if spitted on this bone ; the hand was shortened by the manner in which the radius had yielded, for it was shattered and broken into small fragments, and so violently was the hand distorted, that the head of the radius was turned out at the wound. Along with the radius, one of the carpal bones projected through the wound ; quite separated.

Treatment.—I cut off the end of the radius, took up the bleeding arteries, snipped away the carpal bone, for it hung by a rag of tendon, and laid the flat inside of the fore-arm upon a splint ; made a stitch in the lacerated skin of the wrist, and finished by laying a large flat sponge, just a little moistened in spirits, upon the open part of the wound. This closed the laceration so smoothly, that almost the whole of the integuments adhered. The part which failed was the contused part ; it suppurated and did well. This man was three months under this able surgeon's care, owing to a knotty swelling which surrounded the wrist, which was very hard to discuss : but at the end of that time he could use his hand pretty freely, and, what was little to be expected, the joint also was restored. He recovered the rotation of the radius, as well as the bending motion of the wrist, and now he finds his hand perfectly useful and strong for work.

Would to God J. Bell had had the two unfortunate boys under his management, for if he could not *certainly* have saved their arms, at any rate he would have given them *a chance.* It is to be hoped that a fuller detail of those cases will be given in your LANCET, to rescue the hospital from the exposé which these boys' friends might occasion, by coming into court for some compensation equivalent to their loss.

JUSTICE.

May 23d, 1832.

UNIVERSITY OF LONDON.

DISTRIBUTION OF PRIZES.*

ON Wednesday, the 16th ult., a numerous meeting of the students and their friends took place in one of the theatres, to witness the distribution of prizes to those gentlemen who had been successful candidates at the previous examinations of the medical classes.

The chair was filled by Dr. Lushington in the absence of the Duke of Sussex, who was prevented attending.

The business of the meeting was opened by Professor Thomson, who read the report of the medical professors for the session, in which he stated that the medical school had answered the expectation formed by his colleagues and himself, two hundred and fifty-two students having attended the medical classes during the session.

The following were the successful candidates :—

Principles and Practice of Medicine.

Professor ELLIOTSON.

Gold Medal—James Wearne, of St. Ives, Cornwall.

1st Silver Medal—J. Storrar, of London.

2nd ditto—John P. Wall, of Ross, Herefordshire.

Anatomy—Prof. QUAIN.

Gold Medal—J. R. Noble, of Hawkeshead, Lancaster.

1st Silver Medal—George Houlton, of Killingholme, Lincolnshire.

2nd ditto—C. Nossoc, of London.

Practical Anatomy.

Mr. R. QUAIN, *Demonstrator.*

Gold Medal—W. Rayner, of Castlemoat, Lincolnshire.

1st Silver Medal— W. K. Wright, of Bristol.

2nd ditto—J. Bartlett, of Great Bedwin, Wilts.

Surgery—Prof. S. COOPER.

Gold Medal—R. Lanyon, of Camborne.

1st Silver Medal—W. Rayner, of Castlemoat, Lincolnshire.

2nd ditto—David Hartley, of Bristol.

Midwifery—Prof. DAVIS.

Gold Medal—G. P. Gill, of London.

1st Silver Medal—W. Rayner, of Castlemoat, Lincolnshire.

2nd ditto—J. N. Hudleston, of London.

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* We have delayed publishing this list, in consequence of not having been before furnished with a correct account of the successful students.—ED. L.

Materia Medica—Prof. THOMSON.

Gold Medal—Francis Taylor, of York.
1st Silver Medal—M. Forster, of Holywell, Bedfordshire.
2nd ditto—G. Allarton, of Warwickshire.

Chemistry—Prof. TURNER.

Gold Medal—D. B. Meek, of London.
1st Silver Medal—Henry Cook, of Hampstead.
2nd ditto—A. M. A'Beckett, of London.

Comparative Anatomy—Prof. GRANT.

Gold Medal—D. W. Nash, of Bristol.
Silver Medal—J. Storrar, of London.

Medical Jurisprudence.
Professors THOMSON and AMOS.

The Prize—P. H. Chavasse, Birmingham.

Botany—Prof. LINDLEY.

Gold Medal—R. B. Hinds, of London.
Silver Medal—J. H. Rogers, of London.

Each gentleman having received from the President the honour awarded to him, the President, in a short and excellent address, testified the pleasure he felt on the present occasion, in distributing the rewards of merit. He proceeded to state that the Council felt, equally with the professors, the necessity for a hospital, and he might say things were in such a situation now, that it would not be long before they had one. The situation seemed greatly in want of one, being surrounded by a densely-populated neighbourhood; and it was only reasonable to conclude, that the benevolence of the public would assist their efforts in such humane intentions. He concluded by exhorting the students to preserve the same honourable course which had this day gained them distinction, and they would do honour to the school to which they were attached; so by their assistance, and the broad principles on which the institution was founded, the University of London must prosper.

PRIZES TO THE MEATH HOSPITAL PUPILS.

ON Saturday, the 28th of April last, the annual distribution of premiums to the medical students of the Meath Hospital took place, when premiums were awarded to the following gentlemen, by Dr. Graves and Dr. Stokes, physicians to the hospital.

1st premium,	Mr. Alfred Hudson.
2nd ditto	Mr. Thomas Purefoy.
3rd ditto	Mr. — Fullaker.

No pupil can obtain a premium at this hospital who has not been engaged in taking cases and making daily clinical reports to the attending physician for at least six months. By this means tolerably constant attendance, at least, is ensured.

FILTHY STATE OF DISSECTING ROOMS.

To the Editor of THE LANCET.

SIR,—You have so often shown yourself to be the student's friend, and the corrector of mismanagement and abuse in the public schools of this metropolis, that I do not hesitate to appeal to you upon a subject which may appear to some to merit but little notice, but which to me and to many of my fellow-students is an intolerable nuisance. I allude to the filthy state of some of the anatomical schools, but more particularly the one in Little Windmill-street. Upon entering the dissecting-rooms, the museum, &c., the nose is assailed by a "compound of villanous smells" not to be described. Is this to be wondered at, when putrid animal matter may be seen in every corner dragged by the rats (which abound in this building) even from the dissecting tables? As to a mop or a broom it would be a perfect *rara avis* in this establishment. I should be sorry to be considered as more fastidious than my brother students, but I do say, that the health of the pupils ought to be considered, and therefore that there should be a plentiful supply of water, and that the utmost cleanliness should be observed. Trusting to your kindness in permitting this short letter to appear in your journal,

I am, Sir, your very humble Servant,
A WEST END PUPIL.

DR. KNIGHT'S PAPER, PAGE 300.

The Editor has been requested to add the following note to Dr. Knight's account of "Three cases of Incipient Cholera," published at page 300 of the present number of THE LANCET. The note reached us too late to be appended to the paper itself:—

"In all the cases the urine was nearly, if not wholly, suppressed, not more than two or three tablespoonfuls having been voided, from the commencement of violent vomiting till its cessation from the effect of the alkaline salt, which in one case was in about ten hours."

CORRESPONDENTS, &c.

The letter of *Mr. Sharp* can only appear on the cover, and must be accompanied by a London order for payment.

We shall shortly review the work, some points in which are so sharply handled by *Testor.* For that reason we make no use of his letter.

Medicus. A part of the list appears in another form. For more than this we have not space.

A Constant Reader. The remainder of the letter is a pure advertisement.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JUNE 16. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXIII.

THE FRONTAL BONES OF CATTLE, SHEEP, SWINE, AND DOGS.

The horn is the principal weapon of offence belonging to the ox. To enable him to use it effectually, it is necessary that it should be firmly based on the head. It is to give sufficient and secure attachment to the horn that the frontal bones of the ox have that great development of surface and substance which we see in the specimens before us.

The great Extent of the Frontal Bones.—The frontal bones in this animal reach from the nasals to what we have called the occipital ridge or crest in the horse; but that crest is here formed by the union of the frontal and parietal bones, so that the parietal appears on the upper and fore part of the head, only at this ridge; behind, it occupies the situation of the superior part of the occipital bone in the horse; and on each side it is deeply buried in the temporal fossæ, while the occipital bone is depressed far below the ridge, and has comparatively little volume.

The frontal Sinuses extending through the whole of the Frontals.—This immense expanse of bone, were it solid, would be most inconveniently and fatiguingly heavy to the animal, and therefore we find not only a separation of the plates of the bone, constituting the frontal sinuses which I have described in the horse, but extending from the place of the commencement of the frontal sinus in the horse to the summit of the head, and even below that, through the parietal and the occipital bones, to the foramen magnum, and likewise continued almost to the tip of the horn. There is a strong and perfect septum, as in the horse, between the sinuses on each side, reaching from commencement of the sinus to the foramen magnum. The other septa are imperfect, as in the horse, so that there is one continuous cavity or sinus, on each side, from the inner angle of the eye to the great foramen, and the summit of the horn. All these connected cells are lined by a continuation of the schneiderian membrane, and their extent accounts for the more serious character which nasal gleet, and other affections of this membrane, assume in cattle, and for the frequent accumulations of pus about the root of the horn, and particularly at the posterior part of it, and which we are sometimes obliged to evacuate by perforating the horn.

The Blood-vessels and Nerves of the Forehead.—It will be seen that the supra-orbital foramen must necessarily open into the frontal sinus, and that the nerve and artery which supply the forehead must pass through a portion of that cavity: and that as they have a greater surface to supply, they must be considerably larger than in the horse. In fact there are usually two foramina on each side; and as the increased size of the artery and nerve would render them more prominent, they are lodged in a depression or sulcus, extending both above and below, until they divide into smaller branches less liable to injury.

In the polled variety of oxen, the frontals reach to and below the parietal crest; but the diploe is not so great, yet the frontal sinuses are considerably larger than in the horse. This, however, varies much in the different breeds of polled cattle.

I shall not enter into a full description of these bones, as in the horse, but content myself with the points of difference.

Difference in the Formation of the Frontal Bones, compared with those of the Horse.—The orbitar process, as you will see in these crania, is not so developed as in the horse. It presents a much narrower and sharper surface externally, and its breadth is from without, inwards. It is small and weak compared with that of the horse, because the upper and outer portion of the orbit of the eye is defended from injury by its very situation, while it is the part most exposed to danger in the horse; and it abuts upon, not the temporal bone as in the horse, but a bifurcation of the malar bone, and does not enter at all into the composition of the zygomatic arch. That arch, depressed in its situation in the ox, needs not the frontal bone to give it curvature and strength, for the food of this animal is gathered more at leisure, and the process of rumination is slowly and sleepingly performed, and there is not wanted so much bulk of temporal muscle, nor such extensive motion of the coronoid process of the lower jaw. The orbitar process is united to the upper bifurcation of the malar bone by a suture both squamous and serrated. Its union with the parietal—there is but one parietal in the ox—is found at the summit or ridge of the head, and deep within the temporal fossæ.

The Growth of the Horn.—The greatest variation, however, between the frontals of the horse and of most breeds of oxen consists in their prolongation in the form of the internal or bony part of the horn. In the fœtus of three months, there is no bony enlargement at the situation of the future horn; but it soon afterwards begins to be visible, and at the seventh month there is a distinct tubercle raising the skin. This continues to grow, forcing its way through the integument, and carrying with it the cuticle, which by degrees hardens over it.

The Dropping of the Cuticle.—The cuticle continues to cover the horn until the beast gets twelve or fifteen months old; and then it begins to peel off in laminæ or scales, discovering the horn beneath smooth and glossy. The detachment of the cuticle is soon perfect towards the tip of the horn, but a portion of it continues to cling round the base; and even at three years of age some of the cuticle remains there in the form of a ring, and successive rings are afterwards added year by year, marking the age of the animal,—but of this presently.

The great Vascularity of the Horn.—The horn, as it is commonly called, is composed of an elongation or process of the external plate of the frontal, covered by a cartilaginous investment or defence. This bony process, in its prolongation from the frontal, contains likewise a continuation of the cells of that bone, and it is vascular to an extraordinary degree. Its surface is furrowed and perforated by innumerable vessels, which are designed not only for the nourishment of the bone itself, but of the horn by which it is covered. It is on account of this great vascularity of the bone, that, when the horn is broken, there is often so much hemorrhage, and which the surgeon finds it very difficult to arrest. A veterinary friend, who was compelled to amputate the horn of an ox on account of an enormous bony excrescence that was forming on it, told me that the blood spouted out in a stream as big as his finger, and which he could only stop by the long application of a large budding-iron.

Why great Inflammation follows a Fracture of the Horn.—The continuation of the frontal sinuses through the horn will also enable us to account for the violent inflammation which often follows a fractured horn. The injury of the membrane would be a sufficient cause of inflammation; but air is admitted, not indeed into a cavity from which it had before been altogether excluded—it is not a serous membrane—but where it had not free access, and the unaccustomed stimulus produces great irritation. Tetanus is not an unfrequent consequence of a neglected fracture of the horn. Farriers and cowleeches are properly anxious to close the opening. They place over it plaster upon plaster of tar or pitch, not merely to restrain the hemorrhage, but to exclude the air.

The Horn never perfectly reproduced.—The bone of the horn once broken off is never reproduced, but if any partial fracture has taken place, union is most easily effected between the divided edges by careful bandaging. I recollect one case in which the horny covering was torn off, and half of the bone likewise broken off. It was replaced after the lapse of a few minutes, and perfect union was speedily accomplished. Nature often attempts to restore a lost portion of the frame, but the process is irregular, a misshapen mass of bone alone is here formed on the fractured part. I have one that weighed, when first separated, no less than thirty-seven pounds. The horny covering is generally reproduced in young beasts. There is considerable hemorrhage, which, however, gradually ceases, and the bone is surrounded by a layer, more or less thick, of coagulated blood. This by degrees hardens and forms a case for the bone. Presently another process commences at the base of the bone: —a dense flexible substance of the colour of the hair of the forehead appears—it thickens, hardens, and becomes good horn—it extends up the bone—it displaces this temporary crust as it proceeds, and at length covers the whole of the bone, and becomes almost as perfect as the original horn.

Composition of the Horny Covering.—The covering of the bone differs little in construction from the horn of the foot of the horse. It is composed principally of coagulated albumen with a small portion of gelatine, and about a half per cent of phosphate of lime; indeed, when either the horn or the hoof is calcined, there is scarcely more than $\frac{1}{360}$ part of earthy matter. The albumen is somewhat greater in quantity in the horn of the ox, and hence its superior hardness. It is divided by maceration, and even mechanically into numerous laminæ or plates, but no nerves or blood-vessels have, after the most careful injection, been traced into it, although they doubtless exist there.

Towards the base of the horn the covering is very thin. It seems to be a continuation of the cuticle. After the most careful examination, it does not appear to be separable from the cuticle, but the process of maceration will distinctly separate them.

How is the Horn produced?—It has been disputed whence or in what way the horn is produced. Some have contended that, as the crust of the horse's hoof is principally secreted from the coronary ligament, so the horny covering of the bullock is secreted by, or is a prolongation of, the cuticle at the base. That a portion of it is derived from the integument at the base cannot be doubted. It was the original covering of the bone; and the rings, which increase yearly in number and recede from the root of the horn, clearly point out this origin; but as the horn recedes from the base, it thickens very much, and this thickening, and indeed the greater portion of the horny covering, is secreted from the vascular substance underneath.

The Interposed Vascular Substance.—The connexion between the horn and its bony base, is more simple than that between the hoof and the os pedis. There are no laminæ or lamellæ, fleshy and horny, both elastic, and one extensible, the one received into the other, forming a great extent of union, and the strongest possible union, yet which would by the elongation of one of the parts adopt itself to the varying superincumbent weight. There is no weight here—no necessity for elongation; and therefore we only find a dense, vascular, filamentous, reticulated substance surrounding the bone; and there is a corresponding filamentous reticulated substance on the interior of the horn—appearing to be horn partly formed, but with numerous irregular edges, and papillæ, and sulci, corresponding with the irregularities of the opposed surface. When the horn is first separated from the bone, the adaptation of the filamentous substance of its interior to the irregularities of the fleshy substance beneath is very striking. From this vascular substance the horn derives the greater part of its nutriment. If a portion of the horn is raised from the substance underneath, a whitish mucous secretion will soon appear in the form of little papillæ or points; which will become more numerous, and approximate and coalesce, and harden and form a thin layer of imperfect horn. There are evidently escaping from it, as plain to the eye as are the furrows of the bone, a multitude of vessels, which we can trace to the softer interior part of the horn, but not further. Some have fancied that they have traced numerous fibriculi, nervous or vascular, escaping from the papillæ of the elastic cushion belonging to the bones, and ramifying over, or penetrating into, the interior surface of the horn.

The Heat at the Base of the Horn a Test of the Degree of Fever.—The thinness of the horn at the base enables us to account for the common practice of farriers and cow-leeches, of placing the hand on the root of the horn, in order to judge of the degree of fever; and they do ascertain it with considerable correctness, for the skin is thin, and the bone beneath is highly vascular. It is covered by a mesh-work of vessels, and the temperature of the frame, being one indication of the degree of fever, is at once evident: this bone is likewise lined by a continuation of the schneiderian membrane, which would sympathise with any intense affection of the thoracic viscera.

Great Tenderness at the Root of the Horns.—Brutal fellows are too apt to strike the ox on the root of the horn. The thin covering over a vascular and sensible part is an inadequate defence, and the animal suffers great pain from the blow. It is said that some fool-hardy persons, armed with a stout stick, will not shrink from the attack of the most ferocious bull. A few smart blows on the root of the horn will inflict so much pain, that the animal will retreat from the combat.

Strange way of Modelling the Horn.—On account of the thinness of the horn at this part, and the imperfect formation of the supporting bone at an early age, some miscreants have undertaken to give any desired form and direction to the horn. The process is painful enough, for the horn is softened by the application of hot irons about its root; and it is said that many of the beautifully-turned horns, which are so much admired in various breeds, had their shape and curve artificially given to them.

This art, however, is not confined to European brutes. Mr. Barrow tells us that the long horns of the oxen in the south of Africa are often trained by the Namaquas and other tribes so as to twist in spiral curves and other fanciful forms, and that this is managed by the means of hot irons.

The French Method of applying Poultices at the Root of the Horn.—Influenced by this thinness of the covering of the bone of the horn at its base, the French recommend fomentations and poultices around the root of the horn for inflammatory affections of the head. This does not appear to be a very scientific mode of proceeding, for the thin part runs but a little way; and then there is considerable thickness of bone before we arrive at the membrane of the sinuses within. A poultice or a fomentation could hardly have much effect over so small a space, and through the horn and bone.

Different Breeds distinguished by the Horns.—The shape of the horn is one and not the least distinguishing characteristic of the different breeds of cattle. Thus we have the coarse hair, the large hoof, the depth of fore-quarter, and the long spreading horn of the Lancashire. The Cheshire and the Gloucestershire are but varieties, and no improvements of the Lancashire. The Leicestershire, with its thinner skin, and rounder form and smaller bone, betrays the same origin; the little Kyloes seem to be a branch of the same race. Of the short horns there are numerous and valuable families in the Yorkshire, the Durham, the Northumberland, and the Holderness; the diminutive Scottish runt, and the little Alderney, with its crumpled horn: while the Devonshire with its red colour and thin neck, and clean head, and wide hips, but narrow chine, and too flat ribs; and the somewhat larger Sussex, and the still bulkier Herefordshire, have horns of an intermediate length. Then there are the polled cattle of many a varying form and character, from the Suffolk Dun, with its profusion of milk, to the stinted Galloway, the parent perhaps of the breed, and, although little, good as a milker and for the butcher too.

The enormous Horns of some Breeds.—In some foreign breeds, the horns of cattle attain an almost incredible size. The oxen of the Galla country, and the northern part of central Africa, although considerably smaller than the majority of English oxen, have horns nearly four feet in length, and which would hold ten quarts. The Burmese oxen, which are much larger, have singular horns of a half-spiral form. Captain Clapperton says, that "the corneous external coat is very soft, distinctly fibrous, and at the base not much thicker than a human nail; the osseous case full of vascular grooves, and the inside very cellular, the pair together scarcely weighing four pounds; yet they are three feet seven inches in length, two feet in circumference at the base, and one foot six inches midway towards the tip. The longest horn, however, is that of the great Arnee, or Indian buffalo, of an angular form. eighteen inches in circumference at the base, and more than six feet in length.

The influence of Sex on the Horns.—The horns vary with the sex—the short, straight, strong, glossy horn of the bull; the longer, weaker, and branching horn of the ox, and that changing so much with the period of castration; and the more delicate and variously-curved horn of the cow, are objects of every day's observation. The slow growth of the horn of the bull, compared with the rapid development of horn after castration, is a fact deserving of notice, especially as being altogether contrary to the appearance of the horns in other animals;—but the consideration of this belongs more properly to the generative system.

The Age of Cattle indicated by the Horn.—The horn is to a certain degree indicative of the age. At three years old the cuticular covering of the whole of the horn, except at its base, has dropped off, and is not afterwards renewed; a portion of it, however, as I have already described, resembling a ridge or ring of thicker horn, seeming to mark the different layers of the horn, is seen a little above the root, this is forced *downward*, and a new ring appears lower down on each succeeding year; so that if you count these ridges or rings, and add two to them, you will have the age of the beast. The first of these ridges or rings is always the most distinct, especially in the cow, and marks the extent and form of the horn at two years old. The successive rings mark the separation of the horn from the cuticle during each year. The rings, however, are not to be traced in every beast, and they sometimes present a confused character, and seem to run into one another. The two or three first may be distinct enough, but those which follow are nothing more than little irregularities of surface. It should also be remembered, that it is not always that the horns on each side tell the same story, especially in aged beasts. An apparent difference of one year is not at all unusual, and I once saw two more plain and palpable rings on one side than on the other; and to this it may be added, that it is very easy for the dishonest chapman to rasp off one or two, and give a false appearance of youth. We find in the teeth more satisfactory marks of the age, and where no roguery can lead us astray. The rasp and the polisher are oftener used about the horns than many persons are aware. The purchaser likes to see a small and smooth and glossy horn; it is considered to be an indication of blood and of condition, and it is often given to many a beast that has no other pretension to either. The ugly spreading horn can always be reduced a little in its length, and proportionably a great deal more in its bulk.

Larvæ in the Frontal Sinuses.—On account of the vast extent of cavity from the communication between all the partitions of the sinus, the ox occasionally suffers much from the larva of a species of fly which creeps up the nose and lodges in the sinus. He suffers more than the sheep from this cause, but not so frequently, for the frontal sinuses of the ox seem to be more the accidental than the natural and regular habitation of these insects.

Collections of Pus in the Frontal Sinuses.—He is, however, sometimes seen in great pain, holding his head low, and carrying it on one side. This is not from the irritation of these insects, but from the pus which has been effused within the sinuses, collecting about the root of the horn. If the horn is cut off, a very great quantity of matter often escapes, or, at all events, the inflammation is much relieved by the copious bleeding that inevitably follows.

THE FRONTAL BONES OF SHEEP.

In the sheep the frontal bones are less developed. They are more convex both in the horned and polled variety; and in both of them the parietal bone, as these specimens will show you, is permitted to occupy its usual situation on the roof of the cranium, and the ridge is formed by the occipital bone. The ridge or crest is considerably depressed in both. A very small portion only of the bone actually covering the brain is exposed to concussion, when the sheep violently butt each other; and if that which has the brain beneath it is not protected as in the horse by the yielding resistance of the temporal muscle, it is more effectually preserved by being lowered out of the way of danger.

Singular Difference in the Sinuses of the Sheep and the Ox.—The bony support of the horn springs, as in the ox, from the frontal bone, but much nearer to and a little above the orbit of the eye. The frontal sinus in the polled sheep is proportionably smaller than in the horse; there is a singular and irregular depression on each side where the horn should have sprung, which limits the extent of the sinus backwards; but in the horned sheep the sinus reaches, as in the ox, to the very tip of the horn. In neither is there any separation of the plates of the bone at the posterior part of the frontal in the young animal; but when the horn is nearly grown, the sinus penetrates through the frontal, but never reaches the parietal. This is a singular difference in the construction of these bones in our two-horned servants the ox and the sheep.

Variety in the Form of the Horn.—Although the breeds of sheep are not nominally distinguished by the form of the horn, yet the difference in the size and shape of it is much greater than in the ox. It is sufficient to hint at the round bold horn of the Dorset standing from the head; that of the Wilts bending inwards towards the cheek; the spiral and contorted horn of the Heath sheep; and the strangely-convoluted one of the Norfolk, sometimes nearly a yard in length if unwound, and a full foot in circumference at the base. The best breeds, however, the Lincoln, and the Leicester, and the Cotswold, and the Ryeland, and the South Downs, and many others, are hornless; and a shrewd grazier will tell us that they are the better for being so, for that which would go to the nutriment of a comparatively useless horn, is devoted to the production of flesh, and fat, and wool; yet some of the horned ewes, as the Dorsets, are the best milkers, and particularly selected in the rearing of house lamb.

Varieties in the Horns of Foreign Breeds.—Among some of the foreign sheep, this difference of size and shape is very curious. The Asiatic Argali has horns nearly four feet long, and weighing upwards of thirty pounds; at the base they occupy the greater part of the back of the head, and nearly touch each other above the forehead; and their hollow is so large, that young foxes shelter themselves in those that have fallen off. These horns first bend backwards and downwards, then forwards, and then outwards and upwards. In the American Argali the horns are still larger, covering the sides of the head from the eyes to the occiput, and touching each other at the front; these take a spiral direction. The Musmon, or Corsican Argali, has horns little exceeding the length of the head, and with only one bending.

Of the African sheep, the horns of the Adimain breed form a segment of a circle round the ears; the Morocco turn spirally outwards; the Goitered breed turn inwards towards the forehead; the Congo are bent back upon the neck. The Iceland sheep have three or four or five horns variously bent; the Cretan forms a complete circle at the base, and three spirals above; the Wallachian diverge at almost right angles from the head, and are then twisted into a spiral form; and the Merino ram has a spiral curve turning forwards and laterally into an additional crook.

The horns of the sheep, different from those of the ox, are smaller and of a simpler form in the castrated animal; and in some breeds where the female is polled, the horns of the wethers are curiously diminutive, compared with those of the rams.

The Foramen Supra-orbitarium. — The foramen supra-orbitarium passes through the frontal sinus in both the polled and the horned variety; and, as in the ox, there is a comparatively greater surface for the vessels to supply, there are usually two foramina on each side, and the vessels, at their exit from the bone, are defended by a sulcus or trench; and especially as these animals, even more than the ox, make too free use of their heads in their combats.

The Orbitar Process.—In order to afford space for the attachment or origin of the horn, the orbitar portion of the frontal bones projects both forward and laterally, which gives the peculiar breadth of forehead and prominence of eye to the sheep. This process, occupying the superior and exterior portion of the orbitar ring, presents, forwards, a sharper edge than in the ox, while it has comparatively less depth and strength, except that which it acquires from the more perfect arched form of the process, and the roundness of the orbit.

The orbitar process abuts upon the superior bifurcation of the malar bone, as it does in the ox, and has no connexion with the temporal bone; and, deep in the temporal fossa, it is almost cut off from connexion with it by the curious lateral development of the parietal bones in these animals.

The Œstrus Ovis in the Frontal Sinuses of Sheep.—The frontal sinuses both of the horned and polled sheep are subject to an excessive annoyance, from which the ox is comparatively exempt. There is a fly of the Diptera order (flies with two wings, and behind them two globular bodies, supported on slender pedicles, called, and properly, poisers), the *Œstrus Ovis,* the gad-fly of the sheep. It assumes its perfect and winged form in June and July, and then it is an intolerable nuisance to the sheep, especially in woody countries, or in the neighbourhood of copses. If only one appears, the whole flock is in the greatest agitation. They gather together with their heads in the centre, and their muzzles buried in the sand if they can find any, and are in continual motion to guard their noses against the atacks of their puny enemy. The œstrus, impelled by powerful instinct, endeavours to deposit its eggs on the inner margin of the nose. By the warmth and the moisture they are almost immediately hatched, and the larvæ or little maggots crawl up the nose, and find their way to the residence that nature designed for them. As the eggs of the horse gad-fly, being licked from the parts on which they are deposited, and hatched by the warmth and moisture of the tongue, are conveyed into the stomach with the saliva, or the food, and attach themselves to, and live on, the inner coat of the stomach, until they are ready for their transformation, so the eggs of the gad-fly of the sheep, being deposited on the edge of the nostril, are quickly burst, and the little worms crawl up the the nose. In the act of passing up the nose they appear to give dreadful annoyance, for the sheep gallops furiously hither and thither, and snorts violently, and seems almost mad. Having traced their circuitous course, through the aperture under the upper turbinated bone into the maxillary sinus, and thence into the frontal one, the larva fixes itself on the membrane of the sinus by means of two tentacula or hooks which grow from the sides of the mouth, and there it remains feeding on the mucus secreted by this membrane, from June or July to April or May. There are seldom more than three or four of these insects found in any sheep.

The larva having reached and fixed itself on the sinus, no longer annoys the animal: ten months pass, and it produces neither disease nor inconvenience. In this respect it resembles the bot in the stomach of the horse, which, except existing in great numbers, is perfectly harmless. Nature would not have assigned it this residence, if it did mischief there: the permanent comfort or health, or life of the sheep, would not have been sacrificed to so insignificant a being. The stories of nasal gleet, and vertigo, and phrenitis, and death, being occasioned by it, are all fabulous.

It passes the longer but secluded portion of its life harmless in its destined abode; and when it has attained its full growth, and is ready to undergo its next mysterious change, it detaches itself from the sinus, and creeps out through the circuitous course by which it first entered, and again it sadly annoys the sheep, who makes the most violent efforts to dislodge his enemy. The grub having dropped from the nose, burrows into the ground, and assumes the form of an oval and motionless chrysalis, from which in six weeks or two months bursts the perfect fly. It is found more particularly on the sides of copses, and is recognised by the shepherd on the walls and rails near the flock. The fly lives but a short time: it has but one duty to perform, the propagation of its species. The male having accomplished this duty, immediately dies; indeed it takes no food, for it has not the organs by which it can either receive or digest it. The female lingers a little longer until her eggs are ripe, but she too eats not, but, impelled by powerful and unerring instinct, she seeks and persecutes the nearest flock of sheep, and finds, in the manner we have described, a habitation for her offspring.

The Practice of the Spanish Shepherds.—Spanish shepherds are said to use the trephine for it. They attribute various affections of the head to the worm which they are almost sure to find in the frontal sinus; and they materially relieve or remove the complaint, not by the removal of the grub, but by the bleeding which is the necessary consequence of the operation.

LONDON HOSPITAL.

CLINICAL LECTURE

BY

DR. BILLING,

Jan. 14, 1832.

DISEASES OF THE HEART.

I WISH to-day, Gentlemen, to call your attention to the consideration of the symptoms and treatment of several cases of diseased heart which we have on the clinical journal and in the hospital, and shall not occupy any time now with the subject of pathological anatomy, as I have already had opportunities of showing you specimens of diseased structure, and of explaining how they produce the secondary states of disease, of dropsy in the cellular and serous membranes, congestion of the mucous, &c. I shall begin with the worst kind of cases, those in which, though you may keep the patient alive and render life very comfortable, you may not be able entirely to cure them, both on account of the amount of the disease, and the advanced period of life; for the same amount of disease is, of course, more under the control of our art at the earlier age. You will see also in these cases what some of the others are likely to terminate in—that is, those of them which, though of recent date, have irremediable change of structure, and what those which have not, may be saved from by judicious treatment.

CASE I.

Disease of the Heart, with Dropsy—Operation of Hydragogues.

P. M., ætat. 60, labourer. Dec. 8. Admitted in a state of universal dropsy, the abdomen ascitic, and the scrotum, legs, arms, hands, and face, anasarcous; difficulty of breathing and cough most troublesome on lying down; says that he expectorates thick phlegm; rhonchus sibilans and sonorus, and crepitation; pulse at the rate of about 60, but irregular, intermitting, and weak. On auscultation the heart's action weak, but occasionally a single evident impulsion, and extended pulsation into the right side; tongue clean; papillæ white (exsanguine); little appetite; bowels free without medicine; urine scanty; skin soft and warm; sleeps ill. He was ordered

Aceti scillæ ʒi, *ex Aquæ* ℥iss *ter die*; *Elaterii* gr. i, *cum hydrarg. submur.* gr. iij, *alternis auroris. Milk diet.*

The first thing evident in this case was, that the man must soon sink, unless we could relieve him from the load of serous fluid which was infiltrated throughout every part, including the spongy texture of the lungs, and which was the cause of the crepitation; had the crepitation been from inflammation, and not from œdema pulmonum, we should have had frequent pulse, and other feverish symptoms belonging to peripneumony. In a very short time the increase of the dropsy in the lungs would have suffocated him, even if no dropsy existed in the cavities of the pleura or pericardium; and it is an interesting fact, which you will find confirmed by your future experience, that those cavities, the pleura and pericardium, the most essential to be free from dropsical effusion, do always remain so the latest in dropsy, unless some inflammatory state in themselves, gives a disposition to effusion, such as takes place in pleurisy, which is many times the cause of, and precursor of, that kind of hydrothorax which Laennec calls empyema. Though the œdema, or dropsy of the lungs, had been mentioned by one or two previous writers, it was only by Laennec that it was made generally known; the suffocation, orthopnea, and starting from sleep, which it produces, had usually been attributed to hydrothorax.

It is necessary to enter into an explanation of the medicines ordered in this case; we had two indications to fulfil; to get rid of the fluid already effused, and to check the deposition of more. Elaterium affords the quickest means of evacuating dropsical fluid next to paracentesis, and in this instance, as paracentesis could only have emptied the abdomen, elaterium was more applicable to the general anasarca. It carries off the water, by causing a profuse weeping from the capillary exhalants of the mucous membrane of the intestines, to the amount of many quarts per diem; and of course, whilst the exhalant capillaries of the sanguiferous system are throwing off serous fluid, the lymphatic absorbents go on pouring it into the veins, by which the dropsy is carried off, which is the modus operandi of drastic, or, as they are called, hydragogue cathartics and emetics. Elaterium is emetic as well as purgative, but it is specifically hydragogue above all others, and produces little griping uneasiness as long as there is fluid to come away, when that is evacuated it produces irritation; hence it is not a good purgative medicine to use, even where you want a strong one, except in dropsical cases, as it may prove more irritating than effective. It is well for you to learn the value of this remedy in an hospital, as you must feel confidence in it yourselves before you can urge your private patients to persevere with a remedy which is so disagreeable in its operation, and which requires to be repeated many times. If patients will not take it more than two or three times, that most likely is not sufficient to cure the dropsy, and they are dissatisfied at having been disciplined without permanent benefit, for dropsy is not to be cured in a week; but after persevering for ten days or a fortnight, which is often necessary, they will feel the advantage sufficiently to go on without any persuasion.

At the same time that you are evacuating the fluid, you must endeavour to restore the tonic action of the capillary system, so as to check the effusion of more, and we know of no medicine more efficacious for this purpose than mercury, for which reason the calomel was prescribed in this case, as well as to assist in restoring the action of the secreting capillaries of the kidneys, for which purpose calomel is constantly combined with diuretics, as it was in this instance with squill, which is both diuretic and expectorant; and until you can restore the secretion of the kidneys, so as to keep up the natural evacuation of redundant serum, the dropsy will gain upon you again after each dose of elaterium, or after tapping, and especially if the skin be dry, so as not to relieve by insensible perspiration, which was not the case however here. You must make nice discrimination in your diuretics; digitalis, for instance, is often much more powerful than squill, but that is where there is some degree of hardness or force in the pulse, which would be better kept down, and not when there is such a staggering pulse as that of P. M. There are no cases in which sudden death occurs more frequently than these of dropsy with diseased heart, and if you misapply digitalis in them, you may become afraid to use a most efficacious remedy in other instances, when it not only could do no harm, but would be beneficial, merely from want of that discrimination which it is my endeavour to teach. Thus digitalis is useful in some cases of phthisis, in some cases of epilepsy, &c., on account of its sedative, controlling, power over the heart, and totally inapplicable in other cases of the same diseases, when the action of the heart is already weak. You see that you must consider the operation of your remedies on the nervous as well as the vascular system, that the depressing effect is by no means proportionate to the evacuating—of which you cannot have a better comparison than in these two—elaterium and digitalis.

Though this man's pulse was very weak, his skin was of a natural temperature, and there was no appearance of collapse to induce me to administer any stimulant, so that I trusted to relieving the load from the system, and giving him unstimulating, but nourishing food; his tongue was clean, and there was no evidence of defect in the primæ viæ, except want of appetite, and this it was to no purpose to attempt to restore, otherwise than by lightening him. He did not want opening medicine, and I should not have given him any, but that which I knew would bring away watery motions. His cough was one of congestion of the mucous membrane, ascertainable from the rhonchus, and the whole malady originated in enlargement of the heart, evinced by the impulsion and extended pulsation, the latter not perceptible at admission on account of his debility, except at the moments that impulsion also was evident. He slept ill, but I must caution you against narcotics in such a case; the only safe means of procuring rest, is by relieving that congestion of the lungs which causes the starting and uneasiness; if you give a patient in such a state, so much anodyne as to throw him into a state of insensibility, which can scarcely be called sleep, the consequence may be, that the insensibility may make him allow the phlegm to accumulate, so that he will be almost, if not altogether, suffocated when he awakes, making it scarcely safe for a person in that state to have more than short snatches of sleep. The next report is,

13. Feels better; dropsical swelling diminished; cough troublesome, with watery mucous expectoration; pulse and tongue as before. The elaterium produces copious watery motions, with vomiting, on the alternate days; urine more copious; sleeps ill. *Perstet.*

There was here some slight evidence of improvement in the diminution of anasarca; increase of secretion of the kidneys; and the wateriness of the expectoration showed that the lungs were getting off the fluid which loaded the air-cells.

16. Feels better; swellings diminishing; cough much better; still rhonchus and crepitation; pulse as before; urine increased; appetite good. *Full diet; contin. med.*

20. Swellings nearly gone; ditto cough; pulse and tongue as before; appetite good; urine copious; complains of the action of the elaterium.

Capt. tinct. ferri muriatis ♏xx; *ex infusi gent. co. ter die*; *omitt. cætera.*

He did not complain of the action of the elaterium until the necessity for it had ceased, which being the case, I ordered him only a tonic, which has also considerable diuretic properties, to keep up the secretion of the kidneys; and left off all active medicine. On this plan he continued improving. On 27th the pulse was reported more uniform in strength, though still irregular and intermitting.

Jan. 1. Feels better in every respect; no cough; appetite good; no thirst; a very little œdema of the legs; bowels free; urine free; tongue clean; pulse as at last report.

3. Legs rather more swelled; pulse more regular, and not so weak; impulsion and extended pulsation in the chest.

> *Capt. elaterii* gr. i;
> *Calomel.* gr. iij *alt. aur.*; *contin. mist.*

You may observe in this report that the consequence of the return of force in the heart's action, before the capillary system had recovered its tone, was, that an increase of the dropsical effusion took place, upon which I lost no time in resorting to the elaterium and calomel again; these he took on the alternate days until the present time. He has now only a slight fulness below the ankles, which would disappear were he able to take exercise. He has again begun to complain of the elaterium, as the exhalants of the intestines do not like to be troubled by its demands when they have nothing to give. I shall from this time let him have only the sulphate of quinine gr. v ter die, with rest and full diet, until he recovers his strength, so far as he is capable of doing with extensive disease of the heart.

CASE II.

Disease of the Heart, with Acute Inflammatory Affection.

I shall next take a similar case, which you have seen, in which there was less organic disease, but complicated with an acute inflammatory affection, requiring different modification of treatment.

S. L., ætat. 50, labourer, October 29. Has had dropsical swelling of abdomen and legs, cough, and shortness of breath, for 8 weeks, and had previously had palpitations. Pulse irregular, frequent, and rather hard; action of heart irregular and rapid, with impulsion; respiratory murmur dull, crepitation posteriorly and inferiorly in chest, and pain in left side; tongue slightly furred; appetite pretty good; urine scanty; bowels open. He has been bled and purged with relief.

> *Habt. tinct. digitalis* ♏xv;
> *Magnesiæ sulph.* ʒi; *ex mist. scillæ* ℥iss, *ter die;*
> *Pil. hydrargyri* gr. iv;
> *Scillæ rad.* gr. i, *omni nocte;*
> *Empl. cantharidis lateri sinistro thoracis.*

In this patient we had dropsy connected with acute inflammation, and he had fortunately been relieved by antiphlogistic treatment to a certain extent. I must observe to you that the crepitation in the lungs here was equivocal; it might have depended upon œdema pulmonum entirely, as in the former case, but he had pain in the left side, for which the blister was applied, and the hardness of the pulse; and his having been feverish before he was bled, according to his account, evinced inflammation, the complaint of palpitations, and the impulsion of the heart, indicating organic disease,—hypertrophy. Here was a case of dropsy with diseased heart, in which I did think it not merely safe but expedient to administer digitalis, for the purpose of softening the pulse, of which it is one of the most powerful means we possess. The mercury and squill was given to fulfil the same indication as in the former case, and the salts to keep up the action of the bowels, by which means I thought the dropsy might be reduced, as well as the inflammation, without elaterium.

On November 1, his gums became sore, and his pulse softer, but he was not relieved of any of his symptoms. I therefore resorted to elaterium every other morning, left off the mercury, there being evidence of its sufficient action on the system, and continued the mixture, with digitalis, &c. to keep down the pulse. He continued on this plan for a month, gradually improving; the pulse became steadier but never perfectly regular. The dropsical swellings disappeared, and his cough declined, but did not entirely cease until the beginning of December; up to which time he had crepitation in the inferior part of the left side. This must have been the rhonchus crepitans redux, subsequent to peripneumony, for all œdematous crepitation, as well as his dropsical swelling, had ceased by the 15th November. I let him continue the elaterium notwithstanding, as the mixture, even with the magnesiæ sulph., had no effect on the bowels. On the 7th December he felt quite well, and was dismissed from the hospital. You may probably, however, ere long see either of these patients back again, as with the permanent organic disease of the heart which they have, they are likely to have return of these symptoms, unless they take more care of themselves than they are perhaps equal to in their station of life, as has been the case with the woman A. S., at present in Charlotte's ward, now for the third or fourth time. She has been twice in limine mortis, has been tapped several times, and though she has diseased liver, as well as enlarged heart, she rallies, so as to be able to return home and take care of a large family. The treatment has been similar to that in the last two cases, only that she has been sometimes so sunk, that we have kept her alive for days, merely with wine and brandy.

CASE III.

Hypertrophy of the Heart, with Severe Diarrhœa.

The next is a case of incipient disease, and in a younger person, and in which we have every prospect of effecting a permanent cure, and even the reduction in size of a heart, after some degree of enlargement had taken place.

T. M., ætat. 22, a sailor, November 21. Complains of cough with expectoration, and diarrhœa; of debility, and that he is getting thinner; has been ill five or six weeks, having caught cold from exposure to wet; he was attacked with shivering, cough, and diarrhœa. Pulse 110, full and strong; rhonchus sibilans in different parts of the chest; there is impulsion of the heart, with extended pulsation. He feels the pulsation sometimes disagreeably; no pain in the chest; tongue clean.

> *Capt. tinct. digitalis,* ♏xv; *ex emuls. ol. cum liq. antim. tart.* ʒss *ter die;*
> *Lohock, ext. hyosciami* gr. x *omni nocte.*
> *Baln. calid. emplast. cantharid. inter scapulas. Milk diet.*

At first sight I had a suspicion from the cough, expectoration, and wasting, that this man was phthisical; but the wasting was attributable to the diarrhœa, which was a catarrhal state of the mucous membrane of the bowels, coeval with the catarrh of the chest, both produced by cold and wet, and the cough was no doubt aggravated by the hypertrophy of the heart. Had the patient merely laboured under catarrh of the mucous membranes, with a moderate pulse, Dover's powder, or small doses of tincture of opium, with infusion of catechu, might have allayed it; but the frequency and hardness of the pulse, and the hypertrophy of the heart, called for sedative and antiphlogistic treatment,—hyosciamus as a sedative narcotic; digitalis, to give a tendency to the muscles of the heart to diminish by diminution of action; the emulsion and lohock (or linctus) to soothe the membranes; the warm-bath to promote the exhalation of the skin, and diminish the internal congestion and irritation *pro tempore*; the blister frequently has a better effect applied between the shoulders, when there is a diffused irritation in the chest; when there is defined local pain, it is better to apply it at that place. Under this treatment, on the 29th he had very little cough, diarrhœa had ceased, appetite improved; pulse 96, soft, *Contin. med. full diet.* On the 13th December he complained of sensation of debility, and the hyosciamus was omitted. On the 20th he had a sharp attack of rheumatism, generally with pain also in the region of the heart, for which he was cupped to ℥xij over the heart, and his medicine was changed to 1 gr. *calomel every night,* and ʒj *acetat. colchici ex aquæ* ℥j *ter die,* with *milk diet,* which removed the rheumatism. On the 27th there was some dyspnœa, with a hard pulse, 108, and confined bowels; he was cupped again over the heart and blistered, and had ʒj *magnesiæ sulph.* in each dose of the mixture, by which he was quite relieved, but his pulse was not reduced, and on the 3rd instant I ordered him *tinct. digitalis* ♏xv *ter die,* to be added to the mixture. His pulse now is about 80 and soft, but easily quickened by exertion, and though he feels well, there is still impulsion; and ascending the stairs produces dyspnœa. I hope, by keeping him quiet, and under the influence of digitalis, to conquer this affection, and one circumstance greatly in our favour is, that the digitalis does not disagree with either his head or stomach, nor produce mental depression, as it often does, so as to prevent the continued use of it.

CASE IV.

Enlargement of the Heart produced by Acute Rheumatism.

The next is an example of those too common cases of enlargement of the heart, produced by acute rheumatism attacking the organ, and producing carditis or pericarditis, from the irritation of which, continued over-action, and consequent enlargement, ensue. This man, J. W., æt. 20; a cabinet-maker, was admitted 8th December last, in a state of acute or rather subacute rheumatism, with pain and swelling of the joints; pain and beating in the chest; pulse 100, throbbing, impulsion, extended pulsation, and bruit de soufflet of the heart, having been labouring under these symptoms for two months in consequence of exposure to wet. It is unnecessary to enter into the daily minutiæ of the case, the treatment having been in many respects similar to the last; he has been bled from the arm and cupped over the heart; he has had the saline antimonial mixture with colchicum, and also been subjected to the influence of mercury and digitalis. You must have observed that his appearance is very different from that of J. M., who is thin and slightly formed; this patient is of a very plethoric habit, and gets better when reduced, but as soon as he begins to recover his colour, he has a renewal of rheumatic pain, either in the limbs or heart. These relapses have occurred twice since his admission; he has scarcely ever been free from the tendency to sweating peculiar to acute rheumatism. I have not yet given him any bark or quinine, which is so often quickly efficacious in these rheumatic cases, as recommended by Haygarth, but you had an opportunity of seeing that another tonic, *carbonate of iron*, ʒj *ter die*, relieved his pains; he had more pains in the limbs whilst under the influence of mercury, and the heart was much better, partly owing, I consider, to the digitalis which he was taking at the same time. Under these circumstances I gave him on 6th inst. *ol. terebinth.* ʒj, *ex inf. lini ter die*,—another medicine efficacious in rheumatism, and he was yesterday much better, but from his tendency to plethora, and his still having occasionally pain in the heart, I was induced to order him to be cupped again over the heart. The turpentine has produced ardor urinæ, as frequently happens, but it is not of much consequence, as it generally subsides, or is checked by adding a small quantity of opium; as I do not wish to give this patient opium, however, if this irritation continue, I shall resort to colchicum again, with quinine, and keep him at rest, if possible; but it is difficult to confine him to his ward when he feels a little better. He has youth and a good constitution in his favour; but there is much greater enlargement of the heart than in the former case.

ON VACCINATION
AS A
PREVENTIVE AND MODIFIER OF SMALL-POX,
AND ON THE NATURE AND EXTENT OF ITS PROTECTING INFLUENCE.

With Observations on the Varieties, Forms, and Degrees of Severity, assumed by modified Small-Pox.

By Robert Venables, M.B.

Whether cow-pox be a preventive of small-pox is a question which has agitated and still continues to agitate, the medical world; and it is one also upon which there is great confliction of professional opinions. One party maintain that cow-pox is no preservative at all, and that the practice of vaccination, so far from being a blessing, has proved a curse to mankind, by limiting or wholly suppressing those conservative measures which preclude the introduction of the casual disease. Another party as firmly assert, that cow-pox is a complete antidote to small-pox; the latter disease never appearing in the constitution which has been once fortified by the protecting influence of the former. This party assert, that all the instances recorded of small-pox attacking persons previously vaccinated, are but instances of small-pox succeeding a spurious form of cow-pox, the genuine disease never having been established. Perhaps between the extremes we shall find the truth. "Medio tutissimus ibis" is a rule which might serve to guide others as well as the unfortunate Phaëton. Accordingly, I believe it is now admitted, even in the highest quarters, that small-pox does appear after the vaccine disease, even in its most true, genuine, and characteristic form; but then the former, so mild and so modified as scarcely to deserve the name of a severe, much less a formidable disease. This appears to be the state of medical opinion upon one of the most important questions, that, practically considered, can engage the attention of the profession.

Some years since, I detailed the particulars of a case of modified small-pox after vaccination. The case is related in one of the numbers of the Medical and Physical Journal of 1822 or 1823. One or two professional friends, who did me the favour to examine this case, would regard it in no other light than as a rather severe case of varicella or chicken-pox; nor would they give up the point till the appearance of the casual disease in the confluent form in unprotected constitutions, legitimately traceable to communication with the case in dispute, convinced them of their error. Two conclusions appeared to me fairly deducible from the foregoing facts. 1st. Vaccination, when successful, is a modifier of small-pox. 2ndly. The modified disease will produce the natural one in susceptible habits exposed to its influence, and this also in its most severe or even malignant form.

I have upon all occasions availed myself of every opportunity which has presented of investigating the nature of small-pox; its modes of propagation, and the varieties and degrees of conservative influence afforded by vaccination; the laws by which they are regulated, and the rules for their estimation. The opportunities which presented in the garrison at Woolwich were invaluable, whether regarded as to their extent or the means of observation and facility of inquiry. Previously, however, to entering upon the general result, I shall detail the particulars of a few cases as they occurred in my own family, and consequently subject to my constant inspection and more immediate superintendence.

In the autumn of 1817, I vaccinated my eldest girl, then about two or three months old, with lymph procured at the Small-pox Hospital. The disease went through a regular course, and was satisfactory in every point. Four others were subsequently vaccinated by myself, at intervals after birth, varying from six weeks to two or three months. I felt perfectly satisfied with the general appearance and progress of the vaccine vesicles, and likewise the constitutional irritation excited by the disease, in every instance, with the exception of the last of the five. In this case the progress of the vesicle presented some slight irregularities, hardly perceptible unless to very inquisitive observation, and there was no constitutional irritation excited. These two facts induced me to look upon the constitution, in this instance, as unprotected, and still susceptible of the variolous influence. I, therefore, made several subsequent attempts to excite a more genuine form of the vaccine disease; but all my attempts were futile—they regularly failed. Some professional friends, when informed of the above particulars, and of my solicitude, were inclined to look upon my apprehensions as unfounded, and my fears as idle; the more especially as the several attempts to reinduce any form of the disease proved unsuccessful. They, therefore, looked upon the constitution as fortified and wholly unsusceptible of either the vaccine or variolous action. Sometime after, however, I had proof of the correctness of my own views in the appearance of small-pox most characteristically and unequivocally marked in this patient. In the spring of 1829, the small-pox prevailed both extensively and fatally at Chelmsford. The child, whose case is now the subject of consideration, slept in a small cot in my own bed-room; and I was one night, at this period, suddenly awoke by his screaming out most violently in his sleep. I immediately rose, took him out of bed, but could not succeed in restoring him to his natural recollection. The eyes indeed opened, but with a most vacant and terror-stricken stare. The screaming and fright continued for upwards of a quarter of an hour, when at length he became calm and tranquil; and on being put to bed again, went to sleep. In the morning, however, he awoke very feverish, with redness and watering of the eyes, sneezing, running of the nose, cough, suffusion of the skin, and all the symptoms of impending exanthema. The suffusion of countenance very speedily assumed that erysipelatous character, with, to close observation, a slightly elevated or pimply appearance, which I have found to be an unequivocal diagnostic of the nature of the rash, and an unfailing variolous premonitory. The small-pox eruption speedily followed; pustules formed, filled, and maturated, and the disease went through its regular course with the exception of the secondary fever, which did not supervene the period of declination. Some few pits under the seat of some of the severer pustules unequivocally attest, even at the present moment, the nature of the disease.

At the time that the small-pox attacked this child, there was an infant brother in the house, who had not then been vaccinated. However, I was lucky enough to procure some lymph, and insert it with success in the arm of the infant, the true vaccine disease supervening. The four elder children which had been previously vaccinated, were allowed a free and unrestricted intercourse with the child then labouring under small-pox, but not one of them suffered in the slightest degree, not even the infant (the intercourse with it, however, was completely interdicted), nor was the progress of the vaccine disease in the slightest degree interrupted. Here, then, appears to have been a total unsusceptibility, completely proof against the influence of the variolous contagion, engendered by vaccination; for although freely exposed, every one of those *effectually* vaccinated, hitherto resisted the variolous influence.

However, about three weeks since, the eldest but one of the vaccinated children was taken ill with cough, followed by febrile heat, watery suffused eyes, and general ill health. This state lasted for three or four days, when the erysipelatous rash already noted as characteristic of variola appeared. The whole face became slightly but evidently tumefied. Very small pimples, with a kind of vesicular summit, made their appearance. This state continued for between three and four days, that is, from the first appearance of the rash, till the commencement of its decline. I was in daily expectation in this first case of seeing the vesicles enlarge, fill, and maturate like variolous pustules. The cough all this time continued severe. However, about the fourth day of the rash, the seventh or eighth of the disease, the vesicular pimples disappeared, the countenance remaining somewhat swelled, the appearance of the eruption being exactly similar to that after the throwing off of the scabs on the decline of small-pox. In about eleven or twelve days the complaint had completely vanished, leaving no trace whatever of the nature of the disease, nor indeed any manifestation whatever of previous disorder, with the exception of a little weakness, from which this patient has since completely recovered.

It may, perhaps, be doubted, that the above was a true case of small-pox. I must confess, that had I not witnessed two or three analogous cases before, I should have been inclined to question the correctness of such an opinion. I have very little doubt, however, that, judging from the first appearance of the rash or efflorescence,—for it matters little by what name it is called,—and its subsequent or daily progress, many and even accurate observers would have regarded it as a case of *repelled* small-pox, and most probably would have adopted the measures usually had recourse to in such a contingency. But that no doubt may exist upon the question, I must state, that every one of the other four vaccinated children, including the child which I vaccinated during the appearance of the small-pox in my house at Chelmsford, was seized in succession with an eruptive febricular disease of precisely the same appearance and identical nosological character. The only one which escaped was the little boy who had the small-pox after imperfect vaccination at Chelmsford. He was fortified against small-pox, by having previously had the disease, and consequently was unsusceptible. Now had this been a modification of any other of the exanthemata, he must have suffered equally with the rest, for he has not had measles, scarlet fever, or any of the diseases of the above order. Had it been any peculiarity of febrile efflorescence arising from atmospherical influence, or any other cause, as locality, &c., the variolous patient could not have escaped, for he must have been equally susceptible, and he was equally exposed, for there was no restriction even of intercourse, on the contrary, he slept with his brothers from the commencement all through the progress to the termination of the disease; yet he and an infant (with the latter of whom however, all suspicious communication was immediately suspended) alone escaped. This fact goes far to establish the variolous identity, and, taken with the general circumstances above stated, leave, to my apprehension at least, but very little doubt upon the subject.

Allowing the correctness of the above statements, it may be inquired, Do the facts themselves lead to any conclusions of real practical utility? I think this question may be very readily answered in the affirmative. In the first place we learn that vaccination, if successful, is probably a complete antidote—even a permanent antidote to small-pox. 2dly. We learn that vaccination is not only a modifier of the progress and phenomena, but even a very powerful mitigator of the severity of small-pox; and these modifying and mitigating powers seem to be possessed in various degrees even by the spurious forms of cow-pox, the power increasing as it approaches in character to the genuine disease. These conclusions I think legitimately deducible from the history of the foregoing cases, independent of many other corroborating facts which I could adduce, if time and space would permit.

As the genuine vaccine disease seems wholly to disarm small-pox, not only of its virulence, but very nearly of its nosological or distinctive characters, it may be an object of some moment to determine what are the circumstances which tend to impede the success of vaccination, and give rise to a spurious, rather than the genuine form. Cow-pox is, if I may be allowed the expression, one of the most delicate morbid actions of which the animal economy is susceptible. Therefore it is the most easily suppressed, suspended, or perverted. Any one of these may be sufficient to modify or destroy the protecting influence. How, then, are we to obviate such inconveniences? The most effectual method is vaccination in a state of positive health, that is free from disorder of any kind, constitutional or local, but of increased susceptibility. The first few days of infancy, generally speaking, present such a state. The economy, then, is seldom deranged by artificial agencies; and if any derangement then exist, it must depend upon natural causes, over which probably we cannot exercise any control. However, as the fears of parents, the conflicting opinions of practitioners, and numerous other circumstances, all concur in preventing the institution of vaccination at this period, it may be well to state what are the circumstances which dissuade from the practice. These are, so far as my observation extends, all general or constitutional disorders, more especially if attended with any degree of febrile excitement. On this account the severity of teething, and all gastric and enteric derangements, which latter, in infancy especially, are attended with considerable constitutional irritation, are unfavourable to vaccination, as they frequently wholly prevent, and at all events tend greatly to pervert, the regular progress of the disorder. Hence, in such circumstances, vaccination should be deferred, instituting more rigid precautions against the casual introduction of small-pox. It is, I have good reason for believing, an unfounded notion, that "vaccination may be attempted under unpromising circumstances; as in the event of success it is well, but in case of failure, the practice may be repeated with equal or better chances of success." The truth of the latter part is in my mind very questionable. A spurious form of the disease may be engendered, which, though by no means a perfect safeguard against small-pox, yet generally renders the system proof against all future attempts at perfect vaccination. I have repeatedly had occasion to observe the formation of spurious vesicles, and the failure, in such circumstances, of all future attempts at vaccination, till at length the patient suffered an attack of small-pox. All eruptive disorders, and eruptions, rashes, &c., of the skin, whether chronic or acute, are unfavourable to vaccination, and therefore the operation in such cases should be deferred, with the necessary precautions, till such impediments have been removed.

It has been, and indeed often is asserted, that there are no preparative measures necessary previous to vaccination. It is a very common, but I believe very unfounded opinion, that vaccination may be effectually practised at lengthened intervals—seven years in the same individual. I believe, like small-pox, the rule is that true cow-pox is excitable but once in the course of life. The other is an unfounded, or at least a very questionable dogma. Preparation is, in many instances, essential to the success of vaccination, and perhaps would be but a prudent precaution in the majority; for, by increasing the susceptibility of the constitution, it will increase the probability of success, and so far diminish the chances of engendering a spurious form of the disease. I need not here enlarge upon the preparative means; they consist in the removal of all sorts and sources of irritations, in the reduction of those states of the system which are known to prevent, and the institution of those means which favour the absorption of external agents into the blood.

Another assertion, and it is one upon which much is urged in favour of vaccination, is, that it is not attended with any constitutional derangement; and should any such effect follow, it is to be looked upon as an anomaly, or, at all events, but an exception to the general rule. Not only are the facts themselves false, but the deductions unfounded. In the great majority of cases there is a slight, but, to careful observation, still sensible degree of constitutional excitement. In 217 cases which I vaccinated at Woolwich in 1811-12, and of which I took notes, there was marked constitutional disturbance in 105,* slight but still sensible in 84, equivocal in 15, and no constitutional manifestation whatever in the remaining 13. The general result of my observation is, that constitutional symptoms, however slight the degree, are essential to the protecting influence; and I should prefer an irregular vesicle with the true constitutional vaccine disturbance, to a regular vesicle without any.

From the above facts, confirmed by extensive observation, I feel that I am warranted in the following conclusion:—Vaccination, to prove successful, should be practised at as early a period of life as possible. At this period the constitutional susceptibilities are, as it were, at a maximum; and there is less of that deviation from pure health which certainly tends to pervert or disturb the progress of so delicate a disease as cow-pox.

It may now possibly be asked, why the five persons whose cases I have just detailed, resisted the contagion of small-pox about three years ago when domiciled among them as it were, yet yielded themselves its victims so readily on the present occasion. Of this I admit I can offer no satisfactory explanation. But still I must assert there is no greater difficulty in admitting the fact, than that the individual who, though freely exposed, resisted for years the contagion of typhus, measles, and even the plague, yet fell a victim at a period, and under circumstances which precluded all anticipation of such an event, but would have led us rather to infer a complete immunity. Therefore, till we can solve this latter difficulty, we cannot fairly attach much importance to the former.

Now, in conclusion, I shall make one or two observations upon the variolous modifications. Of these, the disease certainly seems susceptible to a great extent, both in degree and variety. The principal modification seems to be that from genuine cow-pox, in which the eruptive fever is but slight—the eruption consisting in hardly anything more than the cutaneous efflorescence, surmounted by very small vesicular-looking summits, which die away in between three or four days without maturating or assuming the pustular character. In this modification there is neither "pitting" nor "scabbing," but often an efflorescence, or traces of its remains on the face, for eight or ten days, or even a fortnight. There is no desquamation of the cuticle, and all febrile excitement ceases on the fifth or sixth day. A patient in this disease will communicate the complaint to susceptible persons in the usual forms.

The next modification is that which arises from the less spurious forms of cow-pox. It varies principally in the degree of severity. In it the disease goes through the regular course, from the eruptive fever to the maturation of the pustules; on the decline, however, there is no further danger. Sometimes the pustules, though of the regular size, retain a lymphy or milky appearance, never assuming the true purulent appearance. Sometimes they resemble the crystalline variety, but, generally speaking, they are all devoid of danger. I have never observed secondary fever, but there is usually pitting of greater or less intensity.

There is one other remark with which I shall conclude this paper. Individuals not protected by previous vaccination or small-pox, are occasionally attacked after being exposed with an efflorescence of the skin like that which attends the appearance of small-pox, a degree even of pustular appearance succeeding, partaking entirely of the characters of variola, but without any constitutional symptoms whatever. Such persons are apt to believe themselves secure against any future attacks of the disease. This however is but a false security. I have seen seven very clearly-marked and well-established instances of failure under such circumstances. In truth we may say of the "variola sine febre," as of the "rubeola sine catarrho," it affords no security; and as, in the latter case, the individual is still liable to measles, so in the former he generally suffers at some subsequent period from small-pox.

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* The younger the patient, generally speaking, the more marked the symptoms.

7, Suffolk Place, Hackney Road,
June 5, 1832.

LITHOTRIPSY.

To the Editor of The Lancet.

Sir,—I beg to inclose an interesting case of stone in the bladder, transmitted for publication by Sir Richard Dobson, M.D., Surgeon of Greenwich Hospital. The case was treated by Baron Heurteloup, and affords another example of the efficiency of the new instrument, the "percuteur courbe à marteau." The judicious medical treatment adopted by Sir Richard is also an attractive feature in the case. I am, Sir, your obedient servant,

D. O. Edwards.
Westminster Hospital, June 11, 1832.

CASE.

(*Drawn up by Sir* R. Dobson.)

Greenwich Hospital, June 5th, 1832.

At the examination of invalids at the Admiralty on the 19th of April, Charles Sellois, a private marine, aged 23 years, presented himself, having been invalided for stone in the bladder, at Haslar Hospital, where he had refused to submit to the operation of lithotomy proposed to be performed upon him by Dr. Mortimer, the able surgeon of that institution. Finding that though his dread of being cut was insurmountable, he had no objection to lithotrity, (for he had heard that there was a method by which the stone could be got out of the bladder without cutting,) I proposed to Sir J. Pechell, Bart., (one of the Lords of the Admiralty superintending the examination,) that the patient should be sent to Greenwich Hospital, for the purpose of being operated upon by Baron Heurteloup, who had so successfully broken up a stone in the bladder of one of our pensioners in the year 1830, which person has continued ever since quite free from disease. This proposition being readily acceded to, I wrote to the Baron, whose other engagements did not permit him to fix an earlier time than the 5th of May for the operation of lithotripsy; on which day an aperient lavement was administered to the patient at noon, and at half-past two o'clock the Baron operated, using the instrument called *Percuteur courbe à marteau*, a description of which was given in the 456th Number of The Lancet.

In the operation of lithotripsy the patient is placed on the *lit rectangle*, the same as used for lithotrity; and the bladder having been first injected full of warm water, the Baron introduced the instrument, with which he seized the stone in an instant, and with a few smart strokes of the hammer broke it in pieces; some of which fragments he subsequently seized, and in like manner smashed them. The whole of this was done in three or four minutes, without any apparent pain to the patient, who was cheerful the whole of the time, and who said that the only uneasiness he felt was a great desire to make water, which he did the moment the instrument was withdrawn, and a considerable quantity of detritus came away with the urine.

6th. Portions of detritus have been passing during the night, to get rid of which he had very frequent desire to make water, and on that account got little sleep. The water is slightly tinged with blood. Fearing lest some particles might be irritating the neck of the bladder, a sound was introduced, which gave him no pain. A large piece of calculus was felt near the prostate, and as soon as the sound was withdrawn several fragments were dischaıged with the urine.

7th. Has slept well; some soreness in the urethra; sound again passed; a fragment large and rough found close to the neck of the bladder; gave him an aperient, which operated well; in the evening was hot and feverish, with some headach and thirst. *Haust. anod.*

8th. Has passed several pieces of detritus in the night, the urine is high-coloured, and deposits a mucous sediment. This being the day agreed upon with the Baron for operating again, the patient had a glyster at noon; but when the Baron came down at three o'clock, we were of opinion that it would be better to defer the operation, as the patient was complaining of nausea, and being still feverish, he was ordered a hip-bath, and bled to 16 ounces.

9th. He perspired freely after the hip-bath, but being still feverish, and having had some vomiting, he was ordered effervescing saline draughts, with an anodyne at night.

10th. Has slept very well; bowels have been well opened, but he is still slightly sick at stomach; he has no pain at all in the bladder or urethra, the sound dropping easily into the bladder without causing any. The large fragment was felt near the fundus of the bladder. Continue the saline and anodyne draughts. He passed a large fragment to-day.

11. Had a very good night, but still suffers from sickness of the stomach; was formerly in hospital for gastritis, and fearing inflammation of that viscus, which is certainly more affected than the bladder, he was bled from the arm to twelve ounces, and thirty leeches were applied to the gastric region.

12. In every respect better; no soreness about the pubis or perineum, nor does he suffer the least pain by the introduction of the sound; bowels well open.

13. Has had several hours sound sleep, and is quite free from pain, but has vomited a little; the urine is quite clear, and the patient feels hungry.

14. Continues to improve.

15. He complains of headach with noise in his ears; and (as he expresses himself) "a sound all over him," for which a dozen of leeches were applied behind his ears; bowels quite open.

16. Noise gone; much better.

17. Feels very well.

18. He had a glyster at noon; and at three o'clock the Baron operated again as before, seizing the stone without the least loss of time. After breaking it, he lowered the patient's shoulders, in order the more readily to seize any of the smaller fragments still requiring further reduction. After this was done, which did not last longer than the former operation, some more warm water was injected into the bladder, which brought away sufficient proof of the calculus having been further comminuted. The patient had not the least suffering after this operation, which was repeated again on the 24th, with the same facility; and by this third application of the instrument, the stone was so completely crushed that every grain of it has come away, and the man is perfectly cured.

Each of these operations was performed in the presence of thirty or forty medical gentlemen, every one of whom expressed his surprise and delight at the Baron's dexterity; and at the little (if any) pain which the patient suffered.

The slight fever which followed the first operation was ascribable more to the stomach than to the urinary organs, and might probably have been obviated if the patient, who is of a plethoric habit, had been depleted before. This supposition is strengthened by the consideration that no febrile symptom or inconvenience whatever, followed either of the other operations, which took place after the patient had been well depleted and reduced; and this may serve as a hint on future occasions. It must be admitted, however, that when a stone undergoes Macadamization in the bladder, it is impossible for the operator to tell into how many fragments it may break, or what may be the shape and size of each. These from their roughness or sharpness will necessarily produce some inconvenience to the bladder, and in this man's case no doubt caused the frequent desire of making water, which he experienced after the first operation. However, nobody can doubt the great advantage which lithotripsy has over any other mode of operation. The result of the case is the best recommendation of the practice, and the more it is known, the better for the public, as I feel satisfied that it will ultimately render lithotomy an operation of rare occurrence; and if early recurred to by patients afflicted with the stone, will afford them a safe and easy escape from their great and increasing distress.

R. Dobson, M.D.
Surgeon of Greenwich Hospital.

CASE OF

OSTEO SARCOMA OF THE UPPER MAXILLA,

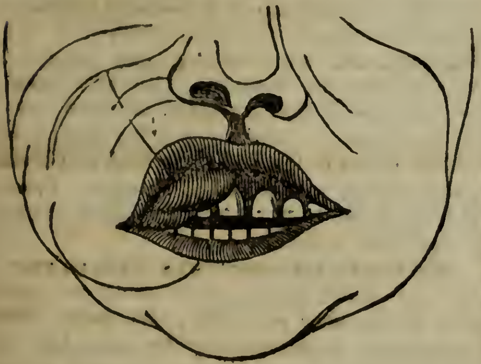
WITH THE OPERATION FOR THE

REMOVAL OF THE PART AFFECTED.

By E. L. Bryan, Esq., *Surgeon, Stowmarket, Suffolk.*

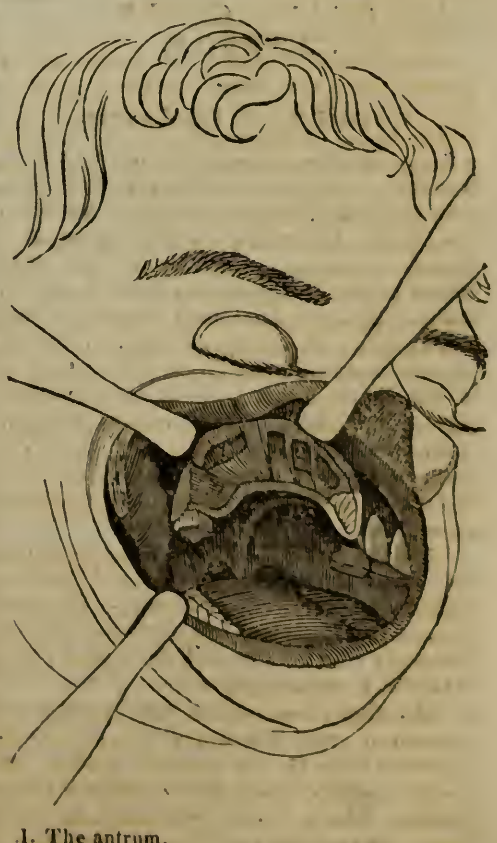
The subject of the disease, Philip Clarke, ætat. 8, was brought to me on the 4th of May last. His parents described the disease as making its appearance about seven or eight months before, in the form of a blister, on the inside of the gum, in the neighbourhood of a decayed tooth. The tumour did not increase much in this direction, but soon appeared on the outer side, and has continued steadily to enlarge, for the last four or five months, under the application of lotions, &c.,—lately, as the mother informs me, an escharotic having been tried. When I first saw the boy, it was of considerable size, covering the alveolar process from the first incisor to the last molar tooth, overlapping the teeth of the inferior jaw, distending the cheek, and opening the lips, between which it projected. Superiorly, it extended towards the nasal process and orbit, to about the level of the infra orbitar foramen, consisting, at this part, of thickened and partially ossified periosteum, distended with (as was afterwards found) a mottled curdy substance of the consistence of brain. In some parts it communicated to the touch the sensation of fluctuation, in others that of carcinoma, and from the surface of the latter an offensive sanies exuded. Considerable bleeding had occasionally taken place. Three teeth of the side affected had fallen out; the others were loose, except the last molar. The child had occasionally experienced in the tumour considerable pain, which he called toothach: his health had begun to suffer materially, but the glands in the neighbourhood were not affected, nor had the complexion that unhealthy aspect which usually accompanies malignant fungus. The parents were anxious that I should operate, or relieve them from their painful uncertainty, by saying nothing could be done for the child. Under these circumstances, and my opinion that there was a tolerable chance of success, being strengthened by that of several professional friends, I determined on the operation, and performed it on the 10th in their presence.

The following is a sketch of the tumour



Operation.—I commenced by making an incision through the gum between the two molares, and along the palate to the mystacial suture, in a slight curve outwards: considerable hemorrhage ensued from the divided branches of the palatine artery, which however was immediately stopped by pressure with the finger. I then passed a bistoury through the cheek from within outwards, dividing the lip rather nearer the angle than the middle of the mouth, in a direction upwards and outwards, just avoiding the orifice of the parotid duct. Having secured the arteries of the lip, I dissected the cheek from the tumour, and made an incision around it, externally commencing and ending opposite to that on the inner side. I then sawed through the alveolar process, commencing in front close to the mesial line, and following the external and internal lines of incision round the tumour. I had hoped to have avoided the enamelled parts of the secondary teeth, and even some of the fangs, by keeping close to the junction of the internal lamellæ of the palatine and nasal processes of the maxilla, but having sawn through the fang of the new second incisor, the enamel of the next tooth was met with. The saw was then changed for a pair of cutting forceps, which embraced the tumour, and in two incisions separated it, one through the alveolar process, between the two molares, as was first intended, the other opposite the canine tooth; the intervening space was easily cut through with a scalpel. There remained a diseased and rough surface, with the teeth projecting: these were extracted, and the bone sawn through in the direction first intended, in a curve from the first incisor to the last molar tooth, opening the antrum at its anterior inferior part. Contrary to expectation, it was found to be healthy internally; its lining membrane remained entire. The whole of the diseased parts were now removed, the remaining bone presenting a smooth, and, to all appearance, healthy surface. No hemorrhage followed the cessation of pressure which had been kept up during the operation upon the palatine, buccal, and infra orbitar arteries.

The sketch beneath will give some idea of the curved line in which I divided the bone.



1. The antrum.
2. Parts of the sockets of the second set of teeth.

On examining the tumour, the disease appeared to have commenced in the periosteum of the cell containing the second half-formed bicuspid tooth. From it slight ligamentous bands extended into the tumour. It is now nearly a month since the operation; the external wound, to which three sutures were applied, united by the first intention: no exfoliation of bone has taken place from the internal wound, which is nearly healed, and presents no appearance of fungoid growth. The constitutional excitement, and the pain following the operation, were slight, and the boy's health and appearance are much improved.

I was enabled to divide the bone in the curved line shown in the sketch, by using a fine, tapering saw, broader at the teeth than at the back; of that kind called by carpenters the keyhole saw. I am not aware that it has before been introduced into surgery, and think it will occasionally be found very useful: the adult jaw might with facility be sawn through in the direction in which I met with so much opposition from the second set of teeth.

CASE OF

MALIGNANT CHOLERA AT CAWSTON.

To the Editor of The Lancet.

Sir,—In the little town of Cawston, situated within eleven miles of the poor, crowded, and closely-built city of Norwich, the cholera has made its appearance in a formidable shape, while in the last-mentioned place not a case has been heard of, and the disease has not been seen any-where else in this part of the country. Cawston presents nothing peculiar in its locality or in the habits of its people, amounting to eleven hundred. It is a retired village, apart from any high road. Mr. Grey reports 37 cases of cholera and 104 of diarrhœa with sickness, as having occurred among this number of inhabitants between the 1st and 10th of June. I saw two of the first description before leaving Cawston, which was likely to become such. As some interest may attach to the appearance of the disease in this isolated situation, after passing over, if we may so speak, places that might rather have invited and better have fostered it; and lest any doubt may exist upon the subject, I beg leave to offer the following case, being one to which I was a witness, which I am authorised by Mr. Grey, the resident surgeon, to draw up as follows:—

CASE.

Dewing, a weaver, aged 36, earning two shillings per week, upon which he and his wife subsisted, was attacked at six o'clock on Saturday, June 9, with cramps, vomiting, and purging. He is now, at seven, covered with a copious perspiration, which turns litmus paper red; surface moderately warm on the central parts; extremities, tips of the ears, nose, and lips, cold and livid; skin on the fingers and toes shrivelled; tongue moderately warm, white, and flabby-looking; countenance and manner anxious and appealing; pulse 90, small and feeble; abdomen drawn in; some tenderness upon pressure. In the morning he passed eight watery dejections resembling whey, with flocculi suspended in them. Since six o'clock, the time when he was seized with cramps, he has had no evacuation from the rectum, but the bladder has been once or twice emptied. Seen at ten o'clock after a bleeding of 10 ounces. Coldness and debility extending from extremities upwards; the abdomen only is now warm; no pulsation at wrist; the heart is heard to act sharply but feebly, without any impulse; breathing, not laboured; cramps and other symptoms aggravated, with the exception of the vomiting and purging, which have not returned. At 12 o'clock, the collapse is making rapid advance; extreme distress; constant call for drink; is rolling about in the bed; the legs and arms affected with succession of cramps. From this state he rapidly sunk, and died at one o'clock this morning.

Mr. Grey gave him at six o'clock an emetic, and afterwards a grain of opium, and five of camphor repeated every hour. At 12 o'clock, in attempting to give him a suppository of camphor and opium, upon opening the sphincter he had a copious dejection of the character described.

Inspectio cadaveris ten hours after death.

Eyes sunken; features drawn in; face blue; and whole body covered with livid patches. On opening the chest, the lungs were firmly adherent to the pleura costalis; on dividing the adhesions the lungs collapsed remarkably, the left not feeling thicker than the finger; the lower and back part of the right slightly congested. With the exception of this spot, both crepitated freely throughout. The heart, of its usual size, was pale and flabby at its apex and forepart; the back or under surface was dark-coloured, its small vessels filled with black blood; the coronary veins distended. The left ventricle contained no blood; its walls dark-coloured, soft, and of the usual thickness. The right ventricle filled with dark pitchy blood; the auriculo-ventricular orifice dilated; the right auricle distended.

Upon opening the abdomen the omentum presented a singular appearance, the veins being distended with black blood, felt as firm as nerves; no appearance of fat; nothing remaining but the serous layers and the vessels; stomach and intestines filled throughout with fluid resembling rice-water with curd-like flocculi. The stomach was studded internally with bright red patches, not elevated above the surface; the same appearance in the upper part of the duodenum; the rest of the canal without any internal lesion, was dark on its external surface. The liver dark from congestion of its veins, with black pitchy blood; the gall-bladder full of greenish black fluid; the kidneys rather smaller, softer, and darker coloured; the bladder not larger than a very small apple.

Another case remained under treatment when I left Cawston. The patient had repeated dejections and discharges from the stomach of the rice-water appearance. The latter seemed to be relieved by the saline doses of Dr. Stevens. Of the result, Mr. Grey has kindly promised to inform me; and, if agreeable, I shall communicate it to The Lancet. I have the honour to be, Sir,

Your obedient servant,
William Travers Cox, M.D.
Great Yarmouth, June 11th.

THE LANCET.

London, Saturday, June 16, 1832.

Several subjects of deep importance press upon us for consideration at this juncture; but there is one which claims attention incomparably beyond all others, namely, the conduct to be pursued by the medical practitioners of England, Ireland, and Scotland, at the coming election—that election which is to return, for the first time in the *modern* period of our history, delegates who will speak the sentiments and echo the opinions of the public. We broach this question of duty at this early moment of our new-born freedom, in order that it may not be said we have neglected our charge as Journalists, or that we have given others an opportunity of alleging that they had taken part against public interests—against the cause of Medical Reform,—in the absence of a foreknowledge of the disastrous consequences. In pursuance, then, of the tenor of many articles written by us on various occasions during the last six or seven years, let us remind surgeons, physicians, and apothecaries, that they, the members of the medical profession—that they, the friends and companions of the aristocratic and middle classes, the comforters and protectors of the poorer members of society,— can exercise a greater influence over the public mind in this country, than could the members of any other profession, or of any other class, however exalted in rank, however useful in character. If, then, the members of our profession have hitherto slept on the post of duty, when they should have presented a bold and dauntless front against the plunderers of their rights, let them now shake off the lethargic stupor of inactivity—let them now cast off the disgraceful ban of slavish and submissive quietude, step forth at once into the ranks of a people struggling for national improvement, proclaim aloud the number and extent of *their* wants in the great work of regeneration, and strive unremittingly, zealously, and fearlessly, in one grand effort to achieve a full measure of justice. There must be no heart-burnings, no misgivings, no sacrifice of time in entering upon too minute an analysis of the causes of past disasters.

The abuses under which we groan in all the departments of our profession have but *one* source, and that is, *misgovernment*. Hence to this one point, and to this one point only, should the whole of our remedial exertions be directed. Whether we look to the infamous state of the laws relating to the medical profession,—whether we reflect on the *traffic* which is made of the sufferings of the poor at our "charitable" institutions,—whether we devote our attention to the indignities heaped upon the licentiates of the College of Physicians,—whether we contemplate the persecutions inflicted upon the members of the College of Surgeons,—whether we remember with shame that the trading Company of Apothecaries was, by the Act of 1815, placed as an iron rod over the heads of the members of the general branch of the profession, —whether we reflect upon one or upon the whole of these enormities, if we look back to the *source* of the calamity, difficulty there is none, in tracing it to corrupt and incompetent legislation. If Parliament had been constructed in 1815, as, we trust in God, it must now be constructed under the Reform Act, would the petition of Rhubarb Hall have obtained the favourable consideration of the Legislature? The Apothecaries' Bill was false in preamble, fallacious in principle, and injurious in detail. To the discerning eye it promised nothing but mischief, and nothing but mischief has it wrought. It held out to the members of the College of Surgeons the certain prospect of accomplishing the destruction of their rights, and the fatal indication has been fulfilled to the letter. To the public it offered the unqualified contingency of a monopoly in favour of Rhubarb Hall, and to students it predicated the cheerless triumph of a body of unlearned dealers in roots and syrups, over the noble and energetic minds of some thousands of the finest of our British youth. Still, a bill, deformed by such disgusting features, was allowed to pass into a law almost without a murmur. But the complaints consequent on the evils which have resulted from its active operation, have been deep, loud, and unceasing. That such a measure *ought* to be erased from the statute-book there can remain no doubt. Whether it *shall* be so expunged is a question to be determined by the members of the medical profession.

When the great attempt—in which Mr. Lawrence nominally assisted—was made, in 1826, to reform the College of Surgeons, Sir Robert Peel and his party proclaimed their opposition to the call of the friends of improvement, and thus the petition fell still-born to the ground. Five thousand members of that College, acting by their deputies, could not succeed even in getting a committee appointed to inquire into the manifold abuses of which they complained. But power was then in the hands of the Legislature. That power is now in the hands of the people, of whom the members of the College form so great and justly influential a portion. It should be understood that an *effectual* reform in the medical profession cannot be accomplished without the aid of a legislative enactment; and nothing would be so simple, at the same time so consistently gradual, and ultimately so thoroughly certain in its effects, as an act of incorporation for the London College of Medicine—an institution, be it remembered, which makes no direct attack upon the privileges of any existing establishment. The Fellows of that College pray not for the repeal of the Apothecaries' Act of 1815, solicit not the abrogation of the charter of the College in Lincoln's Inn Fields, seek not the destruction of that granted to the College of Physicians by Henry the Eighth. The old musty records of ancient date, and the iniquitous statutes of modern times, they seek not to expunge from the statute-book by means of a counter-measure to be carried in Parliament. Nothing so harsh is attempted, though, were even more than this prayed for by the community, a House of Commons elected by the public could scarcely hazard a refusal. Shall it be said, then, that in a reformed House, containing only a few men who will look to private rather than to public interests, the London College of Medicine will not receive that share of support which will place it in a position of honourable rivalry with institutions *confessedly* not answering the purposes for which they were designed? But we boldly contend, that if an act be not obtained for this College—that, in a word, if the cause of medical reform be not now speedily and completely triumphant, the fault will not rest with members of Parliament, but with the members of our own profession. As unquestionably, were they, by thinking on this subject, to stimulate their minds to a proper sense of duty, they would arise from their lethargy in a body, explain to candidates the objects for which the London College of Medicine was founded, and obtain from them a pledge to support a bill for its incorporation. It will not be sufficient to refer to medical reform as an abstract question; ten or fifteen minutes must be devoted to its explanation, and the injury to the public, arising from the present monopolies, must be clearly and zealously enforced. Once enlisted on the side of this glorious struggle in the cause of humanity and science, no candidate having the heart of a man, or the head of a reasonable creature, will abandon it on slight grounds. What are the facts? The children of poor parents—no matter what their talents—are now precluded, by law, from entering the medical profession. If the obstacles, now raised under the authority of the charters and acts of parliament of the monopolising colleges and companies, had existed when John Hunter was a young man—the great, the immortal Hunter—that man whose labours produced the unrivalled museum in Lincoln's Inn Fields, and the strength of whose genius has enabled his successors to save the lives of hundreds, and spare the sufferings of thousands—if, we repeat, such scandalous, such restrictive, such plundering regulations had been then in force, John Hunter himself could not have been a member of the English medical profession. This fact, with the self-evident inferences deducible from it, should be enforced, until every point is fastened upon the memory. When it is considered that it is only through the exercise of talent of the first order that great discoveries in medicine can be effected, and when it is further considered how immensely advancement in medical science has tended to mitigate the afflictions of our fellow-creatures, how perversely, how blindly base do those men appear, who have laboured assiduously to construct nothing but barriers to its progress! But equally base, and not less blind to the true interests of humanity, are those individuals who permit such a work of barbarism to proceed, while they possess the power to cast it into the dust.

We cannot suppose that members of Parliament will ever feel a desire to perpetuate abuses of which they themselves may be the victims, and from which they can never by any possibility derive advantage. Once instructed that great talents are requisite for the just and discriminating performance of medical duties, they assuredly will endeavour to throw wide open the doors of the temple where none but the talented are invited to appear. If the London College of Medicine imposed a single restriction upon candidates for the diploma, other than that of exacting a thorough knowledge of the science, we would be as free in denouncing it, as we are now bold in holding it up to encouragement. In these modern times of advancement, the public have been taught to denounce, as amongst the bitterest enemies of their country, those who uphold monopolies in the commonest articles of trade. The East-India monopoly is denounced; the Bank charter monopoly is reprobated; the Water company monopolies are repudiated; and yet the monopoly of *fees* by the existing medical colleges, extorted by means of base and iniquitous taxes upon mental accomplishments—in a word, a *duty* upon *mind*, passes without notice before the eyes of our wise legislators, and without eliciting a word from the seeming discernment of our contemporaries of the press! For what, then, after all, do we set up a claim? Simply this—FREE TRADE IN MEDICAL

KNOWLEDGE; or rather we solicit even less than this, for we only require that every man, who may display, in a public examination before the scrutators of the LONDON COLLEGE OF MEDICINE, a knowledge of his profession, be empowered by law to exercise his skill in healing the sick. As matters now stand, no young man can pass the door of the Apothecaries' Company, to receive his warrant for practice, without having cost his parents or his friends not less than *one thousand pounds*,—money almost wholly expended in forms or ceremonies, in compliance with the by-laws of individuals, who, in too many instances, have the effrontery to pocket the fees which are thus so infamously extorted.

We must resume this subject; meanwhile, as *canvassers*, we find, are already in the field, we appeal to the members of the medical profession,—with a conviction that we shall not call upon them in vain, and implore them to withhold support from those candidates who withhold *their* promises of aid from the cause of MEDICAL REFORM.

The Anatomy of the Thymus Gland. By Sir ASTLEY COOPER, Bart., F.R.S., Serjeant-Surgeon to the King, "Consulting-Surgeon" (?) to Guy's Hospital, &c. &c. &c. London. Longman and Co. 1832. 4to. pp. 60. plates 5.

SERJEANT-SURGEON Cooper does not enjoy the *otium cum dignitate* of his sinecure. In vain he endeavours to liquidate his heavy debt of time by basking in the smiles of the court, and inhaling the perfumed atmosphere of the drawing-room; in vain he woos the honours of the peerage; in vain "he dips now and then into Celsus." All will not fill up the great *hiatus* in his peace, "the craving void left aching in his breast." Even in the midst of the sumptuous banquet, the portly baronet sighs for his old pursuits, and an ideal scalpel points the way to the aromatic enjoyments of the dissecting-room. This despotism of habit is a wise law of our nature, and upon the whole, its consequences are more favourable than otherwise to mankind. Successful men have, thus, an ever active spring of motive to exercise their powers whilst they retain their validity. Sir Astley forms but one in a host of examples illustrative of this truth. By the bye, it is said, that the baronet pants for a higher title than that he already boasts. If he can persuade some friend at court to procure for him the "desire of his heart,"—why let him have it. He will make a very harmless "conservative." His first motion might be for leave to bring in a bill to permit an anatomical examination of the heads of the most conspicuous of his brother peers." It would lead to some curious disclosures.

Anxious to dispel the *ennui* which oppressed him, Sir Astley was wont to lounge at his butcher's shop, where his imagination was regaled with vivid associations of former days. On one of these occasions, Sir Astley's attention was excited by a remarkably large thymus gland in a calf. A crowd of forgotten notions respecting the uses of this curious structure, rushed upon his memory, his speculations were renewed, and he was led to a course of inquiry, which ended in the production of the important results which constitute the subject of this little book.

Sir Astley's attention was directed early in life to the function of the thymus gland, by observing that a great quantity of fluid always escaped when that organ was incised. In the calf, the examination of this gland was comparatively easy, but there is no part of the human subject so difficult of investigation. Its small size, the delicacy of its texture, its soft and pulpy nature, which renders it liable to tear under the slightest force, and the numerous small lobes which are combined to form it, all conspire to produce this difficulty, and to render it necessary to call in the aids which injection, hardening, unravelling, and the most careful and repeated dissection, can furnish. The author began his inquiries in the calf, and in the lamb, and soon found the difficulties of the investigation were greatly diminished. Proceeding from the gland of the quadruped to that of the human subject, he ascertained the comparative difference in its formation, as well as the points in which they resemble each other.

When the size of this organ is considered, when the quantity of fluid which it secretes, both in the human body and the quadruped, is recollected, when the important situation it occupies near the heart, and upon some of the largest vessels of the body, is remembered, as well as its appearing at the fœtal and infantile period only, it cannot be doubted, that the function which it performs is highly essential to the existence and growth of the fœtus and infant.

Sir Astley proceeds to consider consecutively, the general form of the thymus gland in the fœtal calf; the organization of the gland in the same animal; the comparative anatomy of the gland; the structure of the same gland in the human subject; its relative situation; the fascia of the thorax; the dissection of the thymus gland in the human subject; its physiology; and, finally, its diseases. We cannot withhold our meed of praise from the industry and ingenuity displayed through the whole of this excellent pamphlet; we avow our concurrence in the truth of his statements and conclusions, and propose to lay before our readers a few extracts from the most interesting parts, if any such parts can be chosen, where all is so engaging.

"The thymus gland of the calf is composed of a thoracic portion, of an isthmus, of a cervical part, and of two cornua. The gland reaches from the anterior mediastinum to the angle of the lower jaw, extending by its cornua on each side of the neck, and at the angle of the inferior maxillary bone, it doubles upon itself, so that its length is thus somewhat increased.

"In describing the situation of this organ, and of its different parts, I shall suppose the animal lying on its back.

"The thoracic portion of the thymus is placed under the upper part of the sternum, and it is generally somewhat inclined to the left side. It is situated in the anterior mediastinum, and consequently has the pleura placed upon its sides as it is reflected from the cartilages of the ribs to the pericardium.

"Posteriorly, it is attached to that membrane by cellular tissue; and inferiorly it is connected to a strong fascia, which extends from the cervical entrance of the thorax upon the external jugular veins, and on the arteries from the curvature of the aorta to the aorta itself.

"On the sides of the thoracic portion of the gland, pass the internal mammary arteries and veins, which supply it with branches of blood-vessels, whilst numerous absorbent glands appear between the thoracic portion and the termination of the jugular and subclavian veins.

"The isthmus of this gland produces the communication between the thoracic and cervical portions. It is of very small size compared with the two parts of the gland which it joins. It is placed opposite the upper bone of the sternum, adheres to the vena innominata, and the internal jugular vein descends on each side of it towards the heart. The isthmus passes through a very distinct aperture in the fascia, which is stretched over the opening of the neck to the thorax, and extends upon the jugular veins from one vessel to the other, forming a very strong ligament confining this portion in its situation.

"The cervical portion is placed upon the fore part of the trachea, where it is covered by the sterno-hyoidei and sterno-thyroidei muscles, and the carotid arteries and internal jugular veins pass near to its outer side.

"The cornua of the gland occupy each side of the trachea and larynx; they are covered near the sternum by the sterno-mastoidei muscles, which recede from them near the head, and are placed, more externally, opposite to the division of the common carotid artery into external and internal, and near the os hyoides the cornua are curved upon themselves so as to be doubled at their ends, to add to their length, and to increase their surface for secretion, and there they terminate."

Sir Astley next proceeds to consider the organization of the thymus gland in the fœtal calf. The organ possesses a general covering of coarse cellular membrane, by which it is united to the surrounding structures, and by which the component parts of this conglomerate gland are held together.

"Beneath this coarse envelope a reticular tissue is found, entering into the composition of the lobes so as to connect them with each other, and to unite the different parts of their structure.

"*A vessel of communication* subsists between the different lobes, which is formed of mucous membrane internally, and of a secretory structure more externally.

"The different lobes of the gland are tied together by a ligamentous structure which supports the blood-vessel, and the vessel of communication, and prevents the possibility of laceration. These lobes are most distinct in the cornua, and least so in the isthmus. The larger lobes are also divisible into smaller ones."

If alcohol be injected into the lobes numerous large cavities will be discovered, from which a great quantity of milky fluid will escape. This structure is the most essential part in the formation of the organ.

"When each lobe is unravelled, the external surface is found to be composed of small secreting cells, and within it are placed numerous cavities which are lined by delicate mucous membranes. Those which are placed in the interior of the lobe are reservoirs to the small cavities which are situated near the surface; they are of a considerable size, some as large as peas, but of an oblong form.

"The reservoirs vary in size, according to their degree of distention, but they are so thin, and yield so easily, that when opened they immediately fall together, so as not to be apparent without injecting them.

"Their number in each lobe varies, according to its magnitude; the secerning cavities upon the surface are small and circular, but the reservoirs open into a central cavity, from which proceeds a vessel of communication, passing from lobe to lobe. If quicksilver be thrown into one lobe, it passes, although not very readily, into several of the larger on each side of it.

"Each lobe, then, is made of numerous small secreting cells, and of larger cavities or reservoirs, and each of the larger lobes is connected by a tube surrounded by a portion of glandular substance. Thus the organ is constituted of lobes, having secreting cavities on the outside, reservoirs within, and a vessel of communication from lobe to lobe.

"The cells and reservoirs are lined by an extremely delicate mucous membrane, being as pellucid as the coat of an absorbent vessel, and somewhat elastic. The communicating vessel is equally delicate with the lining membrane of the cavities, and the parts would not be held together but for the additional ligamentous structure."

Sir Astley, in order to render the vessels and cavities visible, had them hardened and rendered opaque by alcohol, to which sometimes a solution of alum or corrosive sublimate was added. Another mode that he employed was, to inject the cavities with quicksilver, harden them in spirits of wine, and then, cutting the gland through the centre, they were easily seen and readily exhibited. Sir Astley's method consists in passing into the lobes a fine steel pipe. A column of quicksilver of from *seven* to *ten* inches should be employed; if the column exceed *ten* inches it lacerates the lining membrane. The tube is made to pierce the covering of one of the smaller lobes, the quicksilver immediately enters the secreting cavities and reservoir, and after filling one small lobe, it passes by the vessel of communication into the nearest large one. Thus two or three may be readily filled, and then the tube should be withdrawn and introduced in the same manner into a lobe two or three from the first, until the whole gland is injected. This is most easily accomplished in the cervical portion. After being filled with quicksilver, the gland should be dried, and a slice of its surface being removed all its cavities will be shown.

The author occasionally injected these bodies with alcohol, sometimes with coloured glue, but he succeeded most satisfactorily with coloured wax. In this way he filled the secretory cells, the reservoirs, and the communicating vessel, and made the injection pass from lobe to lobe, so as not only to show the structure of a lobe, but the communion of the lobes with each other, and the general formation of the organ.

"The blood-vessels of the thymus gland are derived from various sources. The arteries of the thoracic portion, of the isthmus, and of the sternal part of the cervical lobes, spring from the internal mammary; the arteries of the remaining part of the cervical portion derive their origin from the common carotid, whilst those of the cornua arise from the superior thyroideal and external carotid arteries.

"With respect to the veins of the gland, they return the blood from the thoracic portion into the internal mammary veins which accompany the arteries of the same name. There is also a vein peculiar to this gland, and which is placed parallel with the cervical part and with the cornua. The true vena thymica returns the blood of the isthmus, cervical portion, and the beginning of the cornua, into the internal jugular veins, whilst some of those cornua empty themselves into the superior thyroideal and jugular veins.

"Numerous absorbent glands are found on the spinal surface of the thymus, and if injected, many absorbents are discovered. On the posterior surface of the cornua and cervical portion, two large vessels proceed in each corner, and the side of the trachea towards the junction of the jugular veins with the superior cava; they terminate in the jugular veins at their junction with the superior cava by one or more orifices on each side.

"These vessels are formed to convey the fluid of the thymus gland into the veins. Although their size is so large as readily to admit of their being injected with wax, yet I believe them to be more of the structure of absorbent vessels than of excretory ducts.

"An excretory duct is in itself a gland (the ureter, for example); it is generally a muscular tube on the outer side, and a secreting membrane within; is free from any valvular apparatus, excepting at its termination, like the ureter and common duct of the liver. But the vessels I am now describing, although of large size, are transparent, and possess valves, and, above all, if quicksilver be thrown into the absorbent gland of the thymus, small vessels are filled from them, which open suddenly into a tube of considerable diameter, forming the two vessels I have mentioned; and, further to show that they partake more of the nature of absorbent vessels than the structure of an excretory duct, they cannot be injected but in their course towards the veins from the valves which they contain. Around the thoracic portion, numerous absorbent glands are found, which send vessels into the veins at the junction of the jugulars with the superior cava. These vessels I consider, and shall name, the absorbent ducts of the gland, and they are the carriers of the fluid from the thymus into the veins of the lower part of the neck."

Sir Astley has examined the gland in other animals, and the following is the result:—

"In the dog it is divided into two thoracic, and two cervical portions; it is relatively much less in this animal than in the fœtal calf. It consists of lobes which are but loosely connected, and they admit of being easily unravelled. In the lobes small cavities are found, which may be injected with quicksilver or alcohol.

"In the dog of six months old I found cavities of considerable size, and the gland, though larger than it usually is at birth or in the fœtal state, was hollow, and had little solid matter in its composition. In the kitten, the gland resembles in form that of the human subject, and contains numerous small cavities. In the ass the gland is broad, thin, and flat; it is divided into numerous large lobes, which are further subdivided into smaller. Each of the lobes contains many cavities which are of considerable magnitude. The thymus gland in the lamb bears a close resemblance to that of the fœtal calf, being composed of a thoracic portion, of an isthmus, of a cervical portion, and of two cornua. It is divided into large lobes, and then into smaller, and they contain secretory cells and reservoirs, which are easily injected with quicksilver, and which differ only from those of the fœtal calf, in being absolutely less, although relatively to the size of the animal, they bear the same proportion.

"In the pig, the gland is formed of two thoracic portions, of an isthmus and two cornua; its lobes are of considerable size, and minutely divided into smaller ones, and the gland is large in proportion to the weight of the animal. It contains secretory cavities so full of fluid, that it is difficult to inject them."

We shall continue our extracts from this little work in a future Number.

ROYAL INSTITUTION.

June 8, 1832.

LECTURE ON THE HISTORY, ORIGIN, PROGRESS, AND PRESENT STATE, OF LITHOTRIPSY.

By D. O. Edwards, *Esq.*

Mr. Chairman. Two years ago Mr. Burnet did us the kindness of giving in this theatre a demonstration of the lithontritic apparatus then in use. Since that time, several modifications have been made in the old instruments, and others have been constructed upon new principles. Many operations have been performed upon patients of all ranks, and much additional experience has in consequence been acquired. Many things that were then problematical, are now matters of fact; and, in short, our knowledge in relation both to the principles and the practice of this branch of surgery is so much extended as to give it an almost entirely new aspect.

In order to afford a distinct idea of the present state of lithotrity, I may perhaps be permitted to take a transient retrospect at the old methods employed in the relief of the formidable disease which it is the object of our labours to remove, as well as to glance at the origin and progress of the new method.

Stone in the bladder, arising as it does from purely physical causes, is a disease pretty nearly as ancient as the human constitution itself. In times when men were entirely ignorant of anatomy, it would be long before the group of symptoms characteristic of this malady could be accurately referred to their true cause. The two circumstances most likely to afford a clue to the just interpretation of these symptoms are, the occasional escape of small calculi through the urethra; and the occurrence of the same malady in certain quadrupeds.

The nature of the complaint once fully understood, it would not perhaps be difficult to conceive the means of relief that would first suggest themselves. The use of narcotics was very early known, and it is reasonable to suppose that these and other anodynes conveyed into the alimentary canal, would be selected as remedies.

The idea of dissolving the calculus implying a degree of chemical knowledge, would probably be a much later suggestion. The fugitive good effect or total inefficiency of such remedies, which only act indirectly and remotely through the nervous system, or by passing through the round of the circulation, would naturally lead to the contemplation of more direct and decisive measures.

To extract the stone through the natural avenue, and to break it in pieces for that purpose, are very obvious indications; but the deficiency of mechanical means at early periods would render such a plan impracticable, and occasion its immediate abandonment. A less obvious method of relief, it is true, is that of cutting into the bladder, in order to effect the extrication of the stone, but it was the only remaining alternative for the patient's torments.

The incision through the perineum was the first means adopted for penetrating into the bladder; that is, if we may trust the early records of medicine, which are nothing more than collections or compilations of current traditions. The method of Celsus, vulgarly called cutting on the gripe, is evidently but a slight improvement upon the rude means originally adopted, and is indeed the only operation that could have been performed by any person ignorant of anatomy. The prominence produced on the perineum through the traction of the stone by the fingers, would with very little knowledge of the parts afford a sufficient guide to the operator's knife. It is not to be imagined, that many patients could be found willing to submit to such a barbarous process, and of which the consequences were often as disgusting and calamitous as the disease itself.

In the course of ages, but few modifications were effected in the practice of lithotomy. The high operation, or incision above the pubes, is comparatively of modern origin, and may in point of safety and success be considered on a par with the more ancient plan.

The proportion of deaths to recoveries in these operations was always considerable, and amounts, even in the practice of the most accomplished surgeons of the present day, to the ratio of *one* in *seven*.

In the Norfolk and Norwich Hospital, where, from the number of cases, the surgeons have always been celebrated for their skill in lithotomy, the proportion has been but a little more favourable.

During a period of 44 years, from 1772 to 1816, five hundred and six persons were cut for stone in that hospital; out of these, seventy died, being a proportion of *one* to *seven and a quarter*.

In the practice of Mr. Chesselden, the most successful lithotomist of his time, out of 213 cases which he treated in St. Thomas's Hospital, 20 died, being a proportion of nearly one in ten.

Mr. Lynn, senior surgeon of the Westminster Hospital, who for coolness and dexterity as an operator has never been surpassed, informed me the other day, that at the commencement of his professional career, he performed thirty operations successively without losing a patient; but immediately afterwards he had a series of seven fatal cases. He also acquainted me that the average of fatality in *his* practice, during 50 years, has been one in eight.

From these statements it will be evident that lithotomy, under the most favourable circumstances, is rather an appalling remedy; and it is not surprising that patients should rather continue to bear all the anguish of their actual sufferings than resort to such a perilous alternative. Singular instances of long suffering, arising from the repugnance of patients to submit to lithotomy, are on record. One very striking example it may not be amiss to allude to, as affording a very apposite illustration. A baronet residing in one of the midland counties, suffered from this disease for a period of fifty years. The stone grew to such a size as to fill the whole bladder; the ureters or conduits coming from the kidneys became distended into reservoirs, and when the patient wanted to relieve himself of their contents, he was obliged to rest his head on the ground, and raise his pelvis to the utmost extent of his lower limbs. He ultimately submitted to lithotomy, and the perineal operation was performed by the late Mr. Cline. As much of the substance of the stone as would fill a breakfast cup was scooped away; but the patient derived no material relief, and died soon after.

No means of removing this fearful malady less severe than that described, was resorted to during a long series of years, though many speculative plans were probably entertained. Mr. Edwards here gave a general history of the origin of the lithontriptic art, alluding to the labours of Leroy, Civiale, and several others, of which accounts have already appeared in this Journal. Mr. Edwards also minutely described the routine of practice adopted by the continental lithotritists, and pointed out its various defects. He then enlarged upon the plan of treatment established by Baron Heurteloup. He observed, that in contemplating the performance of this operation, two subjects pressed particularly upon the attention of the surgeon. First, the bearings and condition of the bladder, the chamber in which he is to work. Secondly, the nature and properties of the calculi which are the objects of our search.

The lecturer apologized to the audience for alluding to anatomical points, and gave a general description of the relations of the bladder in health, and its modifications by disease. It was not to be supposed that a concretion could go on accumulating in the bladder without deranging its health; and it was a pretty general rule that the larger the stone the more contracted was the bladder, and it not unfrequently happened that the relative magnitude was so much altered, that the one could not be turned in the other. In some cases the organ was irritable and spasmodic, presenting a partial hypertrophy, or thickening of its muscular fibres. At other times it was perfectly helpless and paralytic.

The second subject related to the nature and position of the stone. Chemists have classified these bodies according to their constitution; but these relations, though highly interesting in themselves, are of secondary importance to the lithontritist, except in so far as they tend to modify their mechanical properties. In nine cases out of ten calculi are composed of uric acid, or the triple phosphates, or of both. Simple uric acid calculi are shown by the experience of lithotritists to be smooth on the surface, laminated in texture, frangible, and generally flat. Those consisting of ammonio-magnesian phosphate, are hard, compact, crystallized on the surface, and spheroidal. The calculi formed by a combination of these constituents, tend to flatness, or sphericity, according to the predominating ingredient. Of the other urinary concretions, the bone-earth and the oxalate of limestones are generally spheroidal. The first is easily broken, but the last is more cohesive. Calculi of other kinds occasionally infest the human bladder, but they are so rare, and generally so small, as to occasion little trouble to the operator. It is sufficient for the purpose of the lithontritic surgeon to distinguish these bodies into three classes, the *spherical*, the *oval*, and the *flat*; for it is principally in respect to form and size that they influence his manipulation.

The position of all moveable stones is chiefly determined by their gravitation, unless when they are held by a convulsive action of any part of the organ. When the patient is erect, they are in the *bas-fond*, or pouch. When he expels his water they are impelled into the neck, and, in short, their place is generally influenced by the posture of the patient. Spheroidal stones are usually moveable, but those that are flat and oval are most commonly fixed in the *bas fond*, and not unfrequently held against the prostate gland by a morbid contraction. Occasionally the stone escapes into a herniary sac, and then it ceases to annoy the patient, as in the celebrated case treated by Mrs. Stevens.

Mr. Edwards proceeded to make some observations on the proper position of the patient, and alluded to the rectangle bed, and *point-fixé* of Baron Heurteloup. He stated that the process of the operation might be conveniently divided into the following stages. First, the injection of the bladder. Second, the exploration of that organ and its contents. Third, the seizure of the stone. Fourth, its destruction. Fifth, the removal of the resulting fragments and mollecules. Some germain observations were here made touching certain prejudices that prevail on the subject of the bladder. The application of sudden force in the interior of so delicate an organ was long considered as utterly impracticable; and all that anatomists could bring themselves to contemplate was, the exertion of a regulated friction upon the calculus. The intimate acquaintance, however, recently acquired as to the structure and use of these parts, has led to the adoption of highly energetic measures.

A description was next given, seriatim, of all the instruments employed in the lithontritic process. Their construction was explained, and mode of action illustrated. In this waywere brought under review, the recto-curvilineal catheter, the surgical syringe, the perce-pierre, the trois-branches à virgule, the evideur à forceps, the perforator, the crête de coq, the percuteur droit, the pince-servante, the brise-coque, the percuteur courbe à marteau, and the sonde evacuatrice, of all of which the *modus operandi* has been explained in this journal.

In conclusion the lecturer observed, it was remarkable so little success should have attended the endeavours of surgeons generally in this branch of their profession, and then proceeded thus:—

An examination into the causes of their failure would form an interesting subject of inquiry. That it is not due to any want of ability is evident from the eminence of those who have made unsuccessful attempts. To prove this we need only mention the names of Dupuytren, Roux, and Lallement, who are amongst the most able surgeons in Europe. If I might presume to form an opinion on the subject, I would suggest, that their failure was due to two causes. First, the peculiar habits induced by surgical manipulations; second, a want of acquaintance with the condition of the living bladder, especially whilst influenced by the presence of calculi.

There is nothing singular in the dependence on the touch only which is observed in lithontritic operations; for the obstetric surgeon, in his manœuvres, is equally deprived of the assistance of sight. Indeed the education of the touch generally, is a subject of so much importance in the practice of surgery, that a power of discrimination through this sense has been termed the *tactus eruditus*. The manipulation of these instruments, therefore, out of sight, is a task that may be easily mastered.

There is this peculiarity in almost all surgical instruments, that they have no moving property, except what they derive from the hand of the surgeon. The knife and the forceps move in exact obedience to his volition, and produce an effect no greater or less than that which he meditates. The importance of this circumstance may be conceived, when a tissue as fine as a spider's web under the knife, is the difference between life and death to the patient. The intervention of elasticity, or any other eccentric mobile property, would materially increase the difficulties of the case. The surgeon's hand is, therefore, not prepared immediately to adapt itself to these secondary properties. The difficulty in manœuvring lithontritic instruments appears to me to arise, if difficulty exist from their possessing this property of elasticity. A mechanic would probably find less difficulty in their management than a surgeon. The knowledge of the anatomy of the bladder is usually derived from an examination of the dead organ. In the usual mode of showing this organ, it is distended to the utmost, and consequently but one of its relations with the surrounding parts is exhibited. In the living state, since the sides of the organ exactly adapt themselves to its contents, its shape must constantly vary according to the quantity of fluid it contains. While the organ is empty, it forms a disk applied on the posterior aspect of the pubic bone, and presents a concavity in that view. As it enlarges, its form varies according to certain laws, which it is beside my purpose to specify.

That rash attempts with these apparently complicate instruments, without preliminary practice, or a clear conception of the field of action, should have been unsuccessful, cannot excite surprise.

At the conclusion of the lecture, Mr. Faraday came forward and made the usual address on the conclusion of the season. He stated, that in consequence of the want of patronage * the publication of the Journal of the Institution had been abandoned. In other respects, he was happy to say the institution was in a flourishing condition.

The business of the evening was concluded at the request of Mr. Edwards, by a demonstration of the action of the lithontritic instruments upon real calculi, in which Baron Heurteloup exhibited such tact and dexterity.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

MADE BY

Mr. BRODIE.

May 31, 1832.

ABSCESS IN THE MEATUS AUDITORIUS.

A man was admitted with this abscess, arising from disease of the bone which had been caused by a blow, and a poultice has been applied to the part. Mr. Brodie said, that such an abscess frequently arose from disease of the bone, and that disorganization frequently went on to a very great extent; matter was formed between the dura mater and the bone; the membranes of the brain became inflamed, and death was the result. "I once attended a young gentleman," continued Mr. Brodie, "who had always been from his earliest youth remarkably dull and listless. He had a discharge from the ear, and as long as that discharge continued things went on very well. This discharge, however, one day stopped; he died very soon afterwards; and on examining his body after death, an abscess as large as an orange was found in one of the lobes of the cerebrum, and a small aperture through the temporal bone, leading to the meatus auditorius internus, was also discovered, through which the matter had been discharged. This aperture had been stopped up, and the patient had died from the collection of matter pressing upon the brain. But such cases as this are rare; one, however, somewhat similar to it has been in the hospital under the care of Mr. Hawkins; and after death abscess in the brain was discovered."

25. Mr. Brodie examined this patient again to-day; and after probing the meatus externus, to discover if there were any dead bone, and where it was situated, he found he could not detect any; and said that

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* We marvel at the use of such a word, on such an occasion, by Mr. Faraday. He should have said, "in consequence of the *want of sense* displayed by the public."—Ed. L.

he should not do anything further in the case at present.

"You know, Gentlemen, that one of the first rules of our art is to prevent future and further mischief and danger; and if I meddle any further with this case at present, I may produce inflammation of the membranes of the brain, and the patient may die.* This man, too, has some bad symptoms of mischief in the chest; and even in patients who are in very good health, I am very cautious how I meddle with such a delicate organ. I confess I do not like to do anything in this case. I feel more inclined to let this man wait and watch the symptoms as they occur. I knew a little girl who was in this hospital, and who had broken a piece of slate pencil in her ear; inflammation of the membranes of the brain came on, and she died."

A pupil here inquired, whether a foreign body might not be extracted from the meatus externus by the suction force of a syringe.

"Why, I do not know," replied Mr. Brodie. "You know suction is nothing more than the drawing off the atmospheric pressure from one part to have it applied with more force to another. If the foreign body, whatever it may be, completely fills up the passage, why then suction may do; but if the body is too small for this, and lets the air pass by it, why then your suction will be of no avail, and I do not know that it is a plan which I should recommend."

The pupil said that he had known a case in which a glass head had got into the ear, and had been removed by a syringe; and the gentleman in whose practice it had occurred, meant to follow the same plan whenever any future case of the kind presented itself to him.

After this digression the patient's chest was examined by Dr. Hamson (who was present), and Mr. Brodie asked the Doctor whether he had ever had an opportunity of examining the man's chest.

Dr. Hamson replied "that he had, and that in his opinion the man had incipient tubercles forming in the apex of the left lung; for if you strike the upper and outer end of *the clavicle* of that side, you will find a very different sound given out to that which will result from striking the opposite clavicle in the same place."

"But," remarked Mr. Brodie, somewhat astonished, "Do you strike *on the bone of the clavicle* to ascertain the presence of disease *in the chest*."

"Yes I do! and," added Dr. Hamson, somewhat pointedly, "the wise laugh at it" (Mr. Brodie and several of the pupils were smiling at the time*), "but it is only a proof of their ignorance, for it is under the *outer prominence of the clavicle that the apex of the lung is to be found*,* and it is there that the disease in this instance exists. Pectoriloquism also over the middle of the clavicle, and over the great vessels, will also frequently detect the presence of disease."

Mr. Brodie and Dr. Hamson now proceeded to examine the patient's chest with the stethoscope, and having fully satisfied themselves of the presence of disease, the man was sent back to his bed, and Mr. Brodie then made the foregoing remarks on the case. The man has a pale phthisical appearance; he complains of great pain in the situation of the intercostal muscles when he coughs; he expectorates a great quantity of frothy pus and mucus, and sweats profusely at night. He was ordered to be kept quiet, his bowels to be kept gently open, and to take infusion of roses, with diluted sulphuric acid and syrup in a draught. When ordering these medicines, Mr. Brodie asked whether the apothecary put syrup or sugar into the medicines, adding that he merely ordered it in this instance to cover the taste of the acid. Mr. Fernandez answered that *they never put in either one or the other, except it was underlined in writing the prescription.* In this instance the syrup was underlined; so that the poor man will stand a chance of having it put into his medicine.

Another instance of culpable neglect in hospital practice!

Some few days since, Mr. Brodie, afte dividing the sphincter muscle in a case or fistula in ano, ordered the patient to take the sulphate of quinine. Upon visiting him some days afterwards, Mr. Brodie seemed surprised that the quinine had been of no benefit to him, when the man handed down *the paper* in which the pills were, and *they were found to be as hard as stones, and had evidently been made up for some weeks past*; they were so hard, that those he had taken had in all probability passed through the intestines without being in the slightest degree acted upon. Mr. Brodie immediately ordered the pills to be discontinued, and the quinine was prescribed in another form. Mr. Brodie said that he was once consulted by a gentleman respecting an intermittent affection, for which he prescribed quinine; the gentleman had the prescription (pills) made up at Savory's, yet he derived no benefit from the medicine, and on its being given in a different form, he recovered rapidly.

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* Mr. Brodie ought to have made some mention of the *ear* operation performed at St. Bartholomew's by the "Little Cock Sparrow."—Ed. L.

* Cruikshank only could have done justice to the scene that presented itself at this moment.

Amongst the different officers connected with the hospital, we find the following visiting apothecaries :—

E. A. Brande, Esq.
H. P. Fuller, Esq.
John Nussey, Esq.
M.W. Andrews, Esq.

Apothecary (who resides in the house), Mr. Hutchins.

Assistant-apothecary (ditto), Mr. Hamilton.

Now here are six gentlemen whose duty, we presume, to be to see not only that the patients are supplied with the medicines ordered for them by the physician or surgeon in attendance, but also to see that those medicines are made up *correctly*—whether the different substances ordered in the prescription be underlined or not; and if such be their duty, who will not say that in the above instances that duty has been most shamefully and disgracefully neglected? What a miserable subterfuge is this underlining system—what a despicable artifice to save the hospital funds at the expense of a poor patient's feelings. Out upon all vile charity-mongers.

ABSCESSES IN THE LOINS.

"In opening such abscesses as these," Mr. Brodie remarked, "Mr. Abernethy used to be very fearful of air getting into them; for my own part, I open these abscesses in the same simple manner that I do all others, and leave them to heal or to remain open just as the result may prove."

June 1st, 1832.

MINERAL ACIDS.

In the case of the scrofulous disease of the ancle joint, which we adverted to in a former number, Mr. Brodie asked Mr. Fernandez what medicines the man was taking. "A pint (?) of infusion of roses, with one drachm of diluted sulphuric acid every day." "Oh! but be careful not to increase the dose of the acid beyond one drachm." "What benefit have you found to result from giving large doses of the acid?" asked Dr. Hamson. "Why I really never gave it in large doses," answered Mr. Brodie; "What I have ordered has generally been the nitric acid, and I do not know that I place much reliance upon that. In some cachectic or mercurial diseases, acid, they say, is sometimes of benefit, but I cannot say that I have ever found any very good effects to result from the use of it. I will tell you where I have found acid of very great service, and that is in cases of offensive, stinking, alkaline urine, where I have changed its character to acid by giving from ♏xlv to ʒj of nitric acid in the day. In one case I recollect that it had this effect in twenty-four hours."

DISEASE OF THE LOWER JAW.

A man was admitted on Wednesday last with a tumour on the left side of the lower-jaw. Mr. Brodie saw him to-day: the tumour burst and discharged before he came to the hospital, and through the opening Mr. Brodie believes he can detect diseased bone with the probe. The last molar tooth but one being loose, Mr. Fernandez extracted it. The body of the tooth was sound, but the fang appeared as if it had been in contact with diseased bone. The man's health is good, but Mr. Brodie said he had known a case somewhat similar, which nevertheless had turned out to be malignant.

TURPENTINE IN RHEUMATISM.

"I order the spirits of turpentine in rheumatism for a two-fold reason,—first for its local action on the part, and, secondly, because I believe some portion of it is absorbed, and does good in that way."

OLD CUSTOMS TO BE RESPECTED.

On examining a patient's fractured leg, Mr. Brodie remarked to the house-surgeon, that there was no foot-board attached to the junks to support the man's foot. Mr. Fernandez said, that the man did not feel the want of it then. "Oh, but that is no reason why he should not feel the want of it hereafter. In these cases the foot gets tired, and cannot support itself, and, besides, its weight if not supported, tends to draw down the bone, and thus prevents the reunion of the fracture; further, *you may be sure that there is always some good reason at the bottom of all these old rules and regulations, and therefore they should not be departed from, except some equally good reason for so doing can be assigned.*"

FRACTURED RADIUS *v.* DISLOCATION OF THE WRIST.

A man was admitted into the hospital with fracture of the radius just above the carpus. Mr. Fernandez had put on two splints and a bandage, and slung the patient's arm to his side in the usual manner. Mr. Brodie, however, took these off and said, "Gentlemen, this is a case of what they call dislocation of the wrist, and, strange to say, books and authors of the present day, even down to Sir Astley Cooper, still speak of dislocation of the wrist; now, for my own part, I never saw such a thing, nor do I think such a thing can occur, for in the wrist the ligaments are much stronger than the bones, and, therefore, where any force was used, the bones would certainly give way first.

Now I will show you the way in which I do up these fractures; it is a plan too which I have always followed with success. You see that here is one projection below, arising from the dropping down of the fractured end of the radius, and here is another projection above, of the carpus. Well, now, I put a compress above and below on each of these projections, then I put a piece of strapping to each; the piece of strapping below I fasten over the splint above, and the piece of strapping above I fasten under the splint below, and thus steadily confine them, and I then put a roller over the whole. When the man's arm was thus put up, he expressed himself much easier. "About ten years ago," said Mr. Brodie, "a man came into the hospital, with a very severe accident. I believe it was an injury of the head, a compound fracture, or something of the kind. However, this man had two large bulging lumps, one above the wrist and the other below the radius. On examining the body after death, I cut across the muscles and tendons, and there I found a large quantity of extravasated blood; and down dropt the hand, and there was a fracture of the radius, just as in this case. A medical gentleman once sent to me to say, that he had a case of dislocated wrist, which he could not reduce, and from his description I was led to believe, that it was a subluxation. This patient died, and the medical gentleman examined the body; and on opening the wrist, he found just such a fracture of the radius as this."

To some remarks from some of the pupils who said they had seen a child's wrist frequently dislocated, Mr. Brodie said, "that such cases might be, but that he had never seen them;" adding, "Indeed, I believe Dupuytren says he never saw one."

DISLOCATION OF THE METATARSAL BONE OF THE GREAT-TOE — EXCISION OF THE BONE.

A man was brought into the hospital a few days ago with a dislocation. Mr. Fernandez, who saw the case immediately, states that the bone was fixed *transversely*, and so firmly that he could not move it, and, therefore, he thought that the best practice he could adopt was to *cut down on the bone, and take it out*, which he accordingly did. On Mr. Brodie's visiting the man to-day, he said that he supposed from what Mr. Fernandez had said, that he could do no other than he had done; but that the loss of the bone would prove a very serious one to the man, as it would entirely deprive him of the use of his great-toe, which was more used in walking than all the others put together. Mr. Brodie here asked Mr. Fernandez whether he had left behind any of the periosteum. Mr. Fernandez replied that he had, because on this depends a great deal. If a portion of the periosteum be left behind, ossific matter may be again deposited, or, at all events, a ligamentous band may be formed similar to that which is formed when a portion of the lower jaw is excised. Let the man be kept quiet, and light dressings be applied to the wound. With reference to this case, we cannot help considering, that Mr. Fernandez's treatment was somewhat too prompt, and that he should have paused before he adopted the line of practice which he did. He states that the bone was fixed firmly, and could not be moved, being bound tightly down by the tendons, but we have heard nothing of how long a time elapsed from the period of the accident to the man's admission to the hospital. Of course Mr. Fernandez would amputate a dislocated *thumb* or *thigh*, if, in either case, he were to fail in his attempt at reduction.

BIRMINGHAM SCHOOL OF MEDICINE.

ANNIVERSARY DINNER AND DISTRIBUTION OF PRIZES.

The public anatomical examination of the students of this school, conducted by Bransby Cooper, Esq., was held on the 4th of June, and nearly all the medical men of the town and several strangers, including Sir E. E. Wilmot, Bart. and Sir C. Throgmorton, Bart., were present. On the same day the anniversary dinner, given by the students to the lecturers, took place at Dee's Royal Hotel, and was attended by upwards of sixty students and friends to the institution. Dr. E. Johnstone presided, supported by Sir E. Wilmot, Bart., B. Cooper, Esq., Drs. Hasting, Conolly, John and James Johnstone, J. W. Unett, Esq., the Lecturers, and numerous professional men from the adjacent districts. Edward Townsend Cox, Esq., officiated as vice-president. During the evening the following prizes were awarded. The *Johnstonian* prize of ten guineas, by Drs. Pearson, Eccles, and W. S. Cox, Esq. to Mr. Wilkes; the prize of five guineas, offered by E. T. Cox, Esq., was adjudged by Sir A. Cooper to Mr. Hammond, also

Anatomy—B. Cooper, Esq., *Examiner*.
1*st Silver Medal*—Mr. Walton.
2*nd ditto*—Mr. Vaux.

Materia Medica—Dr. Conolly, *Exam.*
1*st Silver Medal*—Mr. Wilmott.
2*nd ditto*—Mr. Taylor.

Chemistry—Rich. Phillips, Esq., *Exam.*
1st Silver Medal—Mr. Bolton.
2nd ditto—Mr. Beddoe.

Midwifery—Dr. Bagnall, and Edward Grainger, Esq., *Examiners.*
1st Silver Medal—Mr. Taylor.
2nd ditto—Mr. Willmott.*

The general proceedings of the evening were extremely gratifying. Sir E. Wilmot signified his intention of giving a gold medal to be awarded on such terms as may be deemed most advantageous to the institution; an instance of judicious liberality which we have great pleasure in mentioning. It was pleasing to observe, as was remarked by this excellent baronet in his address at the dinner, that whilst the town of Birmingham is rising into political importance, it is also becoming greatly distinguished for its liberal institutions for the promotion of learning and the sciences. To those acquainted with Dr. Johnstone, it is unnecessary to add, that his presence in the chair, and his kind and affable demeanor, contributed much to the enjoyment of the day. The medical profession has reason to be proud of such a man, in whom years have not extinguished the love of knowledge, or those amiable sentiments which render knowledge a happiness to the possessor and a blessing to society, and whose long course has been unmarked by those petty jealousies which have distinguished some leading members of the profession of this town. The president in the course of the evening announced, that the following noblemen and gentlemen had been chosen to form the committee for the present year. Earl Bradford, Lord Lyttelton, Sir G. Shepwith, Bart., Sir C. Throckmorton, Bart., Sir E. Wilmot, Bart., F. Lawley, Esq., M.P., James Taylor, Esq., Drs. E. and J. Johnstone, Timothy Smith, Esq., T. W. Unett, Esq., Richard Wood, Esq., E. T. Cox, Esq.

The library, which is free to every member of the profession, has received a valuable collection of books from Lord Lyttelton, Sir C. Throckmorton, and others.

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* The classes for the Practice of Physic, Surgery, Forensic Medicine, and Botany, are now going on; the examinations in these departments take place at the conclusion of the summer session.

TO CORRESPONDENTS.

So many theories have been projected on the subject broached by *Tyro*, that no additions to the number should be made public, unless the authors are most fully prepared to develop and support them in every particular. Mere suggestions will obtain no consideration.

University of London.—A correspondent informs us, that in consequence of *Mr. Noble* having very honourably declined accepting the gold medal in the class of *Anatomy and Physiology*, the gold medal was awarded to *Mr. Houlton.* The first silver medal to *Mr. Nossoc*, and the second to *Mr. Rayner.*

Mr. Firth. An experiment of a very similar kind was tried some years since in Paris, and failed. It has been repeated in England, also unsuccessfully.

We shall, in the course of a week or two, accord to the work, mentioned by our Shropshire correspondent, ample justice. The Dub was, most probably, jealous.

Mr. C. It is not usual for us to announce in this place the publication of non-medical works.

A Pupil of the London Hospital is requested to favour us with his name and address confidentially. His communication shall certainly be published when it is authenticated.

Several letters respecting the *Naval Surgeons* have come to hand. We will endeavour to find a place for some of them in our next number.

Will *A Subscriber* be kind enough to favour us with his name? In confidence, of course.

Space shall be found for the "series of communications" alluded to by *Mr. I.*——, but it is hoped that they will not extend beyond the lowest number of pages mentioned in our correspondent's note.

"*ρ.*" seems competent to the task of discovering literary errors, but he is himself so great a culprit, that it would be equally unjust and injudicious in us to insert his paper. He has discovered blunders enough, in all conscience in Dr. M.'s work, but his own sentences are full of bad writing. They require, indeed, not less revision than the text under review. As our correspondent has evidently taken considerable pains in the production of his essay, we will take a couple of sentences at random from it, to show him the propriety of our decision. In folio 8, for instance, he says,

"This requires no *comment*, except, perhaps, an *expression of surprise*," &c.

Folio 9. "They demonstrate to a *French scholar*, that Dr. M. is as little acquainted with *that language*," &c.

Why, Dr. M. himself has hardly any thing worse than this in his book. However, putting aside this source of objection, it would be doing too much honour to the author whose work is quoted, to occupy three or four pages in showing his unfitness for a rank in which no one ever yet placed him. A selection of such blunders from *one* page of a book, is enough at all times to settle the literary claims of the work. A paper of a dozen folios is long enough to decide the pretensions of a score of such authors. We fully accord with the sentiments contained in the introduction to "*ρ.*'s" paper. We shall at any time be glad to hear from "*ρ.*"

Some of the opinions advanced by *Medicus*, "on the malignant cholera," can have no value while an anonymous signature is attached to them. Others do not need authentication, but we cannot divide his criticism.

Errata.—In Mr. Faraday's paper, May 18th, page 283, line 19, for "acopadium," *read* lycopodium. Line 30, transpose the *comma* after "transparent" to after "coloured."

In Dr. Knight's paper, page 301, col. 1, line 0, for "her aspect was *healthy*," read "her aspect was *deathly*." Line 0, for "cure," read "case."

London Hospital.—In the case of *hydrops articuli*, page 288, col. 2, it appears as if the *tinct. ferri muriat. concentrat.* had been discontinued. It should be "the *unguent. hydriodat. potassæ* did not appear to be of much benefit, and was consequently discontinued." The patient still continues taking the tinct. ferri. In the case of Watley, whose leg was amputated, *six* ligatures were applied.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JUNE 23. [1831-32.

HISTORY AND TREATMENT
OF
THE MALIGNANT CHOLERA
AS IT PREVAILED
AT VIENNA,

From the 12*th of August* 1831, *to the* 15*th of February* 1832.

By John Freeland Fergus, *Esq. Surgeon.*

[In transmitting the following paper for insertion in The Lancet, I beg to state that it is not to be regarded as a document containing all the various details which distinguished the epidemic to which it relates, but as merely presenting a condensed view of the most important facts observed in Vienna during the prevalence of the disease, as well as those which I noticed in the course of a journey performed in Hungary, Moravia, and Austrian Poland. Each fact has been carefully weighed, and not a particle of evidence advanced which has not been gathered from attentive personal observation. With regard to the post-mortem examinations, I may state, that of the immense number which I had the opportunity of attending, about 200 were performed with the most scrupulous care by Dr. Wagner, professor of pathological anatomy in the university of Vienna, who is justly ranked amongst the best morbid anatomists of Germany — J. F. F.]

ORIGIN AND PROGRESS OF THE DISEASE.

For some years before cholera appeared at Vienna, intermittent fevers had become more and more frequent. In the spring of 1831, an influenza reigned there, as well as over the rest of Germany. In July and August a gastric nervous fever raged with great violence; it was always accompanied with vomiting and purging, and gradually ook on more and more the symptoms of cholera, at the same time that the morbid appearances in the intestines peculiar to these fevers changed into those found in cholera.

On the 10th of August a wretched woman, 52 years old, residing in the house No. 181 in the city (the houses in Vienna are numbered for the whole city, and not for particular streets), in the immediate neighbourhood of a house where seventeen persons had been attacked with this nervous fever, was, after exposure to wet, seized with all the symptoms of cholera; the most minute inquiries led to no source of contagion, and the person who nursed her remained quite well.

On the 13th a man in No. 185, who had had a diarrhœa for three days, from exposure to cold, was, on a second exposure, seized with cholera, and died; his body was opened, and presented the usual appearances after cholera. All his family remained well; but a man who was employed to lay out the body, who lived at No. 177, and who had chronic disease of the stomach, was seized on the 16th and died.

On the 14th a girl was seized in No. 181; she denied having had communication with the old woman, or those around her.

After these cases the disease no longer confined itself to this neighbourhood, but single cases took place all over the town, and, in the course of a week or two, in the most distant parts of the suburbs. The subjects of these cases had all been exposed to cold, or had made an error in diet; none of them had had communication with other sick, and they lived in parts of the town and suburbs at the greatest distance from each other. They were all of the lower classes.

On the night of the 14th of September (after three days of rain and wind from the W. and N.W.), it broke out in its greatest intensity in those streets in the immediate vicinity of that in which the first sporadic case was seen, and where the nervous fever had been so severe. This outbreak was as sudden as a clap of thunder; in two or three hours before and after midnight, more than 100 persons were seized.

It was at this moment only, that people of the higher ranks were attacked, and

No. 460.

these were almost all seized on returning from a place of public amusement. At this time, also, the greatest number of robust healthy people were attacked. The intensity of the epidemic (limited as it was in extent) seemed to have reached such a height, that a stronger constitution, and a better mode of living, were no longer proof against it, and it took place after exciting causes of the slightest kind. At this time no other disease was seen. The cholera ended quickly, in death, or in return to health; it was strong and clear; little complicated with other diseases. People of all ranks complained of loss of appetite—sudden giddiness—rumbling in the bowels—looseness of stools, which lasted from a few hours to some days—cramps in the extremities, with twitchings of the muscles; all these were most felt during the night, and especially towards morning. Afterwards it lost in intensity, and the rich and healthy were rarely attacked; the poor and sickly continued to fall before it, but the course was longer, and it generally ended in a low or nervous fever, or in those diseases to which the patient was most predisposed.

No other diseases were prevalent till about the end of October, when, after cold dry weather, rheumatic and catarrhal fever of an active character became very frequent. November, with cold damp weather, brought catarrh, with nervous fever and intermittents. At the end of this month an anasarca of the extremities attacked patients of all kinds, especially those who were recovering from cholera, intermittent fever, or scarlatina, and those who had lain long in the hospital with chronic diseases. December, January, and February, were in general healthy. The cholera crept slowly along, showing itself here and there with a little force, and then ceasing: the last case occurred in the middle of February.

This was the general course of the disease in the town; in the suburbs, and even in separate institutions, it ran the same course. After a few precursory sporadic cases, it broke out suddenly in its full force. No circumstances of change of weather, of situation, or particular communication, regulated the progress or course of the disease. It was found to reach its height under all atmospheric states; it did not spread regularly from one suburb to another, but often broke out in all its violence in parts the furthest from, and having no intercourse with, those already attacked. It did not attack always first the low and bad-built streets, but in them it was more extended, and had a more adynamic character.

In the general hospital, the first sporadic case took place on the 18th of August in a man of 68 years. He could not have been in communication with cholera patients; none of the forty patients who lay in the same ward, nor of the nurses who attended him, were taken ill. It was not till after fourteen days that another case was seen, and this was in a ward at a very great distance from the other. From this time the cases became numerous, but they took place neither in the order in which the physicians made their visits, nor in that in which the wards stand to each other.

The ground-floor of the house No. 30, belonging to the hospital Zum Blauen Herr Gott, was fitted up for cholera patients. The second story continued to be occupied by patients affected with other diseases of every kind. The first cholera case was admitted on the 13th of August, and from this time till the end of the epidemic it was continually full of cholera patients of every variety. The second story received, as usual, patients with all other diseases. The physician made his visit to those in the second story, immediately after having seen those in the first. No disinfecting means were used, yet it was not till the middle of September that a man, after having been twenty-four hours in the second story for gastric fever, was seized with cholera. During the whole of the epidemic, only six persons had cholera in the second story, and of these only one died.

The Narren Thurm is a large tower of five stories, where the incurably insane are kept. The two undermost stories are occupied in common by the quiet and harmless, the rest of the building is divided into cells for the furious and unruly. The first case took place in the second story on the 18th of September; on the 19th a man died in the first; he was the only one, but in the separate cells the disease committed the greatest ravages, though the patients had communication with none but their attendants, none of whom were attacked. It often happened, that when two were together in a cell, one died and the other was unaffected.

The disease was very strong in the Lying-in-Hospital. Generally, when a newly-delivered woman was attacked, her child was seized and died also. However, there were some examples where the mother died and the child remained healthy. More than fifty mothers whose children died of the disease, and who continued to nurse them till the last, were not affected.

It never broke out in the great garrisons; most of the soldiers were attacked while on guard during the night. Here the very sudden changes of intensity and character in the disease were clearly and strikingly seen. On some nights only two or three were seized, and on others from eight to fifteen, every apparent condition remaining the same. It could not depend on partial or accidental circumstances, because men on duty at very distant stations, of different nations and corps, were attacked, at the same time. Variations in the character of the disease were seen; on some days the ipecacuanha produced vomiting and reaction in the severest cases, while on others it had no effect whatever; this was in October, when the disease was most intense amongst them. Again, on other days all the patients had vomiting or hiccup, &c.

Only three medical gentlemen died of the disease, two of them at the commencement, without having seen a cholera patient. The third was attacked towards the end of the epidemic, three days after being appointed to take charge of some cholera patients.

All this seems to prove the non-existence of contagion. Indeed, till towards the end of the epidemic no person would believe in it, but then some very strong examples occurred. For instance, a man labouring under cholera was brought to the clinic of Professor Bischoff; no case of cholera had yet occurred in that hospital. In the space of twenty-four hours after the arrival of the man, eight patients who lay next him were taken ill, six of them died, as well as three men who had rubbed him. After these the ward was fumigated, and no more cases were seen in this hospital.

During the whole of the epidemic, the meteorological observations made by the celebrated Professor Baumgartner, show that changes in wind and temperature, in the nature and intensity of the electrical luid, and in the hygrometricity of the air, ad no influence on the disease; and also, hat the chemical composition of the atmosphere remained always in its usual state.

PREDISPOSING CAUSES.

The *predisposing causes* of the epidemic re as little known to us as its specific ones. t does not first attack swampy unhealthy ountries, nor swampy unhealthy parts in ountries already affected. It does not ven enter, by preference, countries where iseases which are strongly predisposing r the *individual* are reigning.

Linz is a large town in Upper Austria, tuated on the Danube, ninety miles from ienna. I saw here a most violent epidemic f dysentery while the cholera was at ienna. The cholera has never yet been Linz, but it broke out in a small town lled Wels, twenty miles further than inz, in the *interior* of the *country*, towards e Bavarian frontier. A gastric nervous ver preceded and accompanied the appearnce of the cholera in Galizia, Hungary, d Vienna, and it was peculiarly destruce to individuals labouring under this fever. Yet when the cholera was at Wels, a nervous fever with diarrhœa was very violent in all Upper Austria and Bavaria. The cholera, after raging in Wels and the country round to a distance of six miles, at length disappeared, and from that time till the beginning of March no case was seen in Upper Austria and Bavaria.

The predisposing causes for the individual are, first, the use of too little flesh meat—then, innutritious food generally—swampy countries—dirty, damp, and overcrowded dwellings—and bad clothing. The progress of the epidemic in Galizia offers the most striking example of the action of these causes.

Galizia is inhabited by Poles and Russneaken, Jews, and German colonists. The Poles and Russneaken, especially the latter, are very badly fed and lodged. They seldom taste beef or mutton, living mostly on a coarse rye-bread, cheese, butter, milk, and sometimes a little fat bacon. They live with their cattle, under one roof, only separated from them by a partition. They are very dirty in their houses and persons, and are generally serfs. The Jews are still more filthy than their neighbours the Poles; their houses are much more overcrowded. But they are generally warmly clothed, and otherwise have more of the comforts of life. They are generally employed in commerce.

The German colonists are clean, and live much better in every respect than any of the others; they are occupied principally with the arts, commerce, and farming. These three people live in separate villages, but which are often so close together as to form but one town, or "marktfleck," as it called. When the cholera broke out, about one in every eight was seized amongst the Poles and Russneaken; about one in every forty amongst the Germans. The Jews, according to circumstances, approached more to this or to that extreme.

The proportion of deaths in those taken ill, remained always much the same. At least the difference in favour of the Germans was not very marked.

Another class of predisposing causes was, various diseases:—

1st. Those of the alimentary canal, as gastric fever, and diarrhœa; those changes which are produced by the long use of ardent spirits;—chronic diseases of the digestive organs. In an immense number of cases opened after cholera, cancer, and contraction of the pylorus, polypi of the mucous coat, &c. were found. The liver had often a lardaceous appearance. Enlargement of the mesenteric glands, &c. Worms were so frequent (lumbricus teres) that some physicians maintained that they were spontanously produced during the cholera.

2nd. Those of the nervous system, as nervous and intermittent fevers, madness in all its forms (but especially the furious), hysteria, &c.

3rd. Those of weakness, as cachexies of all kinds, especially scrofulous ulcers and caries, phthisis, and dropsy.

The clear and decided influence of these causes seems only to affect the extension and form of the epidemic—not its intensity and relative mortality. Cases occurred in Vienna as severe as in Hungary; and the number of deaths was nearly one-half of those taken ill. But while in Hungary every 9th man was attacked, in Vienna there was only one in 70. The only reason for the very small proportion of the sick to the rest of the population in Vienna was, the very strong and efficient measures taken to remove these causes. The streets were all cleaned; the houses of all the poor were visited, and proper food and clothing, and occupation in the open air, provided for them. Upon the whole, the proportion of the sick to the rest of the population was at least twice less than in Brünn, Olmütz, or Prague, cities which are all much smaller than Vienna, where there was infinitely less misery and poverty, and where the disease ought to have been less extended. But here the sanitary measures put so vigorously in force at Vienna were greatly neglected. In Vienna, during the existence of cholera, every person, who required it, had one shilling a day. At one period about 3000 persons were so relieved.

EXCITING CAUSES.

The *exciting* causes for the epidemic are not known. It was certainly very remarkable, that the great eruption at Vienna was immediately preceded by three days of severe cold rain and wind, after a number of weeks of very hot weather.

The individual *exciting causes* are, matters of difficult digestion, or food of good quality eaten in too great quantity, or in such haste as not to be well chewed. In the stomach of a great majority of those who died after a short illness (particularly in those of children) large pieces of food, generally potatoes or prunes, were found.

The intensity of the disease often seemed to be in relation to that of the exciting cause (that is in healthy subjects). If this was slight the patient had an ordinarily bad stomach, or he had all those symptoms described as the "prodrome" of cholera. If when in this state, or even when he was convalescent from real cholera, the patient indulged in a new excess, he was often in a few minutes seized with the worst form of cholera. This occurred very often amongst the well-fed, well-clothed, but disorderly, Hungarian soldiers. Indeed from this cause alone there were more cases of relapse amongst the 302 soldiers who had the disease than in all the other hospitals together. Wet and exposure to cold were often given by the patients as the immediate causes of their illness. The soldiers were almost all attacked while on guard during the night.

Fear is another exciting cause of the greatest force. A Dr. Bochie was sent into Hungary to treat the cholera, where he was in a short time attacked, and died. He had left a mistress at Vienna. A brother of the diseased meeting the girl in the street, said "Your friend has died of the cholera." The girl was much shocked, and before she reached her home she was seized with violent cholera, and died in six hours.

Going into the hospital one day, I found a strong healthy woman who had been at the moment brought in. She had just vomited a large quantity of half-digested flesh. On asking her to what she attributed her illness, she said that her master had a friend who had been taken to the cholera hospital, and that he had sent her thither to inquire how he was. As she was going along she was in great fear she should catch the cholera at the hospital; but long before she came near it she felt a sudden giddiness, became faint, and vomited. This proved a very severe case of the first form of cholera.

DIVISION OF SYMPTOMS AND FORMS.

The three following forms seem to me, by the difference in their symptoms and course, and by the difference of treatment required, to be the most natural, clear, and necessary:

1. The highest degree:—Seizure sudden; evacuations almost or altogether wanting; cramps most severe; ends in most cases in a few hours, the action of the heart often becoming imperceptible before consciousness is lost.

2. The most common form:—It presents the greatest variations, and the most numerous succession of symptoms, which at one time resemble more those of the 1st, and at others more those of the 3rd form. Almost always a prodrome; ends fatally by paralysis, the consciousness being lost; respiration the last vital function which ceases.

3. Often no prodrome; comes on with sudden and copious evacuations. The patient sinks rapidly and silently, without any violent symptoms.

First Form.

This form was frequent only when the epidemic was at its height. Men of a firm, hard constitution, with the muscular and circulating systems predominant, who had just reached, or were a little past, their prime, were much predisposed. The gin-drinkers who had not carried their practices so far as materially to affect their constitutions, were often affected by it. Women above 40, in these circumstances, were sometimes seized with it. It became more seldom in proportion to the distance from the above-mentioned constitution.

In general it occurred suddenly after some great excess. The patient felt a sudden giddiness, and dropped down, or he felt a tremendous pain in the region of the stomach, or a sudden stoppage of his breath. In most cases there was one or two fits of vomiting or purging at the moment of attack, or these were altogether wanting. In a few moments the severest tonic spasms attacked first the lower, then the upper extremities, and then the muscles of the abdomen, which were drawn in as flat and hard as a plank,—these were accompanied with great pain. Almost immediately the skin began to cool and to take on a blue colour, which speedily increased to an icy coldness and deep purple. The pulse, at first very irregular, became small, and then ceased, first at the wrist and then at the neck. The tongue was cold, and covered with a white crust. The voice became choleric in a high degree; the patient expressed the utmost longing for cold water; the thirst was excessive; the difficulty of breathing so great as to cause him to gasp for breath, and throw himself from one side of the bed to the other. This spectacle of anxiety and torture was really horrible. Yet during all this time the understanding remained perfect, but rolled in upon itself, the patient taking little notice of what went on around, or even of his own condition, his present sufferings seeming to absorb his entire mind. Death takes place for the most part suddenly, seemingly from the heart ceasing to act. It is almost impossible to obtain a drop of blood. Even if we open a vein at the very first, only about an ounce will flow. The stomach seems to be paralysed, as the strongest doses of ipecacuanha, even some drachms, with a large quantity of tartar emetic and other stimulants, produce no vomiting. It seldom lasts longer than from six to twelve hours. In cases of recovery, the reaction is very violent, and of an inflammatory character.

Second Form.

This was generally preceded by a prodrome, which came on with giddiness; loss of appetite; troubled sleep; convulsive twitchings in the separate muscles of the extremities; rumbling in the bowels, like water rolled about in half-filled barrels. But the most characteristic symptom was a diarrhœa, or rather a looseness; this was extremely constant, and might with more propriety than any of the other symptoms be called the prodrome of cholera. It often came suddenly after an exposure to cold, or error in diet; often without any obvious cause. It is never accompanied with pain or tenesmus. The ejected matter varies from a pultaceous consistence to a fluid nearly resembling that proper to cholera itself. It is seldom that a person has more than three or four stools in a day. In regard to duration, it lasts from an hour to many days; it often ceases entirely, and begins afresh. When it lasts for many days, the person is very liable to be attacked with the third form of cholera. An immense number of persons were attacked with these symptoms, even several times, but by taking proper care, the further development of the disease was prevented.

Copious vomiting, or purging of the peculiar clear fluid, mixed with flakes of a whitish substance, and accompanied with excessive faintness, marked the passage of the above state into real cholera; in a very short time cramps came on in the feet and calves of the legs. The pulse diminished in strength and frequency; the respiration deep, and accompanied with much effort. The voice peculiarly altered; the skin cold and blue; all the various secretions suppressed; the thirst very violent. After these symptoms had continued a shorter or longer time, if the patient was strong and undebilitated, either the vomiting ceased suddenly, and he passed into the state described in the first form, or the vomiting continued, and became of a grass-green or yellow colour; the pulse rose, became full and strong, and, often, active congestions were formed to various organs. A tormenting hiccup frequently followed or accompanied the bilious vomiting.

In patients of a weaker constitution, (women, youths, children, &c.) quite another order of symptoms succeeded,—the paralytic state. The cramps and evacuations ceased; the patient lay quite still and powerless on the back; the consciousness was lost; the pupil dilated, or sometimes strongly contracted; the eyes half closed and turned up so as to show the white, which was often strongly injected on that part which was exposed to the air. The pulse rose a little, so as to be again felt at the wrist, but remained very weak. The respiration was performed at intervals by gasps; the skin was cold, and generally covered with a cold, clammy sweat, which even exuded from the pores after death. Patients in this state generally sank in a few hours by the gradual cessation of the pulse, and then of the respiration, which at the last was only kept up by two or three convulsive gasps in the minute.

In others the consciousness was not quite lost, being more a state of stupor; the pulse increased in strength and frequency, the temperature of the skin rose, and the cheeks flushed of a bright scarlet colour. These states often lasted for a day or two, during which time they frequently (of themselves, or from means used) showed considerable changes. At one time the stupor or paralysis, at another the affection of the circulating or hepatic systems, were more predominant.

Third Form.

This occurred in people of the very weakest constitution, who were highly scrofulous, or nearly exhausted by any severe disease, especially one of a chronic nature. It came on with a sudden and copious discharge of the fluid peculiar to cholera, in greater quantity, and often exclusively per anum. This lasted a very short time, when the forces sank, the patient was seized with fainting fits, pains in the abdomen, and oppression in the breast. The features became haggard; the cramps were seldom violent, sometimes wanting, and, without the skin becoming blue or losing much heat, the pulse and strength of the patient rapidly sank, and he died without any violent symptom. This form often ran its course, rapidly, in from six to twenty-four hours; the patient often fell into a state of stupor, or imperfect nervous fever, which lasted two or three days, and then ended in death. In a few rare cases it lasted longer, and other diseases developed themselves.

PROGNOSIS.

The passage of the second form into the first, or into the state of paralysis, was always bad. The forerunner of this was a sudden cessation of the evacuations. The discharge of a pink-tinged fluid per anum was a very fatal sign; a thick, cold, clammy sweat was always dangerous. When the clammy sweat broke out with a sudden development of heat and return of the pulse; it was often a forerunner of death in the paralytic state. Gradual return of the pulse, of the temperature and moisture of the skin, the occurrence of bilious vomiting, still more the return of the secretion of urine, and the passage by stool of a thick tarry-looking substance, were all signs and accompaniments of a moderate reaction and favourable termination. It was only when these were present that no other disease developed itself as soon as the cholera had ceased. Even when it terminated thus favourably, a bilious vomiting, a hiccup, returns of the diarrhœa, or an obstinate costiveness, often gave much annoyance to the patient.

DISEASES IN WHICH IT TERMINATED.

There was a regular gradation in these diseases, from a peculiar soporose or adynamic state—which was seen after the third form, and came on immediately as the symptoms of cholera ceased, and which did not show any perceptible affection of particular parts, and were therefore to be regarded as the consequence of cholera alone—up to those cases of almost pure inflammation of some particular organ, which followed strong reaction, in robust individuals. During their course, almost always typhoid symptoms came on;—they often ran their course, rapidly and silently; producing much disorganization, without evident signs of the mischief, such as full strong pulse, much heat of the skin or pain in the part. Indeed, the patient often lay without complaining, was obstinate and sluggish, would not be disturbed, and paid not the least attention to what was going on around him. It was not the strength of the patient and the particular form of cholera alone which modified these diseases. The effect of the treatment employed during the cholera itself, was very striking. When ice was used, the reaction was always very violent, and the congestions to particular organs very strong; but these were of an active character, and therefore more under the control of the physician. The indiscriminate employment of strong stimulants in large doses, such as opium, camphor, musk, ether, oil of cajeput (especially in the active forms of cholera), was followed so constantly by a dangerous soporose state, that after some time they were almost entirely neglected by all the physicians of Vienna.

Affections of the Brain.—These bore a greater or less resemblance to encephalitis; they occurred most frequently in women, almost always after the paralytic state. They often ran their course in a few days. The principal symptoms were, delirium, pain in the head, ringing in the ears, giddiness, &c. The pulse was small, sharp, and quick; the cheeks flushed of a bright rose colour.

Congestions and inflammations in the substance of the lungs were seldom attended with copious expectoration, nor did the patient complain of pain. The most characteristic symptom was the hurried and anxious respiration, and the absence of the respiratory murmur. It occurred mostly in strong men, in whom the reaction had been violent; but it was, even amongst them, of but rare occurrence.

Determinations to the intestines took place more or less in every case, and complicated all the diseases which followed cholera, so that sometimes when the determinations to the brain, &c., had been successfully combated, the patient died at a later period with typhoid symptoms, in consequence of the affection of the stomach or bowels.

The termination in dropsy was rather rare; only at one period it became very general; it went off easily when the other bad symptoms were overcome.

The other diseases which developed themselves after cholera, depended much on individual habit; four cases of delirium tremens were seen in hard drinkers, three died, one recovered under the use of opium. In young men there occurred several cases of epilepsy. In women, a good deal of hysteria, and in some, anomalous contortions of the body resembling chorea. In the scrofulous, the glands of the neck often swelled and suppurated. In two or three cases there was inflammation of the parotid; three cases of nephritis, &c. Acute hydrocephalus was very common in children of scrofulous habit.

MORBID APPEARANCES AFTER DEATH.

The only constant and peculiar changes which were never absent, and which were found in every form of cholera, were a specific change in the blood, and in the mucous membrane and glands of the pharynx and intestinal tube, with a corresponding change in the contents of the latter. The other changes, except perhaps the hardening of the nervous matter, seemed only accidental or secondary.

The blood was, in substance and appearance, very black, thick, and grumous. It looked as if the colouring matter alone was left, with as much serum as to make a consistence like syrup, because it was rough and grainy. A few clots of coagulable lymph were generally found in the heart. The veins of every part of the body were so distended with this blood, as to give many organs the appearance of active congestion.

The mucous coat of the intestines seemed always, in acute cases, as if swollen in its whole extent; from place to place it was of a bright red, from numerous vessels—(a consequence of inflammation or passive congestion only?). In other parts it seemed even paler than natural. The mucous membrane of the pharynx was often of a deep purple, from the injection of its vessels.

The contents of the intestines were either a thick tenacious mucus, sometimes as hard as coagulated albumen or curd (it was of a whitish colour, though tinged of a pink hue, opposite those places where congestion had taken place), or another colourless fluid quite clear, resembling serum ("rice-water-like liquid"). These two substances were mixed together in all proportions, and they were tinged of all colours, from the admixture of fæces, bile, or exuded blood; so that sometimes the flakes of the first substance floated in, or sank to, the bottom of the liquid, which remained clear; at other parts they were so blended as to resemble milk, or even cream. In severer cases they were thus intimately mixed, and tinged of a bloody hue.

The thick mucus seemed, from what I have seen, to be produced first, and then the thin fluid. The thick mucus was always in relation to the enlargement of the glands; the thin fluid, to the changes in the rest of the mucus. If the patient had died after a few hours illness, the glands of the pharynx and back of the mouth, those of the intestines, from the cardia to the anus, were much, but simply, enlarged; those of Brunner were elevated above, and stood clear out from, the mucous membrane; those of Peyer were raised about half a line or a line, and their surface was uneven. They were always of a pale colour, and of a uniform texture when cut into; they stood in no relation whatever to those parts of the intestine where congestion had taken place; but they seemed to have some connexion with the production of the thick mucus, because the glands were most developed in those subjects, and in those places where this mucus was most abundant. When the contents of the canal were more fluid, these glands were no longer so distinct.

When the patient died some time after the cholera had ceased, the glands were found in a different state; they were no longer so much raised above the surrounding membrane; they were more flattened down; the separate glands had a small black spot on their apex, and often contained a thin white fluid something like pus. The aggregate ones were quite dotted with black points, some as large as a pin's head; the mucous membrane was much swollen, and the congested parts were of a darker hue. The glands of the pharynx at this time contained pus when cut into, and they often had a ragged appearance as if from the commencement of ulceration. The period at which these appearances were seen, varied very much. At a still later period, when the patient died after very strong typhoid symptoms, the mucous membrane of the stomach and bowels was covered over with a tenacious semitransparent mucus, about one line deep. It was often itself so softened as to form but one substance with it, and on attempting to remove the mucus from the membrane with the back of the scalpel, the membrane was so softened as to come away with it and eave the cellular coat bare. At this time it was difficult to find a trace of the glands. In those places where the mucous coat was of a firmer consistence, it was often of a bright-red colour, from patches of injected capillaries, which were quite straight, about a line long, and could not be traced into the submucous coat; they looked like injected villæ. The whole mucous surface had a green-black colour, caused by the deeply injected cellular coat shining through it. This last was seen of the darkest crimson when the mucous coat was removed. This softened state of the mucous, and strong injection of the cellular coats, seemed to stand in a fixed connexion with the typhoid symptoms.

The great *nervous masses* were often found changed in cholera, but in no constant or peculiar manner. When the patient had died in a few hours, from the first form, the hardening of the nervous matter in every part was very remarkable. It was less and less as the disease had approached the third form, when a quite opposite state was often found—the brain, &c., being softened, and containing a good deal of serum. After the paralytic state, and when the patient had had symptoms of encephalitis, the brain was often much gorged with blood, and exudations of lymph of a gelatinous consistence were frequently found under the arachnoid, as well as large effusions of serum in the ventricles.

The changes in consistence, size, and appearance, in the semilunar ganglion (which was in every instance examined), were never constant, and stood in no relation whatever to the differences of the particular cases. Sometimes it was pale, sometimes red, large or small, hard or soft, &c. The medulla spinalis was like the brain, often hardened, and its veins, particularly towards its lower end, very strongly congested. The serum in the spinal canal was almost always increased. The lungs were most altered in the first form, when, in the middle of their substance, patches of a bright vermilion were found, which contrasted strongly with the very black congested state of the other parts. In some cases they were dry when cut, in others they were filled with a foamy serum. At a later period, if particular determination had taken place upon them, they were of a brown-red colour, they were uncrepitating and friable, and on cutting them much serum flowed out. The trachea and bronchia were throughout of the same colour. At this time, also, effusions of serum were found in the pleura and pericardium. The *heart* and large vessels remained perfectly healthy, with the exception that black spots of ecchymosis were often seen near the base of the heart. The liver, pancreas, spleen, &c., were hard, dry, and contracted, after the first form. The gall-bladder was always filled with gall, of various colours and degrees of consistence—its ducts remained open. The kidneys were unaltered, the bladder was hard, contracted, and contained only a few drops of a dirty white urine. The serous and cellular tissues were remarkably dry in cases of short duration, and it required the greatest force to overcome the tonic contraction of the muscles—even the diaphragm and the intestines were so contracted.

TREATMENT.

Treatment of First Form.

In the most violent cases of the *first form*, all treatment whatever was found unavailing. Bleeding always produced relief, and if, by means of ipecacuanha, or other emetics, or by ice, vomiting was produced, the patient immediately found himself much better. But, in most cases, it was impossible to produce vomiting by any means, and the patients were all lost.

If those symptoms, described as the *prodrome* of cholera had come on in consequence of exposure to wet or cold, the best effects were seen, if the patient went into a warm bed, took a dose of Dover's powder, and drank an infusion of elder or camomile flowers, until full perspiration was produced. If they had been caused by an excess at the table, if nausea came on, if the diarrhœa became very frequent or copious, with cold extremities or cramps, the greatest advantage was obtained from an emetic of ipecacuanha; afterwards an infusion of the same root, ℈j to ℥vj of water, with ten to twenty drops of laudanum, a table-spoonful to be taken every hour. In a day or two, when the purging still continued, the infusion of columba was substituted for that of ipecacuanha, and the quantity of laudanum increased. Under such treatment it was very rare that these symptoms went further.

Treatment of Second Form.

If the patient was strong, the very best effects were seen from bleeding; and then the administration of a large dose of ipecacuanha, which did great service in all cases but those where, from the patient's excessive weakness, it was contra-indicated. If the first dose of one scruple did not act it was repeated, and in the interval a few grains of musk and camphor were given, which were found to assist the action of the ipecacuanha. These were continued till the temperature and pulse began to rise, and the skin to assume its natural colour. In the same circumstances the use of ice was followed with great relief. The patient received it in pieces as large as he could swallow, and his skin was rubbed with large pieces, and afterwards well dried with rough cloths, and the patient put in a warm bed. The first effect of the ice was to produce vomiting, which was soon followed by increase of the temperature, &c. Even in cases where the thirst, cramps, and oppression of breathing, were most violent, and the skin already cold and blue, with no pulse at the wrist, a reaction was produced by the employment of these means.

Treatment of Third Form.

In this form the bleeding, ipecacuanha, and ice, did more harm than good; most benefit was obtained from opium, by the mouth, and in injection with yolk of egg, camphor, musk, and especially the infusion of arnica.

Sinapisms were found of great advantage in every form; they were applied to the calves of the legs, and to the pit of the stomach, when the cramps and pain were severe. When the abdomen was tender to the touch, leeches and warm fomentations had a good effect. Vapour-baths, hot sand, hot-baths, were seldom productive of much good. In a few cases a warm-bath, in which caustic alkali had been dissolved, produced a reaction. Hot and stimulating frictions relieved the cramps in the extremities, but had no effect in raising the temperature.

Here then stand on the one side bleeding, emetics, ice, and sinapisms, and, on the other, opium, brandy, camphor, oil of cajeput, sulphuric ether, hot frictions, baths, &c. These last were, at the commencement of the epidemic, almost exclusively employed. But in so far as the former were found to stop more effectually the course of the disease, to produce reaction, and to give an active character to those congestions which afterwards developed themselves, the use of the latter was almost unanimously abandoned by the physicians of Vienna, except in those cases of exhausting diarrhœa, or vomiting, described as the third form of cholera; for though they sometimes put a stop to the symptoms of the disease, and even roused the patient out of those desperate states of congestion or paralysis in one or two cases, yet their use was always followed by the sopor, and the worst forms of typhoid fever. Even in the third form they were given only in small doses, never in such quantity as was necessary to produce reaction in the 1st and 2nd forms.

With the exception of the employment of ipecacuanha and ice to produce reaction, the physician must be guided by the general rules of practice, as there is perhaps no disease in which the treatment must be more varied, according to the circumstances of the case.

Treatment of the Sequelæ of Cholera.

When the bilious vomiting and hiccup were severe and long-continued, the best remedy was subnitrate of bismuth and ext. of hyosciamus; three grains of each were given every two hours till relief was obtained. Effervescing draughts. The sulphuric and nitric acids, in decoction of salep, or althea, were often equally useful. When vomiting and diarrhœa returned during the convalescence, they were treated with success in the same way as the prodrome.

The patients were bled with the greatest advantage in those cases where the congestions were clear, and accompanied with strong reaction. In other cases, where the patients, on coming out of the cholera, fell into a state of stupor, or incomplete nervous fever, it was often difficult to determine if depletion or stimulants should be had recourse to. The one or the other was most successful, according to the individual circumstances of the case.

When the symptoms of encephalitis came on, leeches to the temples, ice in bladders to the head, and sinapisms to the lower extremities, produced the best effects. In the few cases of inflammation of the lungs, tartar emetic, &c., were employed. When the belly was painful to the touch, leeches and sinapisms were employed; these were necessary in almost every case.

When, after several days, typhoid symptoms showed themselves, such as delirium, black crust on the tongue, &c., no more good was derived from the continuance of the antiphlogistics—but, on the contrary, the most marked good effects, from small and frequent doses of musk and camphor, and opium, with infusions of arnica and valerian. When the patient remained weak, and had frequent returns of purging and inclination to vomit, the best effects were seen from an infusion of columba, gentian, cinchona, or other tonics, with a few drops of laudanum, and a diluted mineral acid. For a long time the least exposure to cold or wet, or the least excess in eating or drinking, brought on a return of the diarrhœa. The constipation, which often followed cholera, was best removed by a few doses of castor oil.

London, March 30, 1832.

LETTER

TO THE

PRESIDENT OF THE ACADEMY OF SCIENCES OF PARIS,

ON THE

DANGERS ATTENDING THE USE OF M. HEURTELOUP'S INSTRUMENT FOR BREAKING STONE.

By W. B. Costello, *Esq., Surgeon.*

PRELIMINARY LETTER TO THE EDITOR.

To the Editor of The Lancet.

Sir,—Feeling as I do a deep interest in whatever affects the justly high repute, or the prosperous progress, of lithotrity, I took the part which became me—namely, to submit my reflections on M. Heurteloup's instrument and system of percussion, as soon as they were proposed to the Royal Academy of Sciences. The memoir submitted to that learned body having been since in part published in your valuable Journal, I am induced, through the same motive, to forward to you for insertion in your next Number, my letter to the President of the Royal Academy. The observations contained in that letter would not have been presented in the present form, had not the result of my labours been grossly misrepresented to the Academy. I should, however, have stated my objections to the proposed instrument under any circumstances, though, if I had not been compelled to it by the right of legitimate defence, I should have refrained from giving to my case any other shape than that which it would have assumed on rational and theoretical grounds.

I am but maintaining the ground on which the true friends of the science would desire to stand. I have never denied that any ingenious cutler may produce, not merely one, but fifty different forms of the lithotritic apparatus. In proof of this, only see the vast number of forms in which they have been produced, and the variety of titles with which they have been decorated, in the short space of six or seven years. Every new form of these instruments, according to the inflated promises of its deviser, *was to be exempt from the defects of those that preceded it. In short, it showed for the moment the "ne plus ultra of surgical power in lithotrity!"* Yet how have these promises been realised? Why, the very authors themselves, instead of being satisfied with the possession of this *ultimatum* of surgical power, have gone on in what is called the career of discovery, till they have resuscitated an obsolete instrument. Leaving the safe and simple instrument of Civiale, they have produced complicated and dangerous ones, and not satisfied with the effect of these, they now return to ransack the antique surgical magazines for novelty. For a man's amusement there can be no objection to this; but when the creations of his fancy are periodically thrown off, with a volley of pompous and unrealized promises, this serious inconvenience is the result, that an apparent abundance of the implements begets hesitation in our choice; and experience cannot pronounce in favour of any one of them, for they have been all praised, each in its turn, "as the very best." Seriously I entertain a strong objection to frequent changing of instruments, and considerable distrust of the surgeon's ability, who is constantly veering about from one instrument to another, particularly when he already possesses one which may be used safely,—as it has been used successfully in upwards of 250 cases, amongst which will be found the names of the most distinguished surgeons of the day, Professor Baron Dubois, M. Boisseau, the celebrated Lisfranc of the Hospital of La Pitié, and many others. Ask any lithotomist who uses *the knife* why he does not try the gorget. He answers because he is satisfied with the instrument he commonly uses. If I am asked why I do not use the brise-pierre, the four-branched instrument, the complex drills, &c., &c., my answer is, Because, independently of these instruments being defective and dangerous in themselves, I am satisfied with an instrument in favour of which experience has already pronounced itself so strongly. If in my letter I have spoken with warmth, and introduced facts which otherwise I should never have noticed, it will be recollected that this is not the first time I have been compelled to refute misrepresentations of the results of my practice.

I have the honour to be, Sir,
Yours, &c.
W. B. Costello.

7, Parliament-street, Westminster,
June 16, 1832.

To the President *of the* Royal Academy of Sciences of Paris.

Sir,—I have learned from the journals the communications made to the Academy of Sciences by M. Heurteloup, respecting a new means of curing stone. Without pronouncing an opinion on the value of the means thus proposed, I deem it proper to refute some of the incorrect assertions contained in the paper of this surgeon, and at the same time to submit a few remarks to the Academy.

In the cases of cure mentioned by this gentleman, two are mentioned as cases in which I had failed. This assertion is incorrect. Since my return to my own country, only three patients out of the great number who consulted me, did subsequently address themselves to my confrère. The first of these I visited in the country, on my way to Edinburgh. After having made an exploration, it was settled that he should go to London in order to have lithotrity performed on him. On his arrival in town, I had not yet returned. This patient placed himself under the care of my confrère, who operated on him, and he died. This gentleman was Captain W. The second patient who left me for M. Heurteloup, had paralysis of the bladder, and a stone which held a fixed situation in that organ. This patient had been cut by Mr. Brodie three years before. I made an attempt with Civiale's instrument; I experienced no difficulty in seizing the stone between two of the branches of the forceps, and detaching from it considerable portions, which my confrère must have seen, but I was not able to dislodge the stone from its fixed situation beneath the neck of the bladder, and which appeared to me to be that of the old cicatrix. This attempt was renewed with a similar result; each attempt occupied about five minutes. * I explained the difficulty to the patient, and proposed to dislodge it by means of Hunter's two-branch forceps. He was, however, intimidated by officious observations, and he placed himself under the care of M. Heurteloup, whom I had previously proposed to call in consultation. The indication of dislodging the stone, whose fixity in this case formed the sole obstacle to its comminution, was fulfilled by the means which I had previously proposed. This impediment once removed, of course the cure became easy. This patient was restored to health, at which I sincerely rejoice; and far from thinking that I had failed, he came afterwards to thank me for my attentions to him. It was admitted that my confrère and myself were of the same opinion as to the difficulty of this case, and the means of obviating it. So much for the imperfection of the means employed by me. I shall speak of the third case in the sequel.

I wish I could refrain from the reflections which I have to submit to the Academy with regard *to the curved percuteur hammer.* But the truth, as regards this instrument, is due to the community, and to M. Heurteloup himself, who, once disabused of his prejudice in favour of it, will apply his talents to weaken the impression against lithotrity, which an unfortunate trial of the *curved percuteur* has created in the public mind in this country. This truth is also due to the Academy itself, as the eulogy which it has betowed on this instrument has caused no small surprise.

I shall first of all observe, that this instrument is not new. I saw in 1826 an instrument, similarly constructed, in the possession of my countryman, Dr. Anthony L. Fisher, of the Rue Louis le Grand at Paris, and it was not invented by Dr. Fisher, for it was a very old instrument. If Dr. Fisher be still in possession of it, I am persuaded he will feel a pleasure in submitting it to the Academy. Reason and experience agree in proving this instrument to be not only defective, but even dangerous. I do not deny that a stone may be fragmented, and that patients may be cured by the use of it. All I contend for is this, that if eight calculous patients are stated to have been cured by it, the Academy ought not to be allowed to remain in ignorance of the danger, to which they have been exposed to obtain this benefit, when these sufferers had an alternative, in submitting to which they incurred no risk whatever. In fulfilling this duty, I shall not suffer myself to be swayed by a recollection of the injustice with which my labours have been alluded to in the Academy.

There are few surgeons in London who are not now aware, that Colonel Rankin was operated on for the stone with the *curved percuteur.* The particulars of this operation were related to me by the celebrated surgeon who met the other medical gentlemen in consultation on this case. The particulars are as follows: the instrument was introduced, and the operation proceeded satisfactorily for some time. It was now observed from the emotion of the operator that something was wrong. The instrument could not be withdrawn; a strong pair of forceps was asked for and procured. An incision was now made in the perineum, and the blood flowed copiously from the divided artery of the bulb. The instrument had been bent in the bladder; a consultation had been instantly held, and it was agreed to complete the cutting operation, and to withdraw the instrument and the stone at the same time. This was done by Mr. Brodie. The Colonel had been a long time on the table, he lost a great deal of blood, and ultimately died.*

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* In a recent number of The Lancet it was stated that I made three attempts, of a quarter of an hour's duration each. I made but two, of about five minutes each.

* Having misunderstood the particulars of this operation as they were first related to me, I take this opportunity of correcting my statement. The bent branches having been brought out through the

To this well-known and well-authenticated fact, it is scarcely necessary to add anything. It was easy to foresee that the maximum of the power must have borne against the anterior curve or branch of the instrument. The case above narrated demonstrates it. It is, however, important to add, that, if I am well informed, this is not the only case in which a misfortune of a nearly similar nature has occurred. Experience then has proved, that the use of this instrument is not exempt from danger, since it is liable to be bent or broken by the violent shocks communicated to it by the hammer. This being established, I shall not undertake to prove that this instrument is, almost in all cases, unnecessary.

I now come to the third case, which fell under my confrère's care after I had been previously consulted, and I shall give it at some length, because it may be fairly presumed that M. Heurteloup's first idea of this instrument is closely connected with it. I should have felt it my duty to preserve silence on this subject, if I had not been called on to correct misrepresentations which have been made, not by any means now for the first time, and if my statement was not calculated to throw light on the question of the modifications of the lithotritic apparatus.

I freely acknowledge that formerly I was myself strongly prepossessed in favour of new modifications, a prepossession which the encouragements granted by the Academy of Sciences had confirmed. In the hope of advancing the science on this point, I produced divers modifications, on the advantages of which I reckoned with a degree of confidence which caused me to undervalue the means already known. I subjected these modifications to the test of experience, and it was not until I became fully convinced of their inutility, and even of their danger, that I renounced the use of them. In narrating this fact, I disclaim all intention of depreciating the labours of a confrère, whose dexterity I acknowledge. I wish merely to express my conscientious opinion, that the most of those modifications have been imagined and executed, without even excepting my own, by authors who did not appreciate at their just value the means already in use. Let me also avow it, the hope of bringing back a labourer in the same good work, to the conviction which I have myself obtained by experience, of the perils of employing complicated means, forms no small ingredient of the motive which induces me to speak out on this occasion. Such a conversion were well worth the trouble, for I am firmly persuaded, that it is only when he shall have renounced the use of complicated instruments, that he will render to humanity and lithotrity the services which both the one and the other are entitled to expect from his talents. But to the fact:—

In the course of the year 1830, a patient labouring under stone in the bladder, accompanied by a gentleman whom I had previously relieved of this complaint, came to consult me. The patient being satisfied that his disease was stone, and being unwilling to be sounded, catheterism was not resorted to; it was merely agreed, that as soon as his harvest was got in (he was a farmer), he should come to town to undergo lithotrity. His harvest was early, and he came to London while I was absent in Edinburgh. He addressed himself in consequence to M. Heurteloup. A sitting took place, the result of which was satisfactory; it was however followed by irritation, which was subdued by bathing and leeching. A second sitting was attempted in the presence of a considerable number of medical men, but on this occasion it was found to be impossible to pass the straight instrument into the bladder. This was in the month of September. From this time to the month of February 1831, a great number of attempts were made, but the instrument could no longer be made to penetrate into the bladder. Each attempt was followed with great irritation, and infiltration of blood in the perineum and scrotum. The health of this patient, at first robust, was broken. He returned home. I was invited to see him. On my arrival at his house, I wished to be informed by his medical attendant (Mr. Nunn) of the motive which prompted his patient to send for me. He replied, "Many unsuccessful attempts have been made to pass a straight instrument into the bladder; his former surgeon states that it impossible, owing to an enlargement of the prostate gland; he is not disposed to submit to the cutting operation, and he now wishes to ascertain from you, if the straight instrument cannot be passed." In the presence of Mr. Nunn, and several persons of the patient's family, I instantly passed a straight lithotrite into the bladder, and made Mr. Nunn feel that it was in contact with the stone!!! The possibility of operating on this patient by means of the straight instrument being thus ascertained, I deemed it my duty to trace out a plan of treatment for the re-establishment of his general health. I should observe, by the way, that I have not met one out of the vast number of cases, in many of which the prostate was enormously enlarged, which

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wound in the perineum, were closed, and then withdrawn through the urethra. What remained of the cutting operation was performed by Mr. Brodie, who extracted the stone.

have occurred to me, either during my connexion with Dr. Civiale, or since my return home, in which I have not found it not only possible, but even easy, to introduce a straight instrument. I infer from this, that such an occurrence must be rare indeed.

In the following week the patient received a letter from M. Heurteloup, from which I subjoin an extract, carefully refraining from commenting on the delicacy of such conduct in a professional point of view.

"He (M. Heurteloup) would also have been greatly obliged to you, Sir, had you communicated to him sooner the decision you had come to; for since Mr. B. and himself had decided that nothing could be done with the instruments then possessed by him, and that it was absolutely necessary to find other means of penetrating into your bladder, which would not admit of the introduction of straight instruments, he had, expressly for you, *proceeded to discover* other means more suitable; by not writing to him you have therefore allowed him to continue his labours, and he has now completely terminated an instrument, without which, experience in your case (experience of an instrument he had not yet used! ! !) has told him, that you cannot be treated for your complaint, without exposing yourself to danger."

Had I not proved the possibility of introducing the straight instrument a week before, this letter would have struck terror into the poor sufferer's mind. He and his medical attendant being quite satisfied on this point, this extra-professional piece of conduct was suffered to pass unheeded, though not uncommented on by them. Doubtless this opinion as to the impossibility of passing the straight instrument was given in perfect sincerity; far be it from me to dispute it. But most assuredly this case did not require for its treatment any other instrument than that of M. Civiale; the proof of this is, that I operated on him three weeks after, with a straight instrument, three lines and a quarter in diameter, and that the introduction of it was, as usual, easy. On this occasion I completely reduced to powder the stone which my confrère had commenced an attack on six months before. Henceforward the patient's sufferings, which up to this time had been most acute, ceased almost entirely. Another calculus which the bladder contained was also attacked. This was the only sitting I held on this patient: he died five weeks afterwards. Having observed that his sufferings in the bladder had nearly ceased after this sitting, we flattered ourselves with the hope of saving him. He even took a walk in his garden the day before his death; but this hope became daily less, on observing the urine resume its bad quality, diminish in quantity, and the skin, pale and flabby, exhale a sweat of a highly urinous odour. The autopsy was performed by the surgeons of the neighbourhood, who had the kindness to send me the particulars, as well as the pathological pieces themselves. The following are the chief points which this *post-mortem* examination presented. The urethra and prostate were healthy, the latter somewhat enlarged. The urethra at the neck of the bladder admitted with facility the introduction of the end of the little-finger; the bladder was thickened considerably at its sides and bottom; the rectal wall was natural; the mucous membrane was perfectly healthy throughout its entire extent; the anterior part of the trigona presented a slight incrustation of calculous matter. The bladder contained a small flat calculus of the size and shape of a peach stone, which presented traces of the action of the drill, and weighed two and a half drachms six grs.; the ureters were somewhat dilated. The right kidney was completely disorganised, and resembled a spleen divested of its capsule. The left kidney was three times its normal volume, and was filled with amorphous substance resembling plaster; it contained a small quantity of dirty, purulent fluid; its consistence was soft, its colour a livid brown. The rest of the body offered nothing remarkable. This document is signed, Wm. Nunn, Surgeon; Edward Barber, Surgeon; Frederick R. Hitch.

I shall offer no reflection on this case; it would be superfluous. Mr. Stockbridge would have been relieved of the stone by Civiale's instrument, and his life would have been saved, had not his kidneys, from long-continued irritation, been so profoundly disorganised.

I avail myself of this opportunity to communicate to the Academy the results which I have obtained by lithotrity in England. I practise this operation with the instrument, and after the operative process, of Civiale; for this reason, that it presents the advantages desired, and greater security than any other. I have operated here on nearly thirty patients with success. I have been consulted by a still greater number, in many of whom the state of the general health proscribed the operation; on others, it was necessary to have recourse to an exploration, before a decision could be arrived at. On the whole, I should say of this process, that it is not applicable to all cases of stone in the bladder; but it possesses the great merit in my eyes, that in the cases in which it is not applicable, the exploration made to ascertain this, does not aggravate the state of the patient. If it accomplishes no good, it does no harm.

Can the same be affirmed of the new modifications? For my part. experience has already resolved this question in the negative. It is to be feared that the profession in general may adopt against lithotrity, such as it is practised with a simple and solid apparatus, prejudices which are only well grounded, when we consider the results obtained by the use of complicated instruments and methods. Of this a judgment may be formed from the following fact:—Some time ago I asked a London hospital surgeon why he did not give lithotrity a trial; his answer was, "Why should I? Since I have seen it performed, I entertain a better opinion of the cutting operation. I know already six patients who have been operated on by lithotrity, of whom five died, and I have seen nothing like that from the cutting operation." My only reply was, that I had not seen anything like it in the practice of M. Civiale.

I offer you many apologies, Mr. President, for this long *exposé*; but I thought the importance of the subject required details. This question has never been to me a question of persons, for I acknowledge myself ready to receive, I care not from whom, any instrument or operative process which shall appear to me really to possess the characters of utility, simplicity, and safety, which this delicate operation requires.

I have the honour to be, Sir, yours, &c.

William B. Costello.

Westminster 7, Parliament Street,
March 10, 1832.

TWO CASES ILLUSTRATIVE OF THE

PERCE-PIERRE EVACUATING CATHETER,

AND THE

NEW MEANS OF BREAKING FRAGMENTS IN THE

URETHRA.

Treated by Baron Heurteloup, *D.M.P.*

CASE I.

General M—, 59 years old, of a good constitution, having, with little interruption, enjoyed a favourable state of health, was seized about a year ago with pain in his loins, and observed at the bottom of his urine a considerable deposition of brownish powder. The patient immediately consulted his medical attendant, who, by an appropriate treatment obtained a cessation of these symptoms, which, however, were shortly afterwards followed by others of a different nature, such as a frequent desire to make water, difficulty in executing the function, and an irregularity in the stream, which clearly denoted the existence of a stone in the bladder. The sensation of tingling or shooting, at the glans penis, which is usually a predominant symptom, was wanting. These symptoms at first caused little inconvenience, but becoming more and more severe, the patient considered it necessary to seek farther advice. He again addressed himself to his surgeon, who, concluding from the symptoms that there was stone in the bladder, sounded, and distinctly felt a calculus, which, on account of its apparently small dimensions, he proposed to extract from the urethra with a pair of forceps. The operation was undertaken, but notwithstanding the most skilful and careful manœuvres on the part of the surgeon, he could not grasp the stone, and thought it most prudent to cease making any further attempts.

The patient was much fatigued by this operation, and had a good deal of fever; the bladder became more irritable, and secreted a considerable quantity of mucus, and the want to make water was more urgent and more frequent. In about six weeks these unfavourable symptoms were greatly abated, and the patient, with the exception of being reduced and weakened, was in nearly the same state as before the operation; he remained quiet for about two months to regain his strength, and according to the advice of Mr. Brodie, whom he consulted, placed himself under my care to be operated upon by lithotripsy.

Having sounded the patient carefully with the *recto-curvilinear catheter*, I discovered a very small urethra, a moderately capacious but irritable bladder, in which was a calculus, nearly round, and from seven to nine lines in diameter. Considering the *perce-pierre* sufficient in a case of this nature, I thought fit to employ it.

As soon as the instrument was introduced, the stone was seized, perforated, and then broken by the pressure of the branches. Some pieces and powder were evacuated immediately, and on the next day; but the day after that, when I visited the patient, he complained of an unusual pain in the urethra. I examined with my hand, and felt on the outside several successive eminences arranged like the beads of a necklace. It was evident that these were caused by fragments of stone lodged in the urethra, and placed the one behind the other. I immediately had recourse to appropriate means for extracting them, and having broken, in the urethra, the first, which was the largest, and which prevented the others from passing, I removed altogether seven or eight large pieces, which, with what the patient had already voided, formed a calculus of the diameter above mentioned, composed of lithate of ammonia, and the mixed phosphates.

From this time (in the month of January) the General has been quite free from pain, the urine is clear, and voided with facility, and the only difference to be observed in his appearance is, that he has acquired a sensible increase of *embonpoint*.

CASE II.

Mr. Joseph Woodall, aged 22, who has been entirely free from any disease except the small-pox, experienced, about five years ago, pain in the loins, which has continued ever since with more or less severity; his general health was not, however, materially affected, for he was able during the last year, to discharge the fatiguing duties of traveller to a mercantile house.

A few months back, whilst suffering from an increased paroxysm of his usual pain, he felt, one day after dinner, a sudden and most violent desire to make water, which he was compelled to satisfy immediately, and he voided with pain and difficulty some urine deeply coloured with blood. From this moment the passing of the water was always attended with a burning sensation at the neck of the bladder, and all along the urethra, and frequently the urine was bloody; the stream was always irregular and incomplete, but was never suddenly and entirely stopped.

These symptoms induced Mr. Woodall to consult Mr. Ondknow, a distinguished surgeon at Nottingham, who sounded the patient twice. The first time, the bladder being empty, the stone was indistinctly felt, but on the second time of being distended, the calculus was clearly and accurately recognised, and Mr. Ondknow did me the honour to recommend the patient to my care.

A careful examination with the *recto-curvilinear catheter* indicated a moderately capacious urethra, except at the orifice, where a small incision was made, which allowed a tube of three lines and a half to be introduced; the bladder was regular, moderately sensitive, and contractile, and of the usual dimensions, containing a small calculus so moveable, that it was difficult to examine it with any degree of accuracy.

On the 25th of April, two days after the sounding, the patient came to have the stone pulverised. It was immediately seized with the *perce-pierre*, and as soon broken, by merely pressing it with the drill against the claws. A considerable portion of detritus was evacuated as soon as the instrument was withdrawn, and two days after, the remainder was brought away by means of the *sonde évacuatrice* (evacuating catheter). The whole of the molecules evacuated would have formed a small, smooth, uric-acid calculus, not much more voluminous than the largest of those formed in the kidneys, which some patients have the good fortune to expel as soon as they fall into the bladder.

Since the introduction of the *evacuating catheter*, the patient has been quite free from painful sensation, and has continued in a perfect state of health.

Reflections on both Cases.

Here we have two additional proofs of patients who, having prudently addressed themselves to skilful surgeons as soon as they observed the least derangement in the performance of their urinary functions, were ably advised and enlightened as to their state, and had the good sense to have immediate recourse to the proper means of obtaining relief from a disease which (although in its earliest stage) was a source of affliction and torment to them. It is because these patients applied thus early, that they were so speedily relieved, and with so little suffering; one of them, General M., did not know that the operation of breaking the stone had been performed, until he saw a considerable portion of it come out comminuted. I had not told him beforehand that the operation was to be performed, and he came to my house, thinking that it was merely my intention to sound him; he was as much surprised as delighted therefore when, on getting off the "*lit rectangle*," he learnt that his stone was completely pulverised. When such a result is compared with the consequences of lithotomy, no doubt will, I think, remain as to the importance and vast utility of *lithotripsy*.

I would also particularly call attention to these two cases, since both were successfully treated with the *perce-pierre*, an instrument which I have been accused of condemning and rejecting. Once more I repeat that I do not consider this as a bad or inapplicable instrument, but merely as an insufficient one. These two cases, as well as many others published previously, such as Mr. Elliot's and Mr. Biggs' of Stratford, Mr. Browne's of Deal, Mr. Messenger's of Petersham, &c., in all of which the *perce-pierre* was employed, prove that I do consider it advantageously applicable in cases of small calculi, and when there are no other obstacles.

Finally, I would notice the importance—nay, the absolute necessity of possessing suitable means, of extracting with ease and safety fragments when lodged in the urethra, as was required in General M.'s case. It is frequently more difficult to extract pieces of stone entangled in the urethra, or to comminute them in that passage, than to manipulate in the interior of the bladder. As soon as circumstances will allow, it is my intention to publish the instruments which I possess for effecting this important part of *lithotripsy*; it is besides a necessary complement to the memoir which I published in a preceding Number of THE LANCET on *Lithocenosis*, or the *Evacuation of Fragments.*

CHARACTER AND TREATMENT
OF THE
MALIGNANT CHOLERA
AT
LIVERPOOL.

TRIAL OF VENOUS INJECTIONS.

To the Editor of THE LANCET.

SIR,—Should the following remarks, carefully made, be deemed of sufficient value to obtain a place in the forthcoming number of your scientific Journal, you will much oblige and serve your correspondent by their insertion.

T. J. MURPHY, M.D.,
Licentiate of the Royal College of Surgeons in Ireland.
36, Mill Street, Liverpool.
June 15th.

From the time that cholera first made its appearance in Liverpool, I have been an attentive observer of its symptoms, and have carefully watched the result of the therapeutic remedies in my own practice, as well as that of others. The first and second cases occurred in the practice of Mr. Kelly, who kindly allowed me every opportunity of watching the disease, and of investigating the value of the modes of treatment employed by him. The third fell to my own lot. I shall as briefly as possible state my opinion on the nature of the disease, its course, and the results of my own practice. Its cause in my judgment is attributable to some atmospheric changes, which I do not pretend to understand. The following are my reasons for adopting this opinion: the difficulty of establishing any case as produced by contagion or infection, the few who have suffered by attendance on those afflicted by the disease, and the many affections of the mucous membrane which have latterly come under my observation, amongst the diseases of which membrane, cholera may be classed, viz. several infants suffering from ophthalmia neonatorum, whose mothers were free from any vaginal disease; the high degree of purulent ophthalmia attending several cases of measles, which completely misled me at the commencement; some cases of epistaxis, and numerous cases of diarrhœa; cynanche tonsillaris was also prevalent. "The cholera" I have looked upon and treated as a severe form of gastro-enterite, which may be usefully divided into three stages, each requiring a very different mode of treatment. The first stage is that of vomiting, diarrhœa, and spasms; the danger increases in proportion to the watery discharges. The symptoms arise in the following order: diarrhœa, vomiting, spasms of the legs, next the arms, and then the diaphragm suffers. Suppression of urine is caused merely by the watery parts of the blood passing off by the bowels, and its secretion is favourable.

Suppression of urine is one of the last symptoms of the first stage. The order in which the symptoms are first evinced is reversed in their disappearance, and hence the secretion of urine does not take place until the other symptoms subside. My treatment in this stage consists of opium and its preparations freely exhibited; sinapisms from the ensiform cartilage to the ossa pubis, and cold water in spoonsful when the thirst is extreme, which I have almost always found successful. I have tried bloodletting in this stage, but am not an advocate for its use, as I consider it has little influence over mucous inflammations, as may be witnessed in conjunctivitis, bronchitis, dysentery, gonorrhœa. One bleeding may be found useful in bronchitis, a second will generally be injurious. The warm-bath is useful sometimes, but should diaphoresis follow, which is the passing off of the watery parts of the blood, the danger is heightened.

Margaret Connolly, ætat. 30, pregnant four months, residing in 24, Jordan Street. I saw her six hours after being attacked, and in the twelve following hours gave nine grains of opium and five drachms of laudanum with little benefit. Being now alarmed, I determined on *injecting acetate of morphia into the veins*, as the stomach and bowels were so irritable. Before trying this experiment, I took half a grain of acetate of morphia, dissolved in one ounce of water, and with a small syringe threw it into the rectum, retaining it by a bandage and compress. The spasms then ceased, no discharge from the bowels took place for six hours, and she is now convalescent. I used no means of allaying the vomiting, as I consider it useful in keeping up the circulation.

The second stage is that of collapse. The danger is proportioned to the loss of serum. The extremities, tongue, and breath, are cold; eyes sunk; respiration slow, even to fourteen in the minute; pulsation of radial and even of femoral and axillary ceases. The action of heart feeble; chiefly lies on side; spasm, vomiting, and diarrhœa, cease. Few recover from this state. Having failed myself, and seen others fail, in rousing them from this state, notwithstanding that the judicious measures hitherto adopted were acted upon, I determined on the use of some more efficacious plan. My first idea was to return into the system the alvine dejections, by injecting them into the venous system; an idea which arose from a quantity having been preserved for my inspection. It was clear, colourless, free from odour, and in every respect resembled the serum of the blood.

But as dejections are seldom preserved, it occurred to me to make an artificial serum, the component parts of which appeared so simple, from the analysis of Marcet, Bostock, and O'Shaughnessy. To introduce it by the stomach, I found not only useless, but injurious, and its introduction by the rectum increased the irritation. Besides, I failed, although I confined the alvine dejection with a compress. The only means left for me was to throw it in by the veins.

I communicated this plan to Dr. Baird in the presence of Mr. Kelly, surgeon, *so far back as the 15th of May*, and of course previous to its publication in any journal. He immediately approved of the plan, and has since tried it. A few days subsequent to this communication, Mr. Kelly afforded me an opportunity of trying the plan. In this case and another, the experiment failed, but so decided was the improvement observed, that I determined on not relinquishing it. Since then, both Mr. Kelly and I have been successful; one case in Oxford Place, Oxford Street; the other, Sarah Pilkington, Sparling Street. These two cases out of four have recovered. My surprise was considerable at perceiving some time after, by an article in THE LANCET, that Dr. Latta had been pursuing the same mode of treatment, and the extraordinary accuracy with which he has detailed the effect, precludes the necessity of any remarks at present from me. The testimony of the above gentlemen can clearly prove, that the idea had occurred to me prior to its publication, although, singular as it may appear, Dr. Latta and I were trying the same experiment at the same time, without any communication. Yet I was unwilling to avail myself of your pages, until I could more safely recommend the plan to the profession.

The third stage I treat as the gastro-enterite of Broussais, a sinapism to the epigastric region, mucilaginous drinks, hyd. cum creta, and, if necessary, leeches.

CASES OF

MALIGNANT CHOLERA

TREATED BY THE

INJECTION OF SALINE FLUIDS INTO THE VEINS.

No. 1.

Letter from DR. ANDERSON, *of Rochester, detailing Five Cases, of which Three were successfully treated.*

Rochester, June 17th, 1832.

SIR,—I beg to aknowledge your letter of the 14th inst., since which time I have had considerable experience in the employment of saline injections by the veins. The first two cases selected for trial had been in a state of the most perfect collapse for about 15 hours, and both passing their stools involuntarily, which constant *drain* from the bowels I have ever found a fatal symptom under every mode of treatment yet employed.

In the first case, upon injecting four pints into the veins at the bend of the arm, the pulse became perceptible with a general sensation of heat over the body; the injection was continued, and two additional pints thrown up very gradually, when the pulse still continued to rise, and the countenance to assume its natural hue—it was before quite blue.

Upon such unexpected results another case in a worse state was immediately selected, and the same process adopted, and with the same results.

Severe rigors took place in both these cases, a few minutes after throwing up the fluid, but still the pulse and heat continued for about three hours after the injection, after which collapse again took place in both about the same time. The injection was again had recourse to, and with the same decided benefit. This plan was followed up in both cases for about 12 hours, when I was obliged to leave, requesting, during my absence, that similar treatment might be persisted in through the night; but I was sorry to find upon my return to the ship next morning that one of my patients had died, and that the other was just dying. In the first case 305℥ were thrown up, and in the other about 190℥. During all this time an immense serous discharge continued to run from the bowels.

Three additional cases were selected yesterday, and this plan of treatment adopted, and I am happy to add with the most decided benefit; they are all at this moment convalescent.

In neither of these had the *insensible* serous discharge taken place from the bowels, although the collapse was equally perfect. Nor was it necessary to inject more than from 4 to 6 pounds in each. In one of the cases sleep took place during the process. Upon the whole I am inclined to place more dependence upon this remedy than on any other mode of treatment yet recommended; and I have no hesitation in saying, that if you see your patient soon after the collapse takes place, and before the *insensible draining* from the bowels, you have an almost certain remedy within your power. This plan of treatment I intend to follow up for the present, as altogether the most favourable I have yet observed. I have the honour to be,

&c. &c., John Anderson, M.D.,
Surgeon, R.N.

To the Secretary of the Central Board
of Health.

No. 2.

Letter from Dr. Lewins *of Leith.*

Leith, June 14.

Sir,—Circumstances have prevented me from transmitting an account of a fatal case of cholera referred to in my letter to you of the 27th ult., sooner. Death took place in the case alluded to, ten hours after the circulation had been restored by means of copious saline injections into the veins: the patient previously suffering severely from symptoms characteristic of fatal abdominal derangement, and these not attributable, I believe, to the means employed to rescue him, and which succeeded from death in another shape. In the space of two or three hours the venous injection has produced the astonishing effects already repeatedly mentioned, the patient appeared to do well; after which, however, he began to complain of distention and uneasiness in the bowels. This was followed by great restlessness and distress, until death closed the agonizing scene.

The symptoms in this case were exactly similar to those which I have observed when complete loss of functional power in the intestinal canal had taken place, as a cause or effect of obstruction, or from mortification or perforation of the intestines. A difference of opinion existed in the minds of my able and respectable colleagues of the Leith Cholera Hospital, as to the immediate cause of death in this case, before an examination of the body took place; and my talented friend Dr. Mackintosh, of Edinburgh, whose indefatigable zeal in the cause of medical science is scarcely equalled, certainly not surpassed by any man alive, thought it probable that we should find a ruptured blood-vessel in the abdominal cavity. But upon dissection all the viscera were found sound, the intestines were enormously distended; the bladder perfectly empty and firmly contracted; there was no effusion of any kind into the cavity of the abdomen. The veins were, perhaps, more turgid than natural, but not so as to attract particular attention; upwards of seven pounds of fluid, of the kind usually evacuated in cholera, were found in the bowels. The distention of the abdomen, the supervention of distressing symptoms, and ultimately the death of the patient, was, I have no doubt, the effect of total want of functional power in the intestinal canal, a condition which I have sometimes observed to take place, although I do not remember to have seen it particularly noticed by medical writers during the progress of other diseases. My conclusion in regard to the case of cholera now under consideration is, that paralysis of the intestinal canal occasioned or increased by the copious venous injections, was the cause of death, an opinion to which I shall have occasion to revert in explaining other conclusions I am inclined to draw from our late experience of this most extraordinary disease, and which I shall speak of fully in a future communication. In the mean time I may, in my opinion, remark that the failures we have witnessed afford no real objection to the practice of treating cholera by venous injections: a method of medical treatment which, as I said before, will, I predict, lead to important changes and improvements in the practice of medicine; and will entitle Dr. Latta's name to be placed amongst the number of those, alas! how few, who have really contributed to the improvement of the healing art.

In a late number of a medical journal a writer, in speaking of the Leith and Edinburgh cholera cases that have been treated by venous injection, seems to imagine that the patients who died, did so in consequence of phlebitis; that is not the fact, there has not been a single case of death from that cause. I have the honour to be, Sir,

Yours, &c.,
Robert Lewins.

To the Secretary of the Central Board
of Health.

No. 3.

Letter from Dr. Latta, *of Leith, detailing Three Cases, of which one was successful.*

Leith, June 16th.

Sir,—Since my last communication, I have subjected to the influence of venous injection a few more cases of cholera, which occurred during my week at hospital superintendence. A few circumstances in the treatment suggested themselves, which you may deem not entirely void of interest with such an impression I have been induced to send you the following brief statements of cases with remarks.

Case 1.—A prostitute of the very lowest order was brought to the hospital on the 23th of May at past-half one, affected with cholera, the case was one of more than ordinary severity. I not being present, she was subjected to the ordinary saline treatment, recommended by Dr. Stevens, till four o'clock. As soon as I arrived, I found her sinking rapidly, tormented with the most fearful spasms, drowsiness, retching severe, pulse gone; eyes sunk, and other symptoms of confirmed collapse. Her body exhaled a most disagreable odour, which increased with her distemper; so much so, that the windows, constantly open, scarcely admitted air enough to render the apartments where she lay tolerable to the attendants. The saline solution was instantly thrown into the veins, which was followed by the most agreeable consequences, having entirely removed the unsupportable load from her chest, which was a source of much anguish. After eight pounds were injected, she experienced entire relief; diarrhœa had all along been very profuse, but now it became excessive, running through the bed all over the floor. In consequence of which the rallied powers again gave way, requiring to be stayed by a repetition of the injection. So rapidly had she sunk, that it became necessary to inject with more than usual speed, which for reasons afterwards to be noticed, should, if possible, be avoided. About twenty pounds were injected on three different occasions within forty hours, by which, time the symptoms of cholera were removed, the diarrhœa considerably lessened; the stools were now tinged with bile, and the urinary secretion was restored.

For two days she seemed to do tolerably well. Previous to the attack of cholera, she had been under treatment for diseased liver, and was also subject to pectoral complaints; these symptoms were now aggravated, for which she was bled; leeches and blisters, with but little effect; her stools now became of a dark olive colour, and, like the increasing fœtor of her person, were very offensive; the whole surface assumed a leaden hue; gradually she sunk, and expired without a struggle.

Case 2.—On the 27th of May, at seven p. m., a middle-aged female was brought to the hospital in articulo mortis. She had been seized with cholera early in the morning, the diarrhœa and vomiting, which had been very profuse, had so thickened the vital fluid by draining off the serum, that by noon the exhaling vessels gorged with their thickened contents ceased to furnish that fluid which constitutes the dejections so characteristic of the disease. When she was lifted from the bed in which she had been carried to the hospital, her head dropped on her chest, her arms hung dangling and lifeless from her trunk, the whole surface was blue, her entire aspect was that of one whose spirit had just fled. Unconscious of existence, she was laid on the mattress. I instantly opened a vein in the right arm, and threw in 132 ounces of saline fluid, keeping the temperature above 105; first it was rapidly introduced, but as soon as symptoms of resuscitation appeared, I proceeded now slowly on, and including some considerable delays, accomplished the whole in about two hours.

Having finished, I felt much gratified with the result; the poor woman was in a short space restored to the entire use of all her senses. But though the result was much more fortunate than I had reason to expect, I entertained not the most sanguine hopes for her ultimate recovery; for though her other symptoms were good, the radial pulsation continued very feeble, diarrhœa returned, notwithstanding the free application of stimulants internally and externally. Again she became pulseless, and by midnight she was as low as ever; eighty ounces of saline injection again restored her, and by two in the morning (May 28) she was greatly improved; respiration not in the least laborious, though quicker than natural; pulse 120, small, but distinct; countenance natural; lips red; tongue moist and warm; temperature of surface restored; skin moist, &c. &c. Towards morning diarrhœa returned; she continued slowly to decline in spite of every remedy; the surface became cold and clammy, the pulse ceased, and she complained of extreme debility, so I was compelled again to have recourse to the only thing that produced any improvement, and at four a. m. four pounds nine ounces were again injected; the good effect of this had fled by eight a. m., and again six ounces were injected, which drew from her the most grateful acknowledgments of relief; mercurials, tonics, stimulants, with effervescing draughts, were administered throughout the day, and before night she had passed five bilious stools, and micturated pretty freely two or three times. Symptoms of cholera were now gone, she passed the night easy, and next morning, May 29, the report is—has no complaint except debility and considerable thirst; pulse 104; tongue dry and red; breakfasted with relish; bowels free; dejections very dark; passes urine freely; during the forenoon seemed absent; in the afternoon she became incoherent; during night she gradually sunk, and died next morning without any symptom of collapse, or of any local disease.

Case 3.—A middle-aged man, of sober industrious habits, in whose family several cases of cholera had occurred, was, on the 27th of May, at 9 p.m., seized with the first symptoms of that disease, and was subjected to the ordinary treatment for seven hours previous to his coming under my care. On the morning of the 28th, at half-past five, his countenance was pallid, his eyes sunk, his voice choleric in the highest degree; pulse 118, fluttering; every feature sharp; extremities cold; had had saline injections per anum, laxatives, opiates, stimulants, hot applications, sinapisms, &c. &c., and notwithstanding he sunk rapidly, particularly during the hour previous to injecting the veins; an operation which was unavoidably delayed, I being engaged with another patient. In half an hour eight pounds were injected with entire relief to every unpleasant feeling, if we except the irritation arising from the sinapism. Was ordered to take hourly small doses of calomel and opium, and beef-tea, enemata, with muriate of soda. Diarrhœa continued copious and frequent, and by 2 p.m. he seemed sinking fast; had almost lost his sight; recommenced the venous injection; soon his sight began to improve, and before a pound was injected it was quite restored; about six pounds expelled all his uneasiness, and from that moment he continued to improve, using only beef-tea, mercurials, and effervescing draughts. Early next morning the stools became bilious, the secretion of urine was restored, and he was convalescent on the third day.

Here are three cases having different features; the first proving fatal from organic disease, the second from the effects of the most inveterate diarrhœa, and the third recovered by the prompt and easy application of venous injection.

With regard to the cause of death in the first case I have nothing to say; it being now generally admitted, that the individual labouring under chronic disease, like the drunkard and the debauchee, is very liable to be assailed by cholera, and that, in consequence of such an attack, the dormant disease develops itself more actively, and very often proves fatal, or issues in confirmed bad health. With regard to the second cause of death—viz. the diarrhœa and its consequences, this is a subject which appears of most interesting importance.

The watery diarrhœa is evidently derived from the extremities of the minute vessels ramifying in the stomach and intestinal tube, these are somehow forcibly dilated, affording exit to the serum and crassamentum of the blood; nay, in the advanced stage of aggravated cases even of the red particles, which last symptom accordingly always affords an unfavourable prognosis. Now, whether this effusion is only symptomatic of hepatic obstruction, or is an idiopathic disease produced by the invisible meteorological cause, may be subject of dispute. At all events, if we consider that those individuals who seem most liable to the attacks of this terrible disease, are those whose diet and habits of life are very apt to derange the functions of the liver—that pain is often very acute in the hepatic region—that the secretion of bile is among the first that ceases—that its restoration is hailed as a happy omen—that it continues morbid, and a source of great uneasiness long after the stage of collapse is past—that hepatitis very frequently supervenes during reaction—that a diseased state of the organ is very generally met with after death—that medicines, such as mercurials, which act on this viscus, are of much utility—that it is always gorged with blood—also consider the nature of the hepatic circulation, which renders it very susceptible of obstruction, more especially if the blood is reduced to a viscid state by the abstraction of a portion of its serum, and the nervous energy subdued by the influence of any sedative agent; and besides, the effusion of serum in cholera is confined to those viscera which return their blood through the liver, in which viscera venous congestion is always strongly marked. All these circumstances certainly render it probable that hepatic obstruction does exist, preventing the return of the blood from the bowels, producing in these the usual consequence of obstructed circulation—the effusion of serum. In other parts of the body the blood returns to the heart, flowing from branches to trunks, and when it reaches the centre of circulation, if thickened from the draining effects of diarrhœa, it collects them, producing the insufferable load so symptomatic of cholera; but so soon as saline fluid is introduced along the veins, and directly mixed with it, then free circulation is restored, the anguish is removed; the blood, thus restored to a state fit for circulation, makes its circuit, and congestion is removed from every viscus except the liver, where the circulation being widely different, the thick grumous blood gorges its venous ramifications. The obstruction thus produced must continue, until by slow degrees it is penetrated by the more fluid circulating mass; until this is accomplished diarrhœa must continue. The symptoms of its restoration are, a cessation of the serous diarrhœa, and a return of the biliary secretion.

But in the second case, noticed above, such obstruction seemed entirely removed, and the secretion eventually restored, and yet the patient died. Even so it happened; but then we must take into account the circumstance that the diarrhœa being profuse and often renewed, had carried away a large proportion of the more substantial principles of the blood, which, though not immediately necessary to existence, yet the deficiency is soon felt, and if not supplied, the vital fluid may speedily fall short of its intentions. But not only was such a pernicious change effected on the blood, but the fearful extremity to which the poor woman was reduced, must have produced an almost irrecoverable shock to the nervous system, and from the abstraction of energy, favoured the deposition of fibrin which was found in the heart, and which was doubtless the cause of the very feeble pulse which induced me to form an unfavourable prognosis of the case.

If the view I have taken of the cause of the diarrhœa is correct, it points out the need there is for the early use of the saline injection, the benign effects of which are clearly demonstrated in the third case. It is not improbable but the quantity of salt formerly recommended may be too large, particularly if we require to repeat the injection. Circumstances, which I may notice on some future occasion, have induced me to think so, and accordingly, in each secondary injection, I diminish the quantity of saline ingredients.

I may mention, though I have no permission for doing so, that solution of quinine and of morphia, pure water, and even blood, have been all used by some medical gentlemen, but not with such encouragement as induced them to persevere. Albumen has also been used in the saline mixture but given up, though I would certainly be disposed to advise its use in extreme cases, when the diarrhœa has continued profuse, and has returned after each injection.

I regret my letter has turned out so voluminous; I trust however I may be excused for such indulgence. I have the honour to be, Sir, your most obedient servant,

T. Latta, M.D.

To the Secretary of the Central
Board of Health.

CASE OF

UTERINE HEMORRHAGE,

WITH ATTACHMENT OF THE PLACENTA TO THE CERVIX UTERI,

In which the Plug was employed with very marked Advantage.

By John T. Ingleby, *Esq., Surgeon, Birmingham.*

April 26, 1832. At the request of Mr. Heeley, surgeon of this place, I visited Mrs. ——, a very delicate woman, whom he was then attending in labour, and who had reached the full term of utero-gestation. It appeared that the patient was seized three weeks previously with a copious hemorrhage, which subsided under medical treatment, and did not return until nearly one o'clock this morning, when Mr. Heeley was again called in. The labour pains were feeble, and the discharge very considerable. This, however, subsided at half-past two; at four o'clock the pains had ceased altogether, but the discharge had materially increased, and as the patient had begun to vomit, a small dose of laudanum was administered. I was called to see her at seven: the bed and bedding were soaked with blood of thin consistence, and possessing little coagulation, but the active hemorrhage had again materially diminished. The os uteri was quite lax, dilated about the size of a half-crown, and several small coagula had collected over it. On passing my finger about an inch and a half beyond the uterine orifice I detected an edge of the placenta, quite detached. Nearly the whole of the child's head was above the brim of the pelvis. The patient was faint and cold, the countenance exsanguineous, the respiration very quick, the pulse undulating, and so feeble and frequent as scarcely to be numbered or felt with distinctness, though Mr. Heeley on one occasion counted 150 beats in the minute. Under an impression that immediate delivery in this state of exhaustion would be fatal,* and fearing to allow even a draining to go on, I resolved, with Mr. Heeley's concurrence, to use the plug. Having steeped a large and soft silk handkerchief in olive oil, I conveyed as much of it through the os uteri, between the fœtal head (which was rather loosely situated) and the detached portion of placenta, as seemed to be necessary, and 50 drops of laudanum, with some hot brandy and water, were now administered. In about an hour the pulse had improved, and as the plug had begun to excite uterine contractions, I thought it prudent to withdraw it, lest an additional mass of placenta might be separated by the action thus induced. I found the os internum rapidly dilating, the head had almost passed the brim, and the membranes were partially on the stretch: I immediately lacerated them, and a large quantity of water was at once discharged. It was unnecessary to apply the short forceps, for the hemorrhage had now entirely ceased, and the pains were so efficient, that in less than half an hour from this time the child was born living, but so feeble, that the usual resuscitative means were unavailing. The placenta was soon expelled. The portion which seems to have been detached on the occasion of the first hemorrhage, was marked on its fœtal surface by a defined circular depression. This part was of an oblong form, two inches wide, and about an inch and a half deep; it presented a disorganised appearance, being yellow, in patches, like curd or lymph, brown in other places, and having a few scarlet and elevated spots interspersed. A fresh placental detachment, covered on its maternal surface by a thin layer of coagulated blood, gave rise, no doubt, to the present hemorrhage. The patient is suffering from the effects of loss of blood, the disease described by Dr. Marshall Hall.

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* Although the head was entering the brim, yet from the great relaxation of parts I could have passed my hand in utero very readily. Possibly the long forceps might have been used with effect, on account of the *dilatable* state of the os internum. But any agitation of body might have proved fatal at this juncture.

Reflections.—In the consideration of this case, two points seem to suggest themselves, the one refers to the hemorrhages being three weeks distant from each other, and the serious effect which the second flooding produced upon the system; the other has reference to the value of the plug when used under a state of collapse.

SECOND COMMUNICATION FROM

DR. VENABLES

ON

VACCINATION.

To the Editor of THE LANCET.

SIR,—In reference to the observations upon vaccination and small-pox, published in the last Number of THE LANCET, I feel it due to candour and truth to state the subsequent facts. When I sent the paper for publication, I had every reason to believe that the little boy imperfectly vaccinated, and whom I stated to have subsequently suffered from modified small-pox, and therefore unsusceptible of any future attack, had escaped upon the present occasion. This, however, has proved by no means the case, and with some, perhaps, the subsequent history may seem to militate against the doctrine in my former paper. Notwithstanding I feel bound to lay the facts before the profession, that each may exercise his own judgment in drawing a conclusion.

Upon Monday week last, the 4th of June, the little boy, whom I looked upon as having escaped, complained of being ill, and after rising in the morning, felt unequal to sitting up, and therefore retired to bed again. He felt hot, feverish, thirsty, and the eyes became suffused and watery. He continued in this state till Wednesday noon, but at this time he felt so much better that he got up, but retired to bed as usual in the evening. On Thursday morning he declared himself perfectly well, and went to school. In the morning, however, previously to his going to school, I observed a few papulous-looking elevations upon the face, perfectly distinct at this time, with here and there an efflorescence. These had increased considerably towards evening, and by Friday morning he was manifestly suffering from what, under other circumstances, would be instantly recognised as the incipient eruptive efflorescence of small-pox. By the afternoon, the efflorescence was thickly crowded with vesicular-looking summits, such as I have described in my former paper, and in the evening many of them on the face had run together or coalesced, as in the confluent form of small-pox. Several had appeared upon the chest, neck, and arms, but remained perfectly distinct, not the smallest attempt at coalescence presenting. The eruption upon the face of this child was far more strongly marked with the variolous character than in any of the other children. Saturday morning the tumefaction of the face and the eruption were subsiding. By the evening the eruption had almost perfectly subsided, leaving the face, however, swollen and red. By Sunday, all traces of anything pustular had disappeared; the redness and swelling of the face, however, continued, and the features, and indeed general expression of the countenance, were remarkably altered, insomuch as to call forth the observation of many persons; and it is only now that the countenance has assumed anything like its former expression. He felt perfectly well by Monday morning, and went to school. Such is a detail of the facts which I felt myself called upon to submit.

Some will now perhaps be inclined to question the variolous character in any of the above cases, and this possibly because they regard small-pox so complete an antidote to itself, that they cannot admit the possibility of a second attack. To this objection, however, it may be answered, that there are most decided and unequivocal instances of a second, and even a third attack of small-pox in the same individual. Therefore if the disease in its most clear and characteristically developed forms, can present a second and even a third time in the same individual, there is no great difficulty in conceiving such modified attacks as those I have detailed. I have however mentioned the circumstances to persons of experience and judgment, and who perfectly coincide in the views which I have taken. The facts however I have faithfully detailed, and each is at liberty to form his own judgment.

As the present is perhaps the last occasion upon which I shall feel it necessary to address any observations upon vaccination, I feel it right to avail myself of the opportunity to make a few remarks upon the mode of vaccination. I have already stated that cow-pox is a very delicate disease, and therefore very easily disturbed in its progress. On this account the patient should be vaccinated in two or three separate and distinct places, and at the same time at such distances from each other as that neither the vesicles nor their areolæ may touch or interfere with each other. One consequence may be such an interruption of the progress of the coalescing vesicles, as the engendering a spurious form which affords no protection. If the distance be such, as that the areolæ only can coalesce, the consequences are still worse, for so much inflammation is excited at the margins, and which spreading towards the centres, not only prevents the progress of the vaccine vesicles, but they most commonly exchange their lymphy for a purulent character, and not unfrequently terminate in ulceration and sloughing. I need hardly observe that such results completely mar the advantages of vaccination. One vesicle at least should always be permitted to go through its course without any interruption. There are many other circumstances to which I might advert, but as the above are the most common errors, and the most frequent causes of failure, I have thought it necessary to notice them more particularly.

Robert Venables.
Hackney Road, June 18, 1832.

P.S. To prove how readily a little activity and exertion will tend to limit effectually the extension of contagious disease, I may observe that by imposing a strict quarantine as it were, an infant only three months old has been completely protected from the contagion of the small-pox, or whatever else it may be called, as there can be little doubt as to its contagious character. How else can we reconcile the disorder appearing in every one of the children among whom intercourse was not interdicted? but it has not attacked the infant between whom and the other children all communication was immediately suspended. This infant was successfully vaccinated on Monday last.

THE LANCET.

London, Saturday, June 23d, 1832.

The readers of this journal stand in no need of being reminded of the frequency and urgency with which we have called for the establishment of a Hospital in connexion with the University of London. The students, the public, the demands of medical science, the character of the new institution, all imperatively called for the erection of such an establishment. Without it, the medical school of the University could only exist in part. Whatever the qualifications of the professors, whatever their exertions, the instruction must be incomplete, and, in a practical point of view, almost worthless, without this vastly important and indispensably necessary adjunct. Rejoiced are we, then, to find that the Council have at last set about the formation of such an institution in right earnest, and under the sanction of the present ministry—many of whom are shareholders in the University—it can scarcely fail to be both prompt in operation, and permanent in success. A call upon the public is made by the Council for subscriptions to the building-fund. To that demand upon the charitable feeling and sound judgment of the county, we shall offer the humble aid of our pages, provided —and only with this proviso—that there be first issued by the Council certain conditions, obligatory upon trustees, as to the mode in which the money to be subscribed shall be expended. We refer, of course, to the extent of medical accommodation to be afforded to the sick, the rules to be observed in the election of the medical officers, and the facilities to be afforded for the instruction of medical students.

These important points once agreed on between the Council and the public, THE LANCET shall not be idle in furthering the objects of those benevolent and intelligent gentlemen, who, after three or four years of disagreeable experience, are forced to acknowledge that the medical department of the LONDON UNIVERSITY can never be fixed upon a secure or useful basis whilst devoid of the support to be derived from a capacious and well-regulated *hospital*. Concerning the condition of the bond to be entered into between the Council and the subscribers, we shall be more explicit in an early number of this journal.

Our object in noticing the subject on this occasion, is two-fold. First, with a view to direct attention to the published address of the Council; and, secondly, to place before our readers a copy of a letter, addressed by Dr. HARRISON to the Council immediately after the Students' Petition was presented to that body in October, 1829. When we recollect the time at which this offer of one hundred guineas was made, this great act of scientific liberality deserves not less credit for the judgment by which it was dictated, than it does admiration for the feeling with which it was proffered. The offer of such a sum at such a period, when the lukewarmness of pretended friends was so apparent, and the misgivings of the actual supporters of the institution so frequently expressed,—this offer, we say, under such circumstances, and at a moment when only the echo of the first proposal for such an institution had reached the ears of Dr. HARRISON, may be classed amongst the most generous benefactions to be found in the modern records of charitable institutions.

"7, Holles-street, Cavendish-square,
Nov. 7th, 1829.

"SIR,—I read in THE LANCET of this day some account of an address to the Council of the London University, from the medical students attending the lectures of that institution, in which they petition to enjoy the great advantages of having an hospital, in connexion with their school.

"Although my name has not hitherto appeared among the subscribers, I have always taken a lively interest in the welfare of the University, from a firm conviction in my own mind, that a great national institution, founded upon liberal principles, has been long wanted in the British metropolis. More than twenty years since, I ventured to recommend an establishment of a complete medical school by Government, in an address to the Lincolnshire Benevolent Medical Society, and which was published at the request of that body.

"Soon afterwards, I had a communication with Mr. Pitt on the state of the medical profession in this country, through Sir Walter Farquhar, who was at the time attending this celebrated statesman. I had also interviews with the Marquis of Lansdowne, then Lord Henry Petty, Chancellor of the Exchequer, and with Mr. Perceval, a short time previous to his death on the same subject. They all admitted the necessity of new regulations, and I am persuaded this desirable object would then have been attained had not the three medical corporations opposed the measure.

"Feeling convinced that a large hospital is essential to the prosperity of the medical department of your University, for however ably complaints be delineated in lectures, the art of distinguishing them can only be acquired at the bed-side of the patient, I therefore write to desire the favour of you to state to the Council, that whenever they are prepared to commence one, upon a scale adequate to the extent and wants of medical science, I will, with pleasure, contribute one hundred guineas in furtherance of the undertaking.

"I have the honour to be, Sir,
"Your very obedient, humble servant,
(Signed) "EDW. HARRISON,
F.R.S.A., EDIN."

"To Thomas Coates, Esq.,
"University of London."

THE following is the address alluded to issued from the UNIVERSITY OF LONDON:—

"NORTH LONDON HOSPITAL.

"The want of an hospital for the reception of patients suffering from accidents and acute diseases, has been long felt in the northern districts of the metropolis.

"The great population of Islington, St. Pancras, Somers' Town, Paddington, and the contiguous districts, which now amounts to 250,000 persons, and which is continuing rapidly to increase, makes the urgent necessity for some corresponding provision for the relief of the sick poor of these districts at once apparent.

"The Council of the London University, with a view to supply this want, and at the same time to add to the efficiency of their medical school, have, with the cordial sanction of the proprietors, determined upon the erection of an hospital on the vacant piece of ground opposite to the University, and a plan for the building has been already approved of.

"The funds of the University at the disposal of the Council, are not, however, such as to enable them to carry their purpose into effect without other assistance, and they are induced, therefore, to appeal to the liberality of the public.

"The success which has attended the dispensary established by them four years ago, and from which thousands of the poor in the surrounding districts have received efficient medical assistance, encourages the Council to entertain a confident hope, not only that this appeal to the benevolence of the public will be promptly and liberally answered, but that the final accomplishment of their object will be attended with incalculable advantages to the cause, both of humanity and of science.

"Subscriptions will be received at Messrs. Coutts and Co's., Strand, and at Messrs. Smith, Payne, and Smith's, 1, Mansion-House Place.

"Donors of thirty guineas in one payment, or annual subscribers of three guineas, will have the right of recommending in and out patients.

"Donors of ten guineas in one payment, or annual subscribers of one guinea, will have the right of recommending out-patients only.

"Thomas Coates, Secretary."

If the causes of the epidemic cholera have given rise to multifarious speculations, and if its pathological characters have perplexed all the reformers of nomenclature, the treatment proposed for the cure of this terrible malady has not been less the theme of controversy, and a profitable source of trade to impudent quacks, and other unprincipled pretenders to a knowledge in the science of medicine.

Did we credit a tithe of the assertions ostentatiously put forward, with all the impress of time and place, of the asserted success of a thousand alleged remedies, our wonder would be that the cholera has not long since been spirited off to the Red Sea, or some such hospitable asylum for banished humbug. Not a journal, medical, religious, political, or literary, is printed for circulation without containing a cure, a "certain cure" for the cholera, with statistical tables of the results, specifying when and where, names, dates, &c., &c., in a rank and file accuracy well calculated to carry home irresistible conviction to the uninformed mind. As this point is of some importance to be proved, we may appeal for our evidence to the history of the disease, from its irruption in Jessore to its present state in London. We find that in Bengal the moxa worked such wonders that scarcely a patient died on whom it was fairly tried. The drogue ancere was equally fortunate in Bombay. Acupuncturation saved every one in Japan. A combination of calomel and opium was a specific in Calcutta. In Persia, cold water did all that was required. In Cronstadt the actual cautery again became ascendant. At Constantinople, out of 200 patients, Dr. M'Guffog cured 196 by mere venesection. We know not in how many places burnt cork, given in milk, at once dispelled the disorder; and our journal would hardly contain the names alone of those districts where "pure brandy" was a never-failing panacea. What inference can we derive from the contemplation of these varied statements? Either the disease is more contemptible than a colic, or else the narrators have exaggerated the circumstances of their pretended successes; for that one and the same malady could be amenable to such a multitude of dissimilar specifics, is contrary to the evidence of general pathology and of common sense.

That the cholera is the harmless bugbear which one branch of our inference would make it, we cannot, unfortunately, admit, while we recollect the scenes witnessed in this metropolis, and while we bear in mind the melancholy statistical facts collected during its progress over Asia and Europe. We are consequently left to the disagreeable alternative of confessing that the truth has not been rigidly adhered to in the reports which have been issued during the progress of the terrible epidemic. Under such circumstances it is manifest, that, for the future, the merits of any novel mode of treatment of cholera will be canvassed by the medical profession in a spirit of the most severe and cautious inquiry. The most scrutinizing scepticism will prevail, and no fact will be admitted until demonstrated by evidence of a perfectly incontrovertible description.

We are led to these observations by a knowledge of the fact, that two or three gentlemen (all of whom, we believe, to be strictly honourable and very meritorious individuals) are now engaged in the prosecution of clinical experiments in London and its vicinity, on the efficacy of *saline remedies* in the treatment of this disease. They have already, we see by the public prints, submitted to the Central Board of Health, details of great and constant successes. The county magistrates have voted two of the gentlemen a handsome pecuniary reward for the reputed triumph of the plan in the Coldbath Fields prison. In short, as far as we have heard of the matter, the results of the treatment are extremely encouraging. Notwithstanding, however, our conviction of the honourable intentions and professional skill of the gentlemen engaged in these experiments, we should still do them a serious wrong, did we hold out to them any expectation that the results of their experience will ever be received by the profession, if their clinical experiments continue to be conducted in the manner at present pursued. Where so much fame is to be acquired, where profit is contingently to be obtained, it cannot be expected by the chief actors that the spirit of envy will slumber. The whisper of detraction will doubtless be heard in many corners; the malicious will insinuate, that Dr. Stevens and Mr. Wakefield exaggerated trifling cases in order to enhance the value of their efforts. The utterly sceptical will openly declare, that the evidence of *two or more remunerated* gentlemen is not to be depended on. In short, we regret to state our conviction, that the future publication of Dr. Stevens's experiments will excite a clamour which will probably defeat his benevolent intentions, unless he abide by the timely counsel of those who wish him every success in his pursuits. The only course left open for Dr. Stevens's adoption, is to memorialise the government to appoint a medical commission to examine minutely into, and report upon, his mode of treatment of this disease. This commission should be composed of men familiar with the disease and its various modes of treatment; above all, of men pledged to no theory, men of indisputable integrity, and well known to the profession. The commission should sedulously watch, and faithfully record, the effects produced by this remedy in an extensive series of cases, and they should unflinchingly report the issue of their inquiries. The government could not venture to refuse to appoint the proposed commission. Slumbering as we are on a volcano of pestilence, ignorant of the hour when the calamities of Paris may be re-enacted in London, that Government would be guilty of abandoning one of the highest interests of humanity, if they turned a deaf ear to the request. It is their imperative duty to investigate the matter, if it can be done without approach to inquisitorial meddling with the privileges of the physician. Indeed, the request being made by Dr. Stevens, it would be a full answer to any such objection.

If, as we take him to be, Dr. Stevens is a truly upright and honourable practitioner, we shall not urge this advice in vain. Should the Government reject his application, we doubt not, that a most trustworthy *volunteer commission* could be readily formed at his desire. Our proposal is not an innovation in medical customs. In France the Institute, or the Academie de Médecine, appoints commissions to investigate the effects of *all* proposed *specifics*, and the disinterested verdict of the reporters justly guides public opinion on the merits of this question. Should Dr. STEVENS follow this example, we can assure him that he will reap a reward of fame directly in proportion to his merits. Let him, on the other hand, spurn the adoption of such a suggestion, and this Journal will at least enter its protest against an unqualified recognition of his pretensions.

We are indebted to the Central Board of Health for copies of some very interesting documents on the practice of venous injection, in the treatment of the malignant cholera. Dr. ANDERSON's report is especially deserving of attention. Its author is a gentleman of many years oriental experience in the disease.

During the preceding fortnight a few cases have been thus treated in London. Of four in the Marylebone Infirmary, two have been recovered. Two unsuccessful experiments have been made in one of the cholera hospitals, and in a lunatic asylum two similar trials have been equally unfortunate. We do not, however, believe that the failure of the operation in these cases weighs against the favourable inference deducible from its success even in a solitary instance. In no case has it been practised until all hope from ordinary measures had long been extinct. In many, the patients were already virtually dead. In some individuals the remedy failed through the pre-existence of organic disease; while in others we entertain no doubt that the imperfection of instruments, and the manual inexperience of the operators, unaccustomed to the delicate process in which they were engaged, contributed in a great measure to the fatal result. Future experience will doubtless suggest many important improvements in the details not involving the great principles of the practice. This all-important fact has at any rate been established, that when performed with ordinary caution *the experiment does no harm of itself*. Can so much be said of lithotomy, or amputation, or the other formidable measures devised for the relief of diseases not more desperate than that for which the saline injection is performed?

A Practical Treatise on Uterine Hemorrhage, in Connexion with Pregnancy and Parturition. By JOHN T. INGLEBY, M.R.C.S., &c. &c. London: Longman and Co. 8vo. pp. 276. 1832.

THE rapidly fatal effects which occasionally attend the hemorrhages of puerperal women, and the difficulties which may be experienced in arresting them, have caused the attention of various obstetrical writers to be directed to this branch of the subject as a particularly important one. Consequently, it always occupies a large space in their productions. But no author had, we believe, devoted an entire volume to the subject of uterine hemorrhage until Mr. Ingleby presented us with the present treatise. The deficiency, however, is now very fully supplied. We have here a work embracing every topic which can justly be looked for under its title, abounding moreover in practical deductions, and rules for conduct, of the highest value.

Having spoken thus favourably of Mr. Ingleby's work, we are compelled, on the other hand, to say, that as a systematic dissertation, it is faulty in one important point, that of arrangement. The student will certainly find therein valuable information on every branch of the subject, but the author has not contrived to lead on the reader in the clear and steady manner which must have resulted from a judicious distribution of parts under a few general heads. Uterine hemorrhages may be divided into three classes. 1st. Those which occur before labour; 2nd. Those occurring during labour; and, 3rd. Those which happen subsequent to labour. These classes may again be subdivided into species, according to the causes whence they originate. In this point of view, class 1 will embrace—hemorrhages arising from certain states of constitution, as plethora, hemorrhagic diathesis, &c., or from special determination of blood to the uterus—from partial separation of the secundines—from tumours, &c., as polypi. The 2nd class, or hemorrhages arising during labour, comprehends floodings produced by partial detachment of the placenta—by the presence of more than one fœtus—by injuries or rupture of the uterus. The 3rd class may be subdivided into three species—viz. hemorrhages arising from complete or partial retention of the placenta—from imperfect or partial contraction of the uterus after the expulsion of the placenta—from an excited and forcible state of the circulation, though the placenta has been expelled, and the uterus fully contracted.

A division of the subject, somewhat similar to that we have proposed, with a systematic dissertation upon each species or subdivision, would have the effect of directing the attention of the student to leading points, and would, moreover, enable him to lay up the knowledge conveyed, in a form most convenient for future use. We do not mean to say that Mr. Ingleby has not treated each and every species of uterine hemorrhage in an able and masterly manner, but that the information in which his work abounds has, to the student, lost some portion of its value, by the desultory character of many of the chapters.

After some observations on the application of bandages, and on certain states of the system during pregnancy which favour the hemorrhagic action, we have an excellent chapter on "the management of labour, with a view of preventing hemorrhage."

This is followed by chapters on vomiting, the action of opium, the circulation in the ovum, &c. These preliminary discussions being disposed of, we have a long and interesting chapter on *abortion*, under which head the author seems to arrange all forms of hemorrhage occurring during the first six months of pregnancy, while floodings of a more advanced period are divided by him into "accidental" and "unavoidable."

The discharges of blood attendant on the earlier periods of pregnancy are, fortunately, much within the control of the judicious practitioner, and, by proper means, the premature expulsion of the fœtus from the uterus may be prevented in a great majority of cases.

The general principles of treatment are familiar to every surgeon. We shall now briefly consider one, the value of which, frequently too much neglected, is fully estimated by our author.

The tampon, or plug, has not been mentioned by any of the earlier writers, nor do we find it alluded to as a valuable auxiliary before the times of Portal and Smellie. Even Rigby and Merriman take little notice of it; experience however proves that we must, with Burns, Dewees, and Madame Boivin, regard it as a powerful mean of rescuing puerperal women from the dangers of hemorrhage.

Mr. Ingleby remarks, that "When flooding threatens to be fatal, the tampon or plug, by stanching the flow of blood, is at once effective and immediate in its action. This grand agent, though noticed by many of the earliest writers, was not applied at all conformably with the principles of science, until advocated by Leroux. It is evident, however, that the efficacy of the tampon is far from being duly estimated, even at the present day, notwithstanding all that Dewees, Burns, and other eminent modern practitioners, have advanced in its favour. The uterus, from the closeness of its texture, and the small size of its cavity, will resist distention in the early months. But in the latter months this is not the case; for, as its substance has then become very ductile and yielding, its cavity capacious, and its length greatly increased, accumulation will more readily take place; and since this is the objection which attaches to the plug, it may be asked, Up to what period of gestation can it be applied without incurring the risk in question? I should say, Certainly up to the beginning, or perhaps the end, of the fourth month. Its advantage, particularly about the third month, is very striking. The vessels will then have acquired a size sufficient to yield a copious effusion, and our other manual resources are at this time very limited. The rupture of the membranes is rarely justifiable before the sixth month. Before the fourth month, this measure is quite out of the question; but even were it otherwise, since all possibility of the continuance of gestation terminates with this operation, which does not necessarily attach to, although it very generally follows, the use of the plug, it is most fortunate that we have so effective an agent within our control. - - - -

"For the purpose of plugging the vagina, a soft sponge soaked in vinegar, solution of alum, or other stypic, is usually selected; but sponge, unless smeared with ointment, or steeped in oil, is not well adapted for the purpose; since, from its porosity, the blood is not completely coagulated, the liquid parts passing through its substance. I give the preference to lint, cotton wool, or a soft handkerchief. The removal of the plug, of whatever substance composed, is facilitated if oiled before it is used. The T bandage may then be applied.

"The plug is peculiarly serviceable in two conditions of the uterus,—first, when the hemorrhage is great, and the os uteri firm and unyielding; and, secondly, when the flooding has so far depressed the system as to leave the uterus incapable of acting. The pressure of the plug excites the organ to contract. Fully to obtain its effect, it is material that the vagina be thoroughly filled with the substance we employ. If the plug only occupies the cavity partially, the blood may continue to escape, as I have frequently noticed, and thus defeat our intention. Several small pieces are preferable to a large single piece; the application is easier, the pressure greater, and the coagulation more readily effected. - - -

"Dr. Dewees authorises the removal of the plug after twelve or fourteen hours. It should not, in my judgment, be removed under twenty-four hours. Anterior to this period, the constitutional powers will not have sufficiently recovered to bear a subsequent hemorrhage, should the withdrawal of the plug be followed by it; but after that time, nature usually will be adequately recruited to bear a recurrence (though the bleeding rarely recurs), until the plug can be replaced."

The question of the propriety of manual interference before the sixth month of utero-gestation, is also carefully considered in this chapter, which concludes with some practical observations on the management of the placenta after abortion. The author expresses his dissent from the proposition of Dr. Robert Lee, that "by far the most frequent cause of abortion is in the product of conception itself, viz., in a diseased condition of the fœtus or its involucra," and says the result of his examinations has been the converse. This is a question of personal experience, and not to be decided on very readily; the argument, however, by which Mr. Ingleby supports his opinion is quite untenable, for it by no means follows, that if malformations of the fœtus be a chief cause of abortion, then malformations would cease to be an exception to the ordinary course of nature. To make this argument of any force, it would be necessary to show that more women abort than bring forth living children.

When hemorrhage takes place at a late period of pregnancy, the author divides it into accidental and unavoidable; the former arising from a partial separation of the secundines from the point of their ordinary attachment; the latter caused by a separation of the placenta when it is fixed over the inferior portion of the womb. These are cases frequently involving the life of the patient, and require not only constant attention, but quick and decided action on the part of the medical attendant. The opinions of Mr. Ingleby are contained in the following extract:—

"In dangerous hemorrhages, then, of the accidental class, which resist approved physical treatment, we must seek to produce the contractions of the womb, either in their passive or active form, so as to constringe the ends of the detached vessels, and prevent further effusion, at least in any hurtful degree. For this purpose, the means recommended in this chapter must be cautiously pursued; and if the hemorrhage is found to continue, delivery must be accomplished, by turning the child, should the head be situated above the brim, and by the aid of the long or short forceps, should it be entering into, or already below, the brim. A proceeding analogous to this, the most rational plan that can be devised, is recommended by Baudeloque. We perceive, therefore, that though the least considerable of these hemorrhages may be stayed by the formation of coagula whilst gestation proceeds unimpaired, the more important effusions will not cease until the liquor amnii is discharged, and even in some cases until the contents of the uterus have been wholly evacuated. These last-mentioned cases so rarely occur that they afford no solid objection to our grand resource, viz. the rupture of the membranes, and the other subordinate means calculated to stimulate the uterus to an active contraction."

In some extreme cases, the loss of blood has produced such exhaustion, that manual interference or even the slightest motion of the patient may be attended with fatal consequences; and here again the plug has been as strongly condemned by some as applauded by others. The chief objection urged against its use is, that blood may accumulate in the uterine cavity, and that we only convert an external into an internal hemorrhage; the possibility of such an occurrence has been denied indeed by Duges, Capuron, and Dewees, but post-mortem investigations have fully proved, that blood may accumulate in the uterus before delivery of the child.

"In employing the tampon under rigidity of the os uteri, a two-fold object is proposed, viz. to arrest the hemorrhage, and to procure the necessary degree of relaxation. The plug, when used early, and whilst the detachment is inconsiderable, may produce either a permanent effect, so that on its removal the hemorrhage shall not recur, or, otherwise, a temporary benefit, arresting the discharge during the time of its application only. In the one case, we shall have gained an advantage beyond our expectations; in the other, we shall have attained the immediate object we had in view. - - - - - We ought never to employ the plug unless under a most pressing necessity, and with the utmost circumspection. For, although an effusion of blood in the gravid uterus very rarely occurs, the mere fact that it really does happen, must be present to our minds whenever we venture to use the plug. Whilst, therefore, I am not prepared to say, that the plug ought never to be used in these effusions, the necessity for its application will very seldom arise. I have never yet met with a case in which the os internum did not soon become sufficiently relaxed to admit of the membranes being ruptured. In the event, however, of the plug being used, no man can be justified in quitting his patient during the time of its application; rather he should exercise more than ordinary vigilance, in observing the pulse and general system, in order to detect a concealed hemorrhage, should it by possibility arise. It is proposed also to employ the tampon during a state of dangerous collapse, when a copious draining, not an active hemorrhage, is present, at which time delivery would be exceedingly perilous. Its application at such a moment, whether attended with risk or not, would be far less hazardous than that of delivery by turning. During its employment, the abdomen must be firmly compressed by means of a proper bandage."

In cases of alarming hemorrhage, arising from implantation of the placenta over the os uteri, many of the highest authorities, including Burns and Denman, advise immediate delivery, as affording the only chance of preserving the woman's life. But there are two states, which, as our author justly observes, may render immediate delivery inexpedient, viz. rigidity of the os uteri, and a state of collapse. In the former case, when manual interference is not admissible, and the patient is unable to bear the least additional loss of blood, it becomes a question whether we are justified in using the plug. Burns, though favourable to the employment of the plug generally, says, "We have only a choice of two dangers. The situation of the patient is most perilous, and I have, in practice, weighed the argument with that attention which the awful circumstances of the case required. I think myself justified in saying, that we give both mother and child the best chance of surviving, by a cautious delivery." In reference to this opinion of Burns, the author remarks, "that excessive effusion is surely one of the strongest reasons for employing the plug, and the professor probably meant, that under a very profuse bleeding the degree of relaxation necessary for delivery would soon be obtained, and thus supersede the plug. He then proceeds:—

"I have been favoured with the opinion of two of the most eminent authorities on these subjects in the present day, Sir C. M. Clarke and Dr. Blundell. Dr. Clarke, who does not seem to be apprehensive of its occasioning an internal hemorrhage, is, notwithstanding, opposed to the principle of using the plug when anything remains in the uterus to be brought away; he regards the practice as in itself unscientific, a half and uncertain measure when decision and action are indispensable, and would seek to procure contraction by the only certain means. This eminent man, who always delivers as early as possible, never lost a patient in this presentation. Dr. Blundell, on the other hand, averse as he has often declared himself to be to officious midwifery, would not hesitate to plug either in general or partial placentar presentation, 'provided (as he observes) 'the os uteri were rigid, and the gushing or draining seemed to require the remedy,' doubtless from the impression the loss shall have made upon the system. 'Repeated deaths from placentar presentation prove that the unaided powers cannot be depended upon in all cases, whilst we are waiting for relaxation.''

Sometimes the patient lies in a dangerous state of collapse, with cold skin, failing pulse, and every mark of extreme exhaustion, while a continued draining from the uterus contributes to diminish the chances of recovery. Here again the tampon may be applied with advantage until a return of strength allows the application of the appropriate means for delivery; a case illustrative of this treatment, which occurred in the practice of Mr. Ingleby *since* the publication of the present volume, will be found in another part of this week's LANCET. Its insertion has been purposely delayed by us until the present occasion.

The hemorrhages which occur after labour, are fully treated by the author in subsequent chapters, under the heads of "Retention of the placenta," "Disruption of the placenta," "States of the uterus after delivery," &c. His observations evince great practical knowledge of the various difficulties by which the labours of the obstetrical attendant are occasionally beset. On the whole, we feel justified in recommending the work as one of considerable merit. The mode of expression is clear and unostentatious, and the information it conveys highly valuable.

INOCULATION FOR THE SMALL-POX IN DEVON.[*]

IT will hardly be credited that the great bulk of medical practitioners in the south of Devon, are constantly in the habit of inoculating for the small-pox when the natural form of that disease makes its appearance; but although such an astounding assertion may goad many to a disbelief of the fact, it is not the less true. Inoculation is undoubtedly beneficial to the few, but it proves fearfully dangerous to the community at large. This opinion is supported by the best and most learned men in the profession, by all our public medical bodies—and by the recorded decisions of the legislature of this and of other countries, which decisions have been arrived at after the most patient and impartial investigation of the results of this practice.

Small-pox before the introduction of inoculation was treated by the *hot system*, when one in four or five died. After this period the *cooling treatment* was adopted, when one only in seven or eight fell victims to the disease. Inoculation was introduced in 1721, and was coming into general practice in 1732. In the 42 years which followed the latter period, notwithstanding the more successful treatment of the disease, and the practice of inoculation, the number of deaths was increased by the enormous proportion of 19 on every hundred, as compared with the 42 years which immediately preceded 1732; this is proved by the table constructed by Baron Dimsdale, by the bills of mortality, and by the evidence given before a Committee of the House of Commons in 1802; one of the witnesses stated that, in the concluding 30 years of the last century, as compared with the first 30 years, the mortality had increased from 70 in each 1000 to 95.

Deaths in every 1000 from small-pox from 1689 to 1731	72
Increased mortality in every 1000 caused by the hot treatment, taken at one-third	24
	48
Deaths in every 1000 from small-pox, from 1732 to 1772, when the cooling treatment was generally adopted ..	89

This gives an increase of 41 on every thousand, but whether the increase be 41 or 1, the conclusion must be the same—namely, that the practice is injurious. In 1763, the French Government issued a decree prohibiting inoculation for the small-pox in Paris under a conviction that it was a means of increasing the mortality. The Spanish Government arrived at the same conclusion respecting this practice in their Asiatic and South American settlements. And lastly, Dr. Crighton informs us that during the extensive employment of inoculation in Russia, "so great was the mortality caught by infection, that every seventh child died of the disease." Thirty years since, when the population of the United Kingdom was about 14,000,000, forty thousand died annually from small-pox. Taking the population now at 24,000,000, the deaths would have been 70,000 annually. But that inestimable boon vaccination was introduced at the commencement of the present century, which soon armed us against the horrors of this pestilence. The following calculations are taken from the best authorities. One in 5 or 600 of the inoculated dies—one in 75 from small-pox taken a second time—one in 3000 of *perfectly* vaccinated takes small-pox, and one case in 336 of those attacked terminates fatally—so that if all the 24,000,000 were *perfectly* vaccinated, 8000 cases of small-pox would take place in this generation, and 23 would die—instead of 70,000 annually. If this calculation be a little exaggerated, the difference is so great that the inoculators are at perfect liberty to make what additions they please to it.

WESTON GOSS, Surgeon, Dawlish.
June, 1832.

SURGICAL REFORM.

THE NEW PARLIAMENT.

To the Editor of THE LANCET.

SIR,—Will the surgeons of England, in general practice, bear in mind how influential a class of men they are, amongst the voters for members of parliament, and that now that the elective franchise is so much increased, their power over the character of the new House of Commons will, through the electors, in future be quadrupled?

And will they consent that medical and surgical reform shall progress none, while reform of all other kinds makes a gigantic stride? They have not only right on their side, now, but surgical reform will be *fashionable* the moment it is *re-agitated*, for lately it has been permitted to fall asleep while political reform was in arms.

Let them, then, briefly and impressively show all their friends and connexions that reform in the profession is a just, an important, an imperative measure. Let them trouble not the public about the details, but quote the great facts which are involved in it, and do their best to obtain promises that parliamentary candidates shall pledge themselves to a large and liberal,—a complete—reform in the government of the profession.

Look you, also, Editor of the honest and unflinching LANCET, to this matter, and so also will your friend,

A MEMBER OF THE PRESENT PARLIAMENT.
London, June 13th, 1832.

COMBINATION OF OPIUM AND ACETATE OF LEAD.

To the Editor of THE LANCET.

SIR,—Illness and absence from town have prevented my seeing until now, in one of your Numbers for April, that my letter has been inserted, and in another, that it has been answered, or at least noticed. In reply to my charge against Dr. Addison, your correspondent says, that "not one-sixth part of the acetate of lead is decomposed." Had he made the experiment, he would not have advanced the argument he has used. Let him dissolve a pill, composed, according to Dr. Addison's directions, in boiling water, and then having filtered the solution, pass sulphuretted hydrogen through the clear liquid; he will perhaps be surprised to observe how small a quantity of sulphuret of lead is thrown down. If he then convert this into chloride of lead, 20 parts of which indicate 23.25 of the acetate, he will be able to give your readers more information than by simply denying my statement. But allowing all that he has written to he fact, why does Dr. Addison use opium at all? Why not substitute the acetate of morphia at once, and thus produce a uniform mixture? Different specimens of opium vary considerably in strength, so that in some cases we may have the whole of the acetate of lead decomposed, and in others merely a part of it; this will also prevent your correspondent from discovering his error, unless he use good opium. I am, Sir, your obedient servant,

GUYENSIS.

TO CORRESPONDENTS.

S. T. An affirmative reply to the first question might be correct, but as there are some considerations which might render it necessary to qualify this answer, our correspondent should obtain a copy of the regulations at head-quarters.

Tyro may be assured that our best exertions will be used in behalf, not only of our young, but our adult friends.

If *An Old Subscriber* be well known in the profession, courtesy would have demanded that the "five-and-twenty" should first have called on him. If not, he must not expect it. Had he no letters of introduction?

Can *Mr. Pidduck* forward us a description of the fracture apparatus? It needs an explanation.

We have received *Mr. Little's* cases of saline injection, and beg to be favoured with the details of the dissections which were performed.

On reflection, *Mr. C.* will acknowledge that it is impossible for us to give insertion to such a letter as he has sent. He himself, after he has got rid of his present excitement, will be the first to thank us for this decision.

The information which *D. O. C.* requires, would occupy at least a page of our Journal to impart in a useful manner. What he wishes to know should be gained from conversation with some friend who has passed through the course *D. O. C.* is anxious to pursue The notice of last week had no reference to *D. O. C.*

The case mentioned by *J. B. M.* shall be inquired into.

A Pupil might have safely trusted us with his name.

We are obliged to postpone the insertion of many letters.

Mr. Fergus's paper on the cholera at Vienna.—We have some tables before us containing further very interesting results of Mr. Fergus's observations, but the length of the ably-drawn-up and highly philosophic paper in our present number, has induced us to postpone their insertion until next week.

The letter of *Mr.* ——, dated June 16, reached us after all the previous pages of our Journal had been arranged. The engravings, &c., shall be the subject of a private communication. His conjectures, as to the motives of certain parties, are very probably correct to a letter.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JUNE 30. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXIV.

THE FRONTAL BONES AND ANTLERS OF DEER.—THE FRONTALS OF SWINE AND DOGS, AND THE TEMPORAL, PARIETAL, OCCIPITAL, ÆTHMOID, AND SPHENOID BONES OF HORSES, CATTLE, SHEEP, SWINE, AND DOGS.

The Frontal Bones of the Deer.—Were I a phrenologist I should probably detain you a little while by a description of the beautifully irregular surface of the external plate of the frontal bones in our occasional patient, the deer; covering in him a considerable portion of the brain; and, from the almost total absence of the frontal sinuses, or those diminutive cells, being confined to the orbitar plate and process alone, actually corresponding with the irregularities on the surface of the brain. I will content myself, however, with simply pointing out the extension of the frontal bones down the face; their suture with the nasals; the extraordinarily involved suture between the frontals; beginning below, with a mere approximation of parts, where the nasal cavities alone are found: a dove-tailed union commencing as soon as this bone begins to cover the brain; and increasing in complexity and in strength, and the bones thickening and elevating themselves into a considerable crest or ridge as we proceed backward, until we arrive at the parietals, where the intricacy of the union would baffle all human skill to copy. Compare these heads of the sheep and the deer;—the head and the horns are the weapons of offence;—see the different form of the cranial covering in these two animals, and how the brain is preserved in the one by being lowered out of the way of danger, and in the other, by this ridge of bone and wonderful complexity of suture. These are illustrations of design which should not escape us. To these I may add, the long and wide, and deep, and polished sulcus connected with the foramen supra orbitarium, and the projection and turning up of the superciliary ridge to deepen and to protect this sulcus, while it gives prominency and expression to the eye. The depression across the frontal bones, resembling in situation and character that across the nasal bones of the blood-horse, indicating the degree of breeding, and sometimes a wild and unsubdued temper, should not be overlooked; but that which principally deserves attention, is the different construction of the horn or antler.

The Antlers. The First Tubercle.—With the exception of the rein-deer, they are confined to the males. The female rein-deer have smaller antlers than the males. At birth there is little difference between the crania of the male and female fawn; but at six months old a small protuberance marks the place of the future antler. It grows, but does not exceed a few inches in length, and assumes a cylindrical form, covered by a prolongation of the skin. It consists of a short solid process of the frontal bone, bearing on its summit a tubercle composed of a substance resembling that of the antler of the adult animal, and which is a singular compound of horny and bony matter; solid, and even denser and harder than bone. The skin covering the tip of this cylindrical body at length gives way to its pressure, and a diminutive horn appears.

The Second Antler.—After the cuticle has peeled off, the tubercle remains stationary for several months. The constitution then begins to prepare for the annual œstrum, or desire to perpetuate the species. The testicles of the fawn begin to appear, and those of the adult animal which had shrunk when the period of œstrum had passed, again enlarge; the vital current flows with increased rapidity and volume through the frame. There is a mysterious sympathy between the antler, the badge or designation of the sex, and the development of the generative organs; and the external carotid, by branches from which the frontals are supplied, increases in size, and more blood is determined to this part. It cannot, however, be employed in the nutrition and growth of the antler, for the vehicle which contained the ramifications of the artery, the skin or periosteum of the antler, is gone. Nature, however, finds out a way to accomplish her purpose: a kind of inflammation is set up at the line of demarcation between the antler and its bony base; a reddish ring is observed at the base of the antler, and surrounding it; it may be plainly enough seen in the adult, and traced with very little difficulty in the fawn. The inflammation runs its course, and the process of exfoliation is accomplished. As the dead bone is thrown off from the living beneath it, so this tubercle, or the full-grown antler, is detached from the long process of the frontal. This shedding or *mewing* of the horn takes place in the adult animal in the latter end of February or March—he has enormous antlers to complete before he meets his mate: the tubercle of the fawn remains until May.

The Reproduction of the Antler.—A sore is now left on the summit of the bony pedestal or base, from which hemorrhage sometimes takes place for two or three days; but at length it skins over, and a pellicle or prolongation of the surrounding integument perfectly covers it. I have spoken, however, of the enlarged caliber of the carotid, and more than the usual quantity of blood which pours on in this direction, and must be employed. First, there appears to be a bony secretion; an irregular bony ring, a burr, forms around the summit of the frontal process; and from its centre a tumour springs, and gradually enlarges and becomes covered with a prolongation of the integument, through and over which and passing within the bony ring, thousands of blood-vessels ramify to produce and to nourish the future antler. The roughened and furrowed surface of the antler sufficiently proves how numerous and how large these vessels have been. This tumour grows and takes upon itself the precise form of the antler that is to be—it is the antler in miniature. At first it is soft—it is filled with a gelatinous fluid; but the usual process of ossification takes place, and this gelatine speedily gives place to a cartilaginous substance. The antler grows and attains nearly its destined bulk; when it becomes still harder: and the cartilage yields in its turn to a compound of horn and bone, without the elasticity of the former, and with more than the density and strength of the latter. This hardening process commences at the base, and spreads upward, the antler above continuing to grow while any portion of it remains cartilaginous. At first the antler grows with strange rapidity—there is nothing in the animal frame that can be compared with it. Antlers weighing 28 or 30lbs. are completely formed in little more than two months; and I have been told, that the horns of some of the Wapiti deer in the Zoological gardens have increased in length more than an inch daily.

Separation of the Integument.—While this process of hardening is going forward, the vessels are necessarily compressed, and diminished in caliber. They are particularly and most speedily so within the burr or ring at the base, and they become at length obliterated there; and all nourishment is cut off from the covering of the antler, and from the antler itself. The cuticle, deprived of nourishment, soon dies—it peels off—it hangs in strips and rags about the antler, and the animal annoyed by it rubs it off against everything within its reach. The antler, having once become solid, remains unchanged notwithstanding the deprivation of nutriment, and continues its allotted time upon the forehead, and when in due time it is detached from its base, it is more enduring even than bone isself. This seems to be a painful process to the animal—the growing antler is exceedingly tender. We may well imagine this when we consider the vascularity of the membrane by which it is covered; the stag accordingly retreats into the thickest coverts, and conceals himself as much as he can, until he once more has his branching honours full upon him.

The Age of the Stag as indicated by the Antlers.—The antler of the first year is a mere tubercle; the second year produces a simple, straight-pointed horn; in the third year the branches begin to appear,—two and sometimes three of them. The usual indication of the age by the number of branches is a very uncertain one. The stag of six years old has generally ten or more branches of different sizes; and, after that, the age is guessed at by the size of these branches, as well as the more *palmated* and *crowned* appearance of them: while the antler increases every year even to very old age. Of that age there are some strange stories told. The stag was said to live for a century or more, and we have an account of one that was taken by Charles VI. in the forest of Senlis, with a collar round his neck bearing this inscription, "Cæsar hoc mihi doñavit." Many who were fond of the marvellous at once concluded that this stag was probably a thousand years old, and that the collar had been given by a Roman emperor; forgetting that he might have migrated from Germany, the emperors of which assume the name of Cæsar. The average age is eighteen or twenty years, although some have been undeniably proved to be at least thirty-five years old.

Connexion of the Antler with the Generative System.—In every animal whether wearing antlers or horns, we trace a mysterious connexion between their development and the function of generation. The branching horn of the ox and the stinted horn of the sheep are proofs of this; but it is more evident in the ruminants with antlers. The process of the casting of the antler commences when the period of œstrum or rutting is approaching, and the testicles are beginning to be developed. During the growth of the horn they enlarge, and when the antler is perfect the stag is prepared to perpetuate his species. If he is castrated during the growth of the antler it falls off, and a little while afterwards a smaller and comparatively insignificant one appears, which is not afterwards shed, and which does not perfectly harden.

Use of the Antlers.—If I am asked why there is this wonderful provision for these immense bodies of so short a continuance, (for they are not perfect until August, and they are shed in March,) it will be sufficient to reply, that they are highly ornamental to the animal, and that we have found a method of converting them to numerous useful purposes. There are many things which we could spare better than the horns of the stag.

THE FRONTAL BONES OF THE SWINE.

These are of a shield-like form, and as flat as those of the sheep are convex; yet the frontal sinuses are much larger than in the polled sheep, and scarcely less than those in the sheep with horns. They occupy the whole of the parietal and temporal bones as well as the frontals, and reach, as in the ox, even as far as the condyles of the occipital bone. The cells are more symmetrically arranged than in the sheep or the ox; there are seven or eight distinct rows of them. The use of this is not manifest. Do they contribute to the piercing shrillness of his cry? or to that acuteness of scent which is necessary to enable him to discover his food, sometimes buried in the earth, or at others surrounded by a strange compound of villanous smells?

You will observe in this animal the peculiar strength of suture by which the frontals are united to the nasals, in order to enble him to dig and burrow for his food—the deep channel for the security of the vessels rom the foramen supra orbitarium — the situation of that foramen, falsely called upra orbitarium here, for it is even below he orbit—the downward direction of the hannel to guide the vessels to the prolonged inferior portion of the face, while the forehead is supplied principally from the temporals—but more particularly you will observe the difference in the orbitar process.

The Peculiarity of the Orbitar Process in Omnivorous and Carnivorous Animals.—This process, instead of being prolonged to meet either the zygomatic arch, as in the horse, or the bifurcation of the malar bone as in the ox and sheep, is merely a short and insignificant tubercle, and the orbit is completed by the interposition of cartilage. This deficiency in the outer part of the margin of the orbit forms, with very few exceptions, and these principally the bimana and quadrumana, a plain distinction in comparative anatomy between the herbivorous, omnivorous, and carnivorous animals. In the one, the bony margin in the front of the orbit is complete; in the others, a portion is supplied by cartilage. The reason of this is evident. This process of the frontal bone is a kind of boundary to the temporal fossa, in which the coracoid process of the lower jaw plays, and around which the temporal muscle (that which is chiefly concerned in mastication) is wrapped. The herbivorous animal has no occasion for any very extensive play of this bone or violent action of the muscle; and additional security may be given to the eye by making its external margin completely bony. The food is easily grasped, and the teeth are not the usual or chief weapons of offence. It is different, however, with the omnivorous and carnivorous animals, who require extensive play of the bone and violent action of the muscle in order to enable them to grasp their prey, or contend with their foes. The complete bony margin of the eye would limit them too much, and the yielding, elastic cartilage is interposed, to give the requisite room.

THE FRONTAL BONES OF THE DOG.

On these I will not detain you. They are also of a shield-like form. There is a peculiar longitudinal depression commencing at the parietal ridge, and running down to the extremity of the nasals. The situation of the frontal sinuses is marked by distinct protuberances, and there are no supra-orbitar foramina. The orbitar processes are also shorter and the interposed cartilage proportionably longer than in the hog, because this is a carnivorous animal; and for our service, as well as in his natural state, he requires more powerful action of the temporal muscles, and more extensive play of the coracoid process of the lower jaw.

Of the other bones of the cranium my account will be exceedingly brief, for they admit of little physiological and less pathological illustration.

THE PARIETAL BONES.

The Horse.—Above the frontal bones are the parietals. If I were speaking of the adult animal I should say the parietal, for although these bones are distinct from each other in the fœtus and the colt, all trace of suture is lost before the horse is three years old; and nothing remains but a kind of ridge which they both contribute to form. They are surrounded by the frontals, the temporals, and the occipital. They are united to the first anteriorly by the coronal suture; to the second inferiorly by the squamous; and to the last posteriorly and superiorly by the lambdoidal; but the lambdoidal is generally obliterated in the adult horse. They cover the middle portion of the brain, and constitute the centre of the roof of the cranium.

The texture of the parietals is considerably closer and harder than that of the frontals, for they take a more important share in the defence of the brain. Their diploe is exceedingly small, and the external and internal plates are in a manner agglutinated together, except in old horses, in which, as I have observed in a former lecture, the frontal sinuses reach to the occiput.

They are completely covered by the temporal muscle on each side, which I have just spoken of, as the main agent in mastication, and which, not only arising from the whole of their convex surface, but from the anterior portion of the occipital, from the squamous portion of the temporal, from the base of the zygomatic arch, and even from a portion of the frontal, possesses immense strength. It suddenly narrows, and, taking a curved direction, winds itself round the coracoid process of the lower jaw, in order to move which it needed to be immensely strong, for it acts with enormous mechanical disadvantage. This, however, belongs to the digestive system.

This origin from the whole of the convex surface of the parietal bones is given to the temporal muscle for another important purpose, viz. more effectually to preserve the brain from injury. It is the part of the cranium most exposed to injury, and it covers the greater part of the brain. The horse has no hands to ward off many a danger, and this muscle is placed here, by its softness and elasticity, to neutralize almost any violence that could be offered. There is but one line of bone uncovered, and that is the thick ridge of the parietals, prominent both above and below; on each side is a dense massy muscle offering that securest of all defences, a gradually-yielding resistance; it is the hand receding and stopping without injury or the slightest shock, the cricket-ball hurled or driven with the greatest force,—it is the pillow of feathers, the very cover of which a pistol-ball cannot penetrate,—or the wool-pack hung over the ship or the wall, and from which the cannon-ball falls harmless. The parietal of the human being is undefended, the whole of the cranial roof is exposed; but nature has here given a secure protection to him who has neither hands nor intelligence to ward off the blow.

On the internal plate of the skull there is no suture between the parietals, but there is a ridge-like projection corresponding with that on the external plate. Along the centre of this ridge runs a groove for the reception of the longitudinal sinus. Posteriorly is a triangular bony process, forming a portion of the bony tentorium, peculiar to the horse and the dog; while the transverse grooves for the reception of the lateral sinuses are sufficiently evident.

In Cattle.—The parietal bone in cattle is a very different one from that which I have just described. It is but one bone. Here is the head of a fœtal calf, and not the vestige of a suture can be traced on the parietal; in a very early stage, however, of fœtal existence there are two parietals. Not the smallest portion of it appears on the superior part of the head, except at what we should term the occipital crest in the horse. That ridge or crest is formed in cattle by the union of the frontal and parietal bones. Below this ridge it extends down almost to the foramen magnum, occupying the place of the occipital bone, and giving insertion to the muscles attached to that bone in the horse, and also to the ligament of the neck, and hollowed by cells to its very extremity. When it approaches the root of the horn it suddenly narrows, it becomes a mere band between the occipital and temporal, and the frontal bones; arriving in the temporal fossa it spreads under the horn, contributes to give it some support, and unites with the frontal by a denticulated and squamous suture. It then suddenly narrows, and is lost in a process between the frontal and the temporal.

In Sheep.—In the sheep it has the same relative position as in the horse, placed anteriorly to the occipital ridge, and between the frontal and occipital bones; but as there is no muscular covering here, it is depressed perfectly out of the reach of danger. The lower and temporal portion of it is alone covered by muscle.

In the Hog.—The parietal in swine is a singularly-shaped bone, occupying but a little portion of the cranial roof, and presenting as many angles as the shield of any templar or crusader. If that portion which enters in to the construction of the roof of the cranium is flat and devoid of muscular covering, the symmetrical arrangement of arched cells beneath gives it sufficient strength.

In the Dog.—The parietal bones in this animal form almost the whole of the roof and sides of the cranium, and are even more deeply covered by the temporal muscle than the parietals of the horse. The consequence of this is that the brain of the dog is securely defended, and it is almost impossible to destroy him by knocking him on the head.

The different Form of the Parietals in the Dog.—M. F. Cuvier has endeavoured to arrange the varieties of dogs into three groups, two of them principally depending on the form of the parietals, either insensibly approaching each other so as to contract the cerebral cavity, or swelling out and becoming rounded, so as to enlarge the hollow of the skull. The third variety is characterized by the high frontal and extensive sinuses, and diminished cranial cavity. Among the first are the wild dogs, whether the East Indian, or the North or South American, the Albanian, the Dalmatian, and ll the varieties of the greyhound; all of hem possessed of comparatively little intelligence. Among the second are the ost useful and intelligent of the canine pecies, the spaniel, the terrier, the hound, e poodle, the Siberian, and above all the ewfoundland, the Alpine, and the sheperd's dog. To the third belong the pug, ll-dog, and mastiff.

M. F. Cuvier's Arrangement erroneous.—I never could satisfy myself of the propriety of this arrangement. I have now before me the cranium of a greyhound with the parietals decidedly rounded and expanded and that of a Newfoundland-dog with the same bones narrowing superiorly and ridge-formed to a degree that almost staggers my faith in the connexion between a capacious cranium and superior intelligence. The fact is, the form of the parietals depends simply upon the size of the dog. The small spaniel and the terrier have the parietals rounded so as to form almost a semicircle. There is scarcely the vestige of a ridge. The skull is polished and smooth, and the temporal muscle is spread thinly over the whole. They want no great power of muscle to seize their diminutive prey, or to defend themselves from their ordinary enemies. Even the vermin-terrier has this round cranium. But the terrier and the spaniel of larger size, and destined for other purposes, needs, occasionally at least, greater strength of jaw; and then the parietals are roughened, and there arises from its centre, formed by them and by a process of the occipital bone, a considerable ridge to give a firmer and tendinous attachment to the temporal muscle; and that this ridge may be firmly based, the sides of the parietals lose their rounded form and assume a slanting direction, and the cranial cavity and the quantity of brain are to a certain degree diminished, *at least superiorly*. Hence the difference in the form of the parietal, depending not so much in the breed as on the size of the dog. One thing, however, is observable, that while the thickness of the ridge in the centre and the yielding resistance of the muscle on each side defend the skull not only from ordinary, but unusual violence, the want of the ridge, and the thinner expansion of muscle on the crown of the skull in the smaller dog, are compensated by a very curious extra plate of bone, which sometimes extends a considerable distance on either side of the parietal suture.

THE TEMPORAL BONES.

In the Horse.—There is a temporal bone on each side at the lower part of the wall of the arch, and each may be divided into three portions:—The *Petrous*, or hard, stony portion, incloses the mechanism of the ear, and is considerably developed within the cranial cavity. I shall speak of this when treating of the auditory system. The *Tympanal* portion, evidently distinct in the horse and the dog, and the prolongation of which forms the external bony meatus, while the internal is expanded into several distinct cells. And lastly, the *Squamous*, forming the base of the zygomatic arch, and overlapping the parietal bone in the form of a shell. It contributes only in a slight degree to the construction of the cranial cavity. That portion of it which helps to make the zygomatic arch, and also contains the glenoid cavity, into which the condyloid process of the lower jaw is received, and constituting the joint of the lower jaw, belongs to the digestive system, and all that we are here concerned with is the squamous or overlapping portion placed as a buttress at the base of the cranial arch to give it strength;—the kind of union best adapted in this situation to neutralize the lateral pressure, and to prevent the base of the arch from spurring out, when there is pressure or concussion on the crown.

In Cattle.—There is a remarkable variation in the glenoid cavity in cattle, conformable to the different mode of mastication, or, rather, to the process of remastication; and from the different situation of the parietal, no longer the wall of the arch, but a mere band, deep in the temporal fossa, and having no stress upon it, the *squamous* suture was not wanted, and is not found; it is only in a slight degree *dovetailed*.

In the Sheep, Hog, and Dog.—The squamous suture is found in each of these animals; because, from the form of the cranium, strength is required at the base of the arch, but in none of them is this portion of the bone at all developed in the cranial cavity. It does not enter into it in the least degree in the sheep.

The Ossa Triquetra.—In the horse, the sheep, and more indistinctly in the dog, there is a singularly elongated, yet somewhat triangular, bone interposed on each side between the squamous portion of the temporal and the occipital bone, and attached at first by suture, and afterwards by bony union to the petrous portion of the temporal bone—the *os triquetrum*, or *three-sided bone*. It reaches from the base of the occipital crest to the petrous portion of the temporal. It seems to fill up a sort of hiatus or gap between these bones. Of its use I will not pretend to speak, but Carus is wrong when he traces the bony tentorium of the horse and dog from the ossa triquetra. It is evidently derived from the parietals, and from both the petrous and squamous portions of the temporal. There is a deep sulcus or channel about the centre of this bone, in which runs the occipital artery in its way to the cranium.

THE OCCIPITAL BONE.

In the Horse.—This bone occupies the same relative situation in the human being and the quadruped, although there is much apparent difference in its position. A consideration of the attachment of the head to the neck in the one and the other will explain every difficulty. In the foal it is composed of four portions, the crest or tuberosity, the condyloid processes on either side, and the basilar portion or base of the bone. The crest or tuberosity is arch-shaped—it is concave anteriorly, and convex posteriorly, and in this form offering the greatest possible resistance to injury, and protection to the cerebellum. It gives origin to the complexus. The continuation of the crest descending towards the styloid process on either side, gives attachment to the obliquus capitis superior. Below, and in the centre, is a spinous process—some call it a tuberosity—to which is attached the curiously elastic cervical ligament. On each side is a broad roughened surface, whence spring the recti postici muscles; and below is the foramen magnum, through which the spinal chord and the sub-occipital nerves escape from the cranial cavity and the vertebral arteries, and the accessory nerves enter into it. Still lower, and taking a direction anteriorly, is a solid projection of bone termed the basilar (leading to the brain the noblest, or *king* among the organs), or, from its shape, the cuneiform, wedge-like process; it is the posterior termination of the floor, or, rather, it is the base of the cranial cavity or arch. It is solid, to give strength to the arch, and to break and stop every vibration or concussion that may be communicated to it. In this it is assisted by the sphenoid bone, the posterior and solid part of which early becomes inseparably united with this portion of the occipital bone, and so makes one continued solid base along the whole of the greater part of the cranium. On either side of the foramen magnum is a beak-like prolongation termed the coracoid or styloid process of the occipital bone. It is peculiar to animals whose heads are set on in a slanting direction. It gives attachment to the obliquus capitis anticus. Between these and the condyles are the condyloid notches or fossæ, and immediately anterior to the condyles are the foramina through which the lingual nerves escape.

In Cattle.—I have anticipated the main difference in this bone in cattle when describing the parietals; there is no crest or tuberosity. The condyles are not so much developed, and the styloid processes are smaller and curved. The basilar process is somewhat increased in breadth and more tuberculous, but much diminished in length and there are two condyloid foramina, th one placed externally and the other internally.

In Sheep.—There is here an occipit ridge, but it is depressed far in the pol and presents a very irregular projectio with a direction posteriorly and downward The styloid processes are less curved, a the basilar process yet more compresse broad, and short, with a peculiar irreglarity of surface for the insertion of t flexor muscles of the head. You have served the nodding motion of the shp as he crops the herbage. These had gun to appear in the suture between e occipital and sphenoid bones in the x. There are likewise two condyloid forama.

In the Hog.—The ridge of the occital bone is bold and prominent, and forms ot unpleasing termination to the craniusuperiorly and posteriorly. The symtry of the parts would scarcely be suspec in the clumsy head of the living animal. The ridge presents a reverse form to that the horse; the concave surface is placeposteriorly, and the expanse of the bo beneath the crest is considerable an perfectly smooth. The cervical ligamedoes not reach to the occipital bone he and the hog has no occasion for any extordinary strength in the muscles of thneck. The styloid processes are long, slenc, and straight, and the condyloid forama, of which there are two, are close to base of it. The cuneiform process is sho thin, and weak.

In the Dog.—Nothing can be so different as the occipital bone in the small and large dog—in him, whose prey is diminutive, and the other who has occasionally to contend with noble antagonists. A ridge of the occipital bone is received between the posterior edges of the parietals, and is bold or insignificant, according to the destiny of the dog. The cervical ligament reaches here no farther than the dentata; but in the large dog the posterior surface of the occipital is roughened to a degree scarcely credible, for the insertion of the muscles of the neck, so powerful in this animal: there is nothing in the horse or the ox comparable to it; and there is no animal who makes so much use of the cervical muscles in shaking or worrying his foe or his prey. This surface is also unusually broad in the more powerful dogs, and the cranium is correspondingly so. There is no false projection of the temporal bone, as in every other domesticated quadruped. The width of the lower part of this occipital surface is the actual width of the cranial cavity, and thus compensation is made for the slanting direction of the parietals, and the comparative narrowness of the skull superiorly and anteriorly. When I thus examine it, I find sufficient cranial capacity and bulk of brain in my favourite; Newfoundland and Alpine and shepherd's dog. The styloid processes are short, and the condyloid foramina double.

THE SPHENOID BONE.

Of the singularly-formed sphenoid bone I shall say but little. Its body is early consolidated with the basilar process of the occipital for the sake of strength, while the union between it and the anterior portion is often exceedingly slight. The body of the one is convex and smooth inferiorly, and regularly concave superiorly and anteorly: on the centre of it lies the pituitary and. From the sides of the body there tend two broad and thin bony plates, ich are termed the wings. The termiion of them is seen in the posterior part o the orbit; and within the cranial cavity ty support tho middle lobes of the brain. Fm the base of these wings there are two presses or legs; they skirt the æthmoid bos, unite with the palatine, and afford athment to the internal pterigoid muscl The anterior part of the body of the bo is hollowed into cells in the horse, but wh are not found in the ox. The sphenobone is small in that animal. In cattle theuneiform process is deeper and less visle, and the pterigoid processes longer; anchere is a very considerable difference in t arrangement of the foramina through whi the nerves proceed from the brain, and the arteries enter into it, and which will be noticed when we describe the course of the nerves. The external pterigoid process in the dog presents a curious loop, round which the tendon of the circumflexus muscle plays as over a pulley.

THE ÆTHMOID BONE.

The crista galli, cribriform plate, and nasal development of this bone, have been described when treating of the contents of the nasal cavity. The body of the bone is situated at the base of the skull, anteriorly to the sphenoid bone. It is hollow, and forms two cavities, called the æthmoidal sinuses, which are divided by a broad perpendicular plate; and two thin expansions of bone proceed from the sides, which are termed the wings. They spread outwards and upwards, forming the posterior and inner parts of the orbit, where from their smoothness they are called the ossa plana—their other surface forms a part of the cranial cavity, and supports the anterior lobes of the brain.

THE CRANIAL CAVITY.

All the bones, with the exception in the sheep of the squamous portion of temporal, can be easily traced in the inner plate of the cranium; the frontal, parietal, and occipital superiorly; the parietal and temporal laterally; the occipital posteriorly; the occipital, sphenoid, and æthmoid inferiorly; and the æthmoid and frontal anteriorly. There are, however, no dovetailed or intricate sutures, but the plates lie in approximation to each other; they are harder and more glassy and elastic, and by the union of these substances of evidently different structure, every vibration or concussion which might have spread within the outer plate is diminished and *damped*, if not destroyed; or that which remains is spread by the inner elastic plate over the whole of the cavity, and rendered perfectly harmless by this distention.

The longitudinal Falx and bony Tentorium.—The cavity is divided into two parts, longitudinally and transversely. Of the longitudinal division, commencing from the crista galli of the æthmoid bone, curving forward and upwards along the frontal and parietal until we reach the tentorium, forming a thickened ridge of bone for the greater security of the central arch, grooved for the longitudinal sinus, and affording attachment to the duplicature of dura mater, and dipping between and separating the lobes of the brain, I have already spoken, when describing the bones of the skull. Posteriorly and transversely there commences a more perfect bony separation. From the centre of the roof, and at the suture between the parietal and the occipital bones, evidently belonging to the parietal, and not to the occipital, as our anatomists describe it, nor to the ossa triquetra, as asserted by Carus; an irregular plate of bone descends, forming a complete division between the anterior and posterior portions of the cranium. As it dips downward, it is joined by a similar projecting plate from each petrous portion of the temporal bone, and these unite to form a curious bony curtain, which, with a duplicature of the dura mater dipping down from it, divides the cerebrum from the cerebellum, merely leaving sufficient opening for the crura cerebri to pass underneath. This bony separation is evidently designed to keep these important divisions of the brain in their proper situation, and to prevent their pressing upon and injuring each other in the various motions of the head; but why they should be found in the horse and the dog and not in other animals of rapid motion, and where the brain is subject to equal concussion and injury, it is difficult to explain Is it that these animals are peculiarly our servants, and their speed and other powers taxed not only for our pleasure, but for various important purposes of commerce, of health, and even of life itself, and that this extra security was given to their brain that they might not fail us when their services were most required?

CASE OF

DELIRIUM TREMENS.

By James S. Whitaker, *Esq.. F.L.S., &c.*

A case of this kind in a supervisor of excise lately came under my care. The symptoms of the disease are well known, and therefore I shall not say more on that head than that the man laboured under them in a very severe and determined form. All I wish is to make two or three short observations upon certain points in the treatment of this troublesome complaint, which the present case suggests to me more strongly than any that I have for some time seen. When I first saw my patient he was remarkably wild and violent, and I immediately gave him *three grains* of solid opium in a pill. Three hours afterwards he was no better, and he then took five grains of opium, with directions to repeat the dose every four hours until more quiet and comfortable. During the two following days he remained the same, as violent as ever, and never once closing his eyes in sleep; the whole of this time the opium was continued. In the evening of the third day I thought it time to increase the dose still more, and accordingly ten grains were given. An hour after this he became more tranquil, and during the early part of the night slept four hours, awaking more sensible and collected. The *opium* was now diminished to three grains. On the fourth day he had eight hours sleep, and appeared afterwards nearly as well as usual. From first to last this patient took *two drachms and one scruple of solid opium,* and yet neither the pupils nor the pulse were affected; the bowels were not constipated; nor was there any effect produced whereby it could be seen that a single grain of opium had been given, until the moment that he began to mend; and even then the anodyne, narcotic, and binding effects, were not more than would have happened from one grain under ordinary circumstances. It is therefore manifest, that though the large doses of opium had been given without *any* apparent result *at the time,* yet when an effect was produced it was not cumulative—that is, it was not a combined result of the various quantities that had been given, but a simple effect of the *ultimate dose.* I have always noticed this; but without offering any physiological argument upon it here, as it would take up too much room, I shall merely deduce the conclusions—first of all, that a precise quantity of opium, varying in each individual case according to constitution, temperament, and so on, is required to saturate (if I may so speak) the unnatural stimulus of the disease.

2ndly. The absurdity of the fear which many entertain of repeating the opium *often* in increasing doses, for, as I have shown, no effect is produced unless a certain quantity is given, and this quantity acts *irrespective of all former doses.* There is no cumulative effect. I am so persuaded of this, that I should have no hesitation in enlarging the opium to *a drachm* in any case if no relief were obtained.

3rdly. The erroneous policy of recommending, as my late esteemed friend Dr Armstrong did, and many others, that th opium should be discontinued, supposing i to be ineffectual within the first 48 hours. admit that under their treatment, if reli be not obtained in that time it is seldc found afterwards, because the already sm doses are still further diminished. I ha proved, and I again repeat, that it is p-fectly safe to give opium, regardless of quantity, until a decided effect is produd. To do otherwise than this, is to leave e patient to a precarious fate, to cast m upon the uncertain waters of chance to lengthen out a terrible disease, undene possibility of being alleviated witho a rigid pursuance of that remedy, wherel it might be so much more safely, effectu. and speedily removed. It is my inte on to enlarge upon this disease at a fure time.

225, Shoreditch, June 14, 1832.

HISTORY AND TREATMENT OF THE MALIGNANT CHOLERA AT VIENNA.

SUPPLEMENTARY TABLES TO THE PAPER OF MR. FERGUS, PUBLISHED AT PAGE 353.

TABLE OF THE RELATIVE MORTALITY FROM MALIGNANT CHOLERA IN GALITZIA, AMONGST THE POLES AND GERMAN COLONISTS.[*]

PLACE.	Inhabitants.	Population.	Duration.	Taken ill in Population.	Dead in Population.	Dead to taken ill.
Flosrow	Poles	752	80 days.	1 in 9	1 in 25	3 in 8
Gergnia	Poles, &c.	414	57	1 — 7	1 — 17	5 — 8
Woloska[†] ...	....	1457	90	1 — 14	1 — 27	5 — 7
Kmazalnka ..	Poles	910	65	2 — 13	1 — 13	7 — 6
Nawoselica ..	German Colony	355	26	1 — 35	1 —177	1 — 4
Nawosyn....	Poles	419	49	1 — 9	1 — 23	1 — 2
Weldsiz	Poles, Jews, &c.	1021	68	1 — 13	1 — 35	1 — 2
Pacskow	Poles	207	47	1 — 7	1 — 29	1 — 3
Solyn	Poles, &c.	356	36	1 — 11	1 — 59	1 — 4
Stizow......	Poles, &c.	1227	62	2 — 19	1 — 14	1 — 2
Bollchow....	Poles	2708	114	2 — 19	1 — 33	2 — 5
Engelsberg..	German Colony	175	—	1 — 46	1 — 92	1 — 1

In Debelawka, a German colony, two miles from Kmazalnka, there were only two cases; one died. New Babylon, a large German suburb, had only one or two cases.

GENERAL TABLE OF DEATHS IN VIENNA, FROM 1805 TO 1831, INCLUSIVE.

		Proportion to Year preceding.		Most Prevalent Diseases.			
Year.	Summary of Deaths.	More.	Less.	Nervous Fever.	Small Pox.	Diarrhœa, Dysentery, and Cholera.	Scarlatina.
1805	16,742	—	—	886	193	193	502
1806	20,359	3617	—	2163	2330	367	147
1807	13,764	—	6595	776	54	340	71
1808	14,924	1160	—	592	12	—	—
1809	20,218	5294	—	1352	123	1382	—
1810	17,445	—	2773	1963	560	—	—
1811	15,359	—	2086	733	589	—	—
1812	14,407	—	952	624	145	—	—
1813	12,971	—	1436	784	254	513	—
1814	15,309	2338	—	1529	116	909	—
1815	11,520	—	3789	433	73	357	—
1816	12,306	786	—	390	57	242	—
1817	12,742	436	—	405	8	284	—
1818	11,070	—	1672	480	47	122	—
1819	11,501	431	—	466	100	90	—
1820	10,822	—	679	442	58	119	—
1821	10,411	—	411	375	176	94	—
1822	11,828	1417	—	371	238	552	56
1823	11,160	—	668	529	92	121	—
1824	10,537	—	623	376	8	101	—
1825	10,959	422	—	439	46	132	—
1826	12,190	1231	—	496	59	344	—
1827	12,363	173	—	502	59	124	—
1828	13,764	1401	—	532	342	106	—
1829	13,468	—	296	669	256	63	—
1830	13,708	240	—	658	280	167	48
1831	16,784	3076	—	893	195	2198	131

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[*] Given to Mr. Fergus by Dr. Flechner, Director of Quarantine at Podgorze.

[†] Sanitary laws strongly enforced here.

GENERAL TABLE OF THE MORTALITY IN VIENNA IN EACH MONTH OF 1831.

January	1135	July	1087
February	1179	August	1213
March	1393	September	2087
April	1363	October	2186
May	1301	November	1464
June	1257	December	1119

GENERAL PROPORTIONS OF DEAD IN 1830 AND 1831.

	Men.	Women.	Boys under 10.	Girls under 10.	Old People, from 90 to 100.
Died in 1830	4076	3633	3131	2868	30
1831	5240	5181	3327	3036	40‡
Increase in 1831	1164	1548	196	168	14

‡ One of these was aged 103, one 104, and one 108.

TABLE OF DISEASES TREATED IN THE GENERAL HOSPITAL, VIENNA,

From 12th of August till end of October 1831.

	Attacked.		Cured.		Died.		
	Men.	Women.	Men.	Women.	Men.	Women.	
Came with Cholera	98	134	43	55	50	66	
Fevers	22	47	9	12	13	35	Nervous, Gastric, Intermittent, &c.
Nervous Diseases	4	12	1	3	3	9	9 Convulsions; 3 Palsy; 3 Colic.
Inflammations	2	7	1	4	1	3	Pneumonitis & Peritonitis.
Skin Diseases	5	4	5	4	0	0	4 Itch; 2 Scarlatina; 1 Small-Pox; 2 Tenia.
Ex. & Secretion Diseases	25	14	6	5	9	9	10 Diarrhœa; 5 Hæmoptysis; 5 Gout; 6 Obstructions, &c.
Cachexies	26	26	9	9	17	17	12 Phthisis; 9 Dropsy; 6 Marasmus, &c.; Syphilis.
Insane	16	28	6	8	10	208	Of all kinds.
Surgical Diseases.	8	10	1	5	7	5	12 Chronic Ulcers, mostly scrofulous; 11 of these died.
Childbed and Preg.	—	30	—	20	—	10	
Puerperal Fever	—	4	—	1	—	3	
New Born	44	34	1	1	43	33	

			Cured.	Died.
Treated with ice	from Sept. 15th, till the end of Oct.	100	65	35
Ditto	from end of Oct. till 12th of Dec.	42	34	8
Ditto	Stimulants, and hot frictions	292	128	164
Ditto	Ipecacuanha alone	21	9	12
Ditto	Ipecacuanha and stimulants	37	12	25
Ditto	Ice and stimulants	58	19	29

NOTES ON THE PRECEDING TABLE.

The cholera developed itself, in most part, during the course of the fever; in a very few cases, when the convalescence had commenced.

The intermittents were almost all tertians; the cholera broke out during the intermission, and did not prevent the occurrence of the succeeding paroxsym.

The cases of small-pox and scarlatina were in the last stage when attacked with cholera.

One who had the itch was taking sulphur.

The syphilis is no protection against cholera, in any form. Some patients had taken mercury up to the moment of being attacked with cholera.

Dropsies disappeared during, but returned eight or fourteen days after, the cessation of the cholera. However, in one case of hydrothorax the water disappeared, and did not return.

The cholera had no effect whatever on the state of mind of the insane.

Eight pregnant women brought forth dead children during the attack; three of them had living children during the recovery, two of which, however, died of the cholera. In seven the cæsarean section was made immediately after death, but always unsuccessfully.

In some the lochia stopped, but returned after the cholera; in others they continued to flow.

The secretion of milk was diminished in all. In some it ceased altogether, and did not again return.*

TABLE OF THE SICK TREATED IN THE CHOLERA HOSPITAL NO. 3 IN VIENNA,†

From September 18 *till October* 31, 1831.

	Taken ill.		Cured.		Died.	
Age.	Men.	Women.	Men.	Women.	Men.	Women.
From 10 to 20	14	10	7	8	6	2
20 to 30	20	26	12	19	8	7
30 to 40	13	16	6	7	7	7
40 to 50	14	12	6	8	7	4
50 to 60	7	11	2	2	5	8
60 to 70	6	6	3	0	2	6
70 to 80	1	3	0	0	1	3
80 to 90	0	1	0	0	0	1
	75	85	36	45	36	38
Total	160		80		74	

	Came.	Died.	Recovered.
On the 18th of September	26	14	12
19th ditto	13	6	7
20th ditto	10	4	6
21st ditto	10	5	5
22nd ditto	9	6	3
23rd ditto	9	5	4
24th ditto	4	2	2

After this the number gradually diminished, with occasional slight exacerbations.

Remained under treatment three men and three women.

Four nurses and four porters were taken ill; two of each died.

The treatment varied considerably. It consisted for the most part of bleeding, leeches, and ipecacuanha. Stimulants were employed only at the commencement. Towards the end a few experiments were made with ice, which proved its efficacy.

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* Extracted from a paper published by Dr. Güntner, in the "Medicinische Jahrbuch" of Vienna.

† Under the Primar Physicians, Drs. Shifner, Reider, Czikaneck, and Saxinger. Reduced from the protocols of the hospital.

TABLE REDUCED FROM THE PROTOCOLS OF DR. VIVENOT'S CHOLERA HOSPITAL IN THE CITY,
From September 15 *till October* 20, 1831.

Age.	Taken ill.		Cured.		Died.	
	Men.	Women.	Men.	Women.	Men.	Women.
From 10 to 20	5	6	4	5	1	1
20 to 30	5	14	4	9	1	4
30 to 40	6	10	2	7	4	2
40 to 50	6	8	2	1	3	3
50 to 60	3	6	1	2	2	3
60 to 70	3	3	0	0	2	3
70 to 80	0	1	1	0	0	1
Total	28	48	14	24	13	17

	Came.		Died.		Recovered.
On the 15th of September	2	....	0	....	2
16th ditto	9	....	4	....	
17th ditto	28	....	16	....	12
18th ditto	12	....	5	....	7

After this the epidemic diminished most rapidly in this part of the town, as we see that in these three days more were seized than in the thirty following.

Remained under treatment, two men and seven women.

Four nurses and two porters were taken ill. The former all recovered; the latter both died.

The treatment was very antiphlogistic—viz. bleeding, leeches, acids, ipecacuanha, and sinapisms. Stimulants were seldom, and then sparingly, used, &c. Ice I believe never.

IN THE MILITARY HOSPITAL FOR CHOLERA.

	Patients arrived.	Recovered.	Died.
During Sept. ..	100	55	45
Oct. ...	169	112	57
Nov. ..	33	23	10
	302	190	112

From 5th till 17th of September, of 21 treated with stimulants, 7 recovered, and 14 died. After this they were treated almost exclusively with ipecacuanha and moderate warmth during the cholera; with bleeding, leeches, and sinapisms during the reaction; and with large doses of musk, camphor, and punch, when the patient fell into typhoid fever, with muttering delirium, black tongue, &c. These patients were all robust, and were attacked with cholera, mostly after having committed excesses in eating, drinking, and being then obliged to mount guard. Owing to these causes relapses were very frequent and often fatal. In two cases the patients had a return of most violent cholera, and yet escaped. No nurse was taken ill. One young surgeon died three days after having come to the hospital in the beginning of December, when instances of contagion were seen in other parts of the town.

CASE OF

VENTRAL PREGNANCY.

By J. DICKSON, M. D., *Armagh.*

—

ON Thursday, the 17th of May, about three o'clock p.m., I was called upon as dispensary physician, to visit Mrs. Walker, wife of a tradesman in this place, and who had been in labour since the preceding night.

On entering her apartment, I was informed that the waters had already broke, of which there was a great quantity; and that the pains were apparently strong and frequent. It was feared the pains were of little service. In the appearance of the woman at this time, there was nothing remarkable; she was extremely anxious, and expressed her fears for the result. She mentioned that her full time had passed; that the motions of the child were strong, and she was at a loss to account for its not making greater progress. I endeavoured to remove her apprehensions, and laying my hand on her abdomen, which was unusually flaccid, nor more protuberant than is usual in the seventh month of pregnancy, I was unable to ascertain the uterine contractile efforts during a pain. Introducing my hand per vaginam, the parts had all the feel as if a large quantity of water had been recently evacuated, and were quite dilated; neither the os uteri, or any septum between the uterus and vagina, was perceptible. But what surprised me was, the absence of a presentation; by no means could I distinguish the presence of a fœtus in utero. I withdrew my hand, if not alarmed, impressed with the necessity for circumspection; but as I had often seen the pains become inefficient from the spontaneous breaking of the membranes, and consequent discharge of the liquor amnii, I considered it proper to suspend, for some time, the pains, and procure for the woman repose, of which she was very much in want. Before leaving her I inquired into her previous condition, and was told that this was her second pregnancy; that her first had been unusually tedious; and that my predecessor, Mr. Nesbit, had been obliged to deliver her with the forceps.

On the following morning I was again summoned, and found her much in the situation I have described above; she was more irritable; the pulse was quicker; she had vomited a dark-green-coloured fluid; there was no impediment to the discharge of the urine or fæces. I now made them acquainted with my apprehensions, that something very unusual existed in her case, but that of its exact nature I was uncertain, and requested that the assistance of another practitioner should be procured.

At noon there was no advancement in her labour; the constitutional irritation was increasing; she vomited repeatedly; the pulse was every hour becoming more frequent and small; her anxiety was excessive, and she implored me to relieve her in any way that I thought proper. My friend, Dr. Trevor, had been sent for, and until his arrival I begged of her to be patient, and endeavoured to keep up her strength by administering, at intervals, active stimuli. This gentleman being from home, it was five o'clock before he arrived; and a few minutes before that hour she expired, as much from exhaustion as from irritation, for during the latter part of her life the pains were almost incessant.

Post-mortem Examination.

On the following morning an examination of the body of the patient took place, in the presence of Dr. Trevor and one or two of the relatives of the deceased. The abdomen was greatly distended, and gave the feel as if a fœtus lay under the integuments. An opening being made, the first object that presented itself, was a fully-formed female child, perfect in all its parts, and not exhibiting a trace of decay. It occupied the entire extent of the cavity of the belly, from the innominata to the ensiform cartilage, resting upon, and nearly covering, the viscera. The head presented towards the pelvis, describing as usual at the commencement of natural labour the lateral aspect of its bason, though not occupying the cavity. The legs were doubled on the thighs, over which lay a fold or two of intestine. Having satisfied ourselves with its appearance in situ, Dr. Trevor removed it very quickly from the mother; and we proceeded to search for the placenta. This substance lay nearly under the child, with the maternal surface next to the intestines; but, to our astonishment, it was quite unattached to the mesentery, or any other part, and differed merely in being thinner and less vascular, from a placenta under other circumstances. It was also removed by the chord from its situation.

The uterus we found in its natural situation, perfectly healthy in its texture, pale, and about four times the size of one in the unimpregnated state. Its sides were about nine lines in thickness, and in its cavity there was a small quantity of coloured fluid. On its anterior surface, and a little above the cervix, we discovered a rent of about two inches in its greater (lateral) direction, the edges of the breach, which was quite open, presenting a ragged appearance; they were a little more vascular than the body of the uterus, though quite healed.

The colon was greatly distended with air; the intestines did not bear any marks of inflammation. All the other viscera were perfectly healthy, and in their natural situations.

I should mention that the child had no peculiar covering, nor could a trace of the membranes be discovered, or any sac capable of containing the large quantity of water that was discharged.

Upon a review of this case, which differs from any that I can find a record of, it would appear that the only prospect of relief that presented itself would have been by means of a section of the abdomen, and that of no trifling dimensions; but this from the constitution of the woman, and her rapid sinking, I feel confident she could not have survived. A fact with which I was made acquainted at the post-mortem examination elucidates the nature of the affection. In the fourth month of pregnancy she got a fall, in consequence of which the uterus was ruptured, and the child passed through the aperture into the cavity of the abdomen, where it remained till the full period of utero-gestation would have been completed.

In a physiological point of view, it would be extremely satisfactory to understand by what means the nourishment of the fœtus proceeded,—as from the relative situation of the placenta and uterus in the abdomen, from the appearance of lining membrane of the latter organ, and also from the opening in the uterus bearing no proportion to the bulk of the placenta, both Dr. Trevor and I considered it quite impossible that they could recently have had any connexion.

Poynty-Pass, Armagh,
8th June, 1832.

TWO CASES OF

MALIGNANT CHOLERA

IN WHICH

VENOUS INJECTIONS

WERE SUCCESSFULLY EMPLOYED.

To the Editor of THE LANCET.

SIR,—In compliance with the wish of the professional gentlemen engaged, I transmit to you for insertion the two following cases of cholera treated by the injection of saline solutions into the veins. The operation was performed by Messrs. Little and Bennett,—in the first case, in the presence of Mr. Luff, the district-surgeon, and myself; in the second, in the presence of Messrs. Luff, Vandenburgh, Moore, and myself. Although the result has not been so satisfactory as we could have wished, still it will be seen that some, and even considerable, alleviation of the most urgent symptoms took place. As injection has been successfully practised in other instances, I am willing to hope that future experience will more completely develop the rationale of the practice, and the principles upon which it should be conducted, so that it may prove more uniformly successful. I shall, therefore, in the following detail, confine myself to facts, without offering a single comment.

I am, Sir, yours, &c.
ROBERT VENABLES.
7, Suffolk Place, Hackney Road.
June 29th, 1832.

CASE I.

On Friday morning, the 15th instant, I visited with Mr. Luff, a young man named John Tennant, aged about 24 or 25. We found him suffering from severe vomiting and purging of a gruelly-looking fluid, without any intermixture of either bile or fæces. The countenance was sallow and sharpened, and expressive of great languor and distress; the eyes deeply imbedded in the orbits; there were great coldness and flaccidity of the skin, and lividity of nails, extending along the fingers to the first and second joints; coldness of the tongue; pulse perceptible, but very weak and frequent, 120; great thirst; the patient constantly crying out for cold water; burning heat of stomach; urinary suppression for the last twelve hours. He had been taken ill the preceding evening, but no more perfect history could be obtained. Mr. Luff administered a dose of calomel, and the saline treatment was adopted and continued throughout the day.* In the evening, about half past six, he was visited by Mr. Little and by Mr. Bennett, and they found him much exhausted, probably from the immediately previous exertion of getting up to the close-stool. They found him pulseless, with all the symptoms much aggravated. It was now decided that the saline injection should be tried, and on visiting him at ten o'clock p.m. for the purpose of injecting the veins, previously to the operation the following minute was made of the symptoms:—

Pulse very feeble, thready, and easily compressed; frequency, 118; respiration, 37; temperature sublingual, F. 95°; axillary, 93°; palmar, 79°; meatus auditorius externus, 93°. The urinary suppression still continued; the vomiting and purging copious, and as frequent as before; the voice languid but audible. As the patient was considered to be somewhat improved since the visit of Messrs. Little and Bennett, it was agreed to defer the operation for an hour or two, to ascertain whether the amendment would advance. At half past twelve we again visited him, and found that the vomiting and purging of the gruelly-looking fluid continued as severe as ever. Feels much worse; feet more venous; the hands and forearms of a dusky-brown hue. Pulse 118, weak, and thready; intense thirst; "facies cholerica" well marked; respiration and animal temperature as before. Mr. Little now laid bare the basilic vein of the right arm, and having detached it, passed a probe under it. An incision having been made into the denuded vein, 50 ounces of a saline solution composed as follows, were slowly and carefully injected.

℞ *Sodæ muriatis*, ʒj;
Sodæ carbonatis, ℈j;
Potassæ chloratis, gr. vii;
Aquæ calidæ, temp. F. 110°. ℥xl.

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* Consisting in the administration of carbonate and muriate of soda, with a small proportion of chlorate of potass in solution.

The temperature of the injection was kept up to 110°, by the occasional addition of a similar solution at a higher temperature, whenever the thermometer, which was kept constantly in the basin, indicated any reduction.

After twenty ounces had been injected, he expressed himself better, but complained of more pain in the abdomen. The pulse fell to 104°, was fuller and more regular. During the injection he complained of a sense of weight or fulness, with uneasiness in the chest, though we could perceive little or no alteration in the respiration. This was followed by a severe rigor, and a sensation of cold. He also had a fit of vomiting. These circumstances induced us to discontinue the injection, the pulse being 94, fuller, and steady.

Saturday, 1½ a.m. Complains of a good deal of pain in his abdomen, but says he feels quite strong. The countenance improved, eyes less sunk, and he converses more freely; pulse 98 and fuller; the coldness has been succeeded by a sensation of warmth. No evacuations.

℞ *Opii*, gr. ij;
Succhari, gr. v. *M. ft. pulvis super. ling. imponend.*

* Half past two a. m. Has since vomited slightly, and has probably ejected the opiate.

Five a.m. Pulse perceptible only occasionally. No vomiting, but two dark evacuations have passed involuntarily. Has been delirious and very restless; is universally cold. *Repet. injectio.*

The tube was introduced into the same vein, but the injection was discontinued after the introduction of twelve ounces, in consequence of a slight extravasation of the fluid into the cellular substance surrounding the vein, and produced, as was supposed, by the laceration of the vessel by the point of the tube within the vein, and the friends not permitting us to expose another vein. This small quantity benefited him, but his friends observing him at seven a. m. to be almost insensible and sinking, allowed us to open a vein in the opposite arm. The pulse about 120, in other respects he was as bad as ever. The fluid injected consisted of the proportion of salts already specified, to sixty ounces of warm water. Sixty-four ounces, temp. F. 110°, were now slowly and carefully injected, the patient expressing no uneasiness during the process, except on one occasion, when in consequence of an accident which befel the thermometer, and broke it, the fluid was injected a little too warm. The pulse fell to 90, and was full. Without enumerating symptoms, suffice it to say, he was improved, and felt so.

[About half past nine a.m., I visited the case. I found him warm, and in a profuse perspiration. The countenance pale, the eye dull, languid, and suffused; subdelirium; pulse very languid, but still perceptible; temperature nearly natural. *R.V.*]

Half past ten a. m. Has been delirious and very restless; at present he is almost insensible, lies supine, throwing his arms about. When loudly questioned, says he feels quite strong; has no pain, but puts his hand to his head. The former dusky lividity of the face and lips has yielded to a bright-red hue. Eyes half closed and suffused; conjunctiva injected, red; pulse varying from 100 to 125, and weak. No evacuations of any kind; trunk and arms warm. Is in a profuse clammy sweat.

Vin. pro re natâ. Hirud. no. vj *fronti.*

Noon. Pulse less and insensible.

Half-past two p. m. On entering the room found his relations weeping, and himself pulseless, and apparently moribund. As a *dernier ressort* we injected fifty-one ounces, composed as last, into the left basilic, with marked benefit. His pulse returned, he replied to questions, took drink, and after some time there was a perceptible difference in the temperature of the skin. Brandy p. r. n.

We returned at half-past seven p.m., and found him again pulseless, &c., but with more blueness than ever, indeed as marked as in any case which I have seen for some time past. I was again induced to inject; not with any expectation of success, but I felt justified by the circumstance of his having been so frequently relieved by it, and from the belief of myself and Mr. Bennett, who assisted me and watched the case throughout, that he was indebted for the last eighteen hours of his existence to the injections alone.

The fluid composed as at the first was introduced at the left median basilic. At first he began to speak, and resisted, but when about forty ounces were thrown in, his respiration became suddenly most laboured. The operation was discontinued, and within ten minutes he died.

Remarks.—The only explanation of the change produced during the operation, if it depended on it at all, is, that, emboldened by former events, the fluid was propelled onwards too rapidly. This closes Messrs. Little and Bennett's account.

Post-mortem Examination.

Drawn up by R.V.—About twenty minutes past nine Mr. Luff and myself called, and observed the following external appearances:—The face pale, with a collapsed and general shrunken appearance. The bulk greatly reduced; the eyes deeply sunk in their sockets. Leave was not obtained to open the body till Sunday evening at seven o'clock. We found the general appearance much altered from that of the preceding evening. There was more colour and general plumpness. There was much discoloration—greenish black about the face, neck, and the anterior upper third of the sternum, extending along the clavicles, over the scapulæ, and down the back. The fingers firmly incurvated, and livid. On incising the abdominal cellular covering, the cadaverous fœtor was infinitely more perceptible than in any of the other bodies which I have examined after death from cholera. The cellular tissues and muscles contained air, and the latter had lost their tonicity, and had some slight degree of crepitus. They presented a dark-livid colour. There were firm and extensive peritoneo-intestinal adhesions, and the cadaverous fœtor on incising the abdominal cavity became remarkably sensible. On pushing back the sternum, the lungs were considered to be darker looking than natural; the pericardium remarkably so, with intense lividity of the muscular structure of the heart. The heart hypertrophied and elastic, apparently from air in its cavities. Crepitation. The substance rigid and firm, and on a superficial transverse incision of its substance there was no hemorrhage as I have observed in almost every other case. On puncturing the right auricle, air in quantity escaped. The blood within thick and viscid. In the left ventricle and aorta the blood was dark, firm, and clotted with a kind of fibrinous layer upon one of the clots. On incising the ventricles, gas or air escaped from all the cavities. The liver was very dark and livid, and the gall-bladder distended with bile. There were very extensive omento-intestino-peritoneal adhesions.

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* The remainder of the case to the dissection, is in the words of Messrs. Little and Bennett, who kindly sat up all night with this patient.

The intestines throughout presented a dark-livid appearance. The mucous membranes in some parts dark, in others nearly natural. There were no fæces, but there was a considerable proportion of a dark gruelly-looking mucus similar to what passed by vomiting and purging. The spleen crepitous, and, on incision, bubbles of air or gas, with a frothy-looking fluid, oozed out from the incised surface. There were several fibro-cartilaginous spots on its edges.

The left kidney was invested by a serous-looking tumour; its substance extremely dark. The same observation applies to the right.

The bladder was flaccid, and contained about a table-spoonful of muco-purulent-looking urine, possessing, sensibly, the urinous odour. The brain was not examined.

CASE II.

Sunday evening, June 17, immediately after the above dissection, we visited — Stutter, a woman aged 43,[*] who had been taken ill in the morning with symptoms of cholera—viz. vomiting and purging of a limpid, watery-looking fluid. We found her cold, and nearly pulseless. The tongue furred and rather dry.[†] She was perfectly well on Saturday evening, but about seven o'clock on Sunday morning she was seized with decided symptoms of cholera. We proposed the saline injection to the friends, and they agreed; but having to send some distance for the injecting apparatus, the operation was not commenced till within a few minutes of ten p.m.

In this case the respirations were 21 in the minute. The pulse in the temporal artery, 160;[‡] sublingual temporal, 93° F.; axillary, 86; palmar, 80.[§]

Sixty-four ounces of the saline solution (without the chlorate of potass) at the temperature of 115° Fah. were slowly and cautiously injected into the cephalic of the left arm. After the injection of the first pint, the pulse was between 124 and 130; second pint, 124; third pint, 124 and 125; fourth pint, 120 and 123.

The syringe was now withdrawn, and as there were some slight rigors, three or four tea-spoonsful of brandy were administered, and directions given for the occasional administration of a little warm brandy-and-water by a tea-spoonful at a time. In the morning, about half-past one, we visited her and found her sleeping, with a degree of stertorous breathing, the natural temperature of the body restored, or perhaps somewhat elevated. Respirations 26 in the minute, the pulsations of the carotid 148. She died at two o'clock a.m.

Dissection.—There was nothing remarkable in the external appearances differing from others who have died of the same disease. The abdomen was rather tumid, and there was discoloration about the back. There was a large deposition of adipose substance. On opening the thorax the lungs appeared dark and congested. The heart contained air in both cavities, and in its muscular substance. The blood in the same condition as the preceding case. The liver rather dark, gall-bladder distended, and on incision of the substance of the liver, a spumous fluid oozed out. The same with the spleen. The stomach and intestines very much distended with air. The mucous lining natural. No fæces, but a quantity of gruelly-looking matter similar to what passed by vomiting and purging. The kidneys enlarged and dark-coloured. The bladder flaccid but empty, with the exception of about half a drachm of mucus, which exhaled no urinous odour.

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[*] Her appearance however would have led us to infer 55 or 60. Her friends stated that she was very much altered since the attack.

[†] This is unusual.

[‡] It was not sufficiently sensible in the wrist to be counted.

[§] The thermometer in this case was not so perfect a one as in the last.

P.S. Mr. Little has since injected three other cases with marked temporary, but no permanent, benefit. One case also has been unsuccessfully injected at the White House, Bethnal Green.

NEW TESTS

FOR

OXALIC ACID, TARTAR EMETIC, OPIUM, THE ERGOT OF RYE, &c.

By John S. Hiley, Esq., *Elland*.

Amidst the many improvements that have been made from time to time in toxicology, there is yet room for more. No test of the presence of any poison is so contemptible, but it may be of some advantage, either in one case or another, and consequently the more means we can discover of making out any particular poison in a court of justice, the better are we prepared to meet such exigencies. There is no poison, whether of the mineral or vegetable world, with which we are so well acquainted as to be able to distinguish it in any mixture. They are all open to deception, and though we may have it in our power to discover them in one solution, yet it is very doubtful whether we can find them in another. The changes which take place in the stomach are constantly acting on the drug, and thus rendering it incapable of being affected by those tests which we employ in its pure state, and in many instances the least variation in point of mixture, as using spring water instead of distilled, or adding too much or too little of any menstruum, is sufficient to destroy, almost in every point, the customary changes which each re-agent is expected to produce.

Such being the case, I consider it highly necessary that toxicologists should be possessed of as many means of discovering poisons as can possibly be procured, and with that idea I send you the results of a few experiments which I have lately made on Oxalic Acid, Tartar Emetic, Tincture of Opium, and the Tincture of Ergot of Rye, which I hope may be of use to your readers, and thus afford some assistance where assistance is most required. It may be said, that after what Dr. Christison has written, there is no necessity for more being advanced; but I would add, that Dr. Christison has by no means exhausted the subject, and that as long as any addition can be made, that addition is acceptable. Cases of poisoning are every day becoming more common, and therefore the law demands that every assistance shall be rendered by means of which it may be discovered how the diseased came to so untimely an end, and thus satisfy at once the anxiety of troubled relatives, and the call of a clamorous public. In order to make the subject more concise, I will confine my remarks on each poison under one short chapter, and in the first place will notice the

OXALIC ACID.

This poison is one of the few to which suicides generally have recourse, and which has sometimes been taken by mistake for the sulphate of magnesia. It may however be readily distinguished by the crackling which it causes on its solution in water, or by the formation of the oxalate of lime, the oxalate of copper, or the oxalate of silver. These have been examined by Dr. Christison, and, as they have been found of the greatest value, have maintained their rank in the opinions of medical men. In your excellent Review, Mr. Editor, of Dr. Christison's paper on this poison, you have noticed a very beautiful property which caustic ammonia has upon oxalic acid. Allow me to say a word or two respecting it. You observe that oxalic acid possesses the property "of being precipitated in singularly beautiful stellated crystals by caustic ammonia; if, therefore, as often occurs in practice, a minute crystal, say the fiftieth part of a grain in weight, be found adhering to a paper, or accidentally scattered on the table where the admixture was made, by dissolving this in a drop of distilled water on a watch crystal, testing it with a particle of litmus paper, and adding a drop of strong caustic ammonia, a beautiful radiated star is immediately produced." Now, Mr. Editor, I do not wish to appear impertinent; I am not one of those who make it their study to find fault with the works of other people, or to cavil with them on the detection of some slight error which, strictly speaking, is of so little consequence, that it may be safely overlooked; but really I think that this test, so excellent and so serviceable as it promises to be, ought to be understood as to its effects in every point, and as such I have taken it upon me to add something to what you have already said. In noticing it then it may be worthy of remark, that the particle of oxalic acid ought not to be entirely dissolved, as the star is not then produced, but two or three atoms should be left remaining at the bottom of the fluid, and the stars, in about one minute after the ammonia is added, will begin to make their appearance, shooting out as it were from the body of the particle. You say that the oxalic acid ought to be dissolved, if so, you must either have made some mistake, or your caustic ammonia and mine differ in point of strength. I can hardly think that the deficiency is on my side, as the ammonia with which I tried the experiment was fresh, and certainly as strong as any that I had ever before made use of, if not stronger; besides that, I purchased small quantities of this article at different shops as strong as I could procure, and repeated the experiment with each, but the effect was the same. The experimentalist ought not to confine himself to one drop of ammonia, as two or three drops will often produce the effect much more sensibly. When what I have thus advanced is borne in mind, I can confidently say, that the test is a most excellent one, but if you in the least doubt my criticism, you will oblige me by repeating the experiment. I will now proceed to my own observations. I have often noticed that the interposition of a third agent is sometimes of the most essential service in toxicology. This idea I took from Mr. Dodgson's test for arsenic, and accordingly I have followed it up with some effect, as you will presently see. The least worthy of the two tests for oxalic acid which I am about to lay before you is the following:— Pour about two drachms of a moderately strong solution of oxalic acid, say three or four grains of the salt to half an ounce of distilled water, into a test glass, or tube; the first is perhaps the most suitable, then add to it nearly the same quantity of the solution of bichromate of potassa of the same strength, and afterwards let fall into the mixture a few drops of rectified spirit. A fine light port-wine-coloured ring immediately forms on the surface, or a little below, gradually spreading downwards; but if, instead of waiting this change, the liquid be shaken, the colour is lost for a few seconds, and then the whole begins to regain its former shade. The time that elapses during the accomplishment of this change does not exceed half a minute. Less quantities of both the poison and tests will answer equally well, as for instance, from one drop to ten of each, though the stronger the solution be, the more sensible will be the change, and when placed on a dirty cloth, or the experiment is performed on a dull day, then the effect is very perceptible. In some cases the colour, after standing a short time, is not so dark as at first, yet on keeping the mixture a day or two it becomes exceedingly dark. There is another way of trying this experiment, where the colour, instead of being that of port-wine, is a beautiful lilac, and from which a rather singular smell arises, somewhat similar to that of ink, but much stronger. This happens when the solution of bichromate of potassa, holding about eight grains of the salt to an ounce of water, is in the quantity of about half a drachm to a drachm and a half of the oxalic acid solution of the like strength, and the rectified spirit in the same proportion (ten drops) as in the first experiment. The mixture is shaken, and the port-wine shade appears as before, but after standing a few minutes it changes to a dun lilac. It may be well to remark, that if the bichromate of potassa be more plentiful than is laid down above, the first colour remains unchanged. The next test for oxalic acid is the solution of bichromate of potassa, and the solution of the hydriodate of zinc. This is a very excellent test, and one which, as far as my own observations go, can be depended upon with safety in cases of medical jurisprudence. What effect it might have upon oxalic acid when mixed with the matter drawn from the stomach I cannot pretend to say, but certainly in other respects it is deserving of every attention. The mode of trying it is the same as in the last experiment, except that instead of making use of the rectified spirit, a few drops of a solution of the hydriodate of zinc must be added to the mixture. This done, the liquid, from being a fine yellow, becomes of a deep brandy colour, growing instantly darker, and then letting fall a black heavy precipitate, leaving the supernatant liquor a second time of a brandy shade. The sediment adheres all down the sides of the glass, and as it subsides gives off a strong smell of iodine. Before we conclude this head, it may be advisable to add, that when equal proportions of the solutions of oxalic acid and bichromate of potassa are mixed together, the liquid, being set aside for an hour or two, changes from a fine yellow to a permanent brandy colour, not unlike that developed from the same mixture by alcohol.

TARTAR EMETIC.

I have examined the various works containing any account of this salt, but in none of them can I find any mention made of the property which the bichromate of potassa has upon it. Whether writers thought it hardly worth noticing I cannot say, but in my humble opinion it ought by no means to be omitted, as it may be numbered with the other tests for tartar emetic with the greatest propriety. Pour a small portion of a moderately strong solution of tartar emetic into a test glass, and add to it a few drops of a solution of bichromate of potassa of the like strength. The mixture instantly assumes a singular dun colour, as though a precipitate was falling, but after standing a minute or two it assumes a beautifully fine green without any sediment. Under certain circumstances it is longer, or does not change at all, that is, when too much of the bichromate of potassa has been made use of. The green has a finer appearance when the test is added in smaller proportions, but the first colour is not so sensible. Emetic tartar possesses another property which I think has not been noticed, that is, in instantly robbing the tincture and the watery solutions of iodine of their colour; add to this, it also disperses the dark blue produced by adding starch to iodine.

OPIUM.

As there has always been some difficulty in testing vegetable poisons, it is necessary that as much time be employed in examining into their properties as possible, that by so doing, the veil that encircles them may be drawn aside, and they may thus appear before us as well understood as the mineral poisons. Amongst a number of experiments which I have lately instituted on the tinctura opii, I have stumbled upon two or three re-agents, which I conceive may be of some benefit to toxicologists; consequently I do not delay to lay them before you. The first and most sensible is the bichromate of potassa. This very useful test, when added in small quantities to highly-diluted laudanum, lets fall a very copious light-yellow precipitate, which is soluble in muriatic acid, leaving the liquid of a dark-brandy colour. When caustic potassa is added to diluted tincture of opium, the tincture is decomposed, and lets fall a rather plentiful dirty-white precipitate; but if a little of the mixture is poured into a test tube, and then applied to the flame of a spirit lamp, this precipitate after a short time begins to run into flakes of a dark colour, and as the liquid becomes hotter they disappear. The mixture is then fine and clear, but as it cools, a part of the precipitate is again thrown down. This is, in my opinion, an excellent test; the tincture may be diluted to almost any extent, till it s nearly colourless; but still the change is ffected, except that the flakes are not so perceptible. In conjunction with Mr. J. T. Cooper's test, the muriate of gold and a few rops of caustic potash, it will prove, I may onfidently say, of essential service, as it trengthens his re-agent, and places the oison in question almost beyond a doubt. ut few of the tinctures undergo decom-osition, when cold, with caustic potash, and though the Pharmacopœia mentions this change with opium, yet it makes no allusion to its running into flakes, and disappearing when heated, or re-appearing again when cold. On this property I bring it forwards as a test for laudanum, and I do not doubt but my assertions, on examination, will be found established on firm grounds.

ERGOT OF RYE.

Though the ergot of rye, as far as I can learn, has never yet been made use of as a poison, yet that is no argument but it may be applied to that purpose, either intentionally or unintentionally. That it possesses poisonous properties cannot be doubted, when we have once beheld the remarkable effects which a very small quantity of it has in exciting the action of the uterus, as such therefore it is only a mark of prudence if we make every discovery in our power of the various tests and re-agents which it may possess, and thus prepare ourselves for any evil that may occur. The ergot of rye is but of late introduction into practice; consequently it is yet imperfectly known; but we have every reason to suppose, that as it becomes more common, recourse will oftener be had to it as a means of committing suicide. Opiates formerly were never used as poisons, because the world was almost unacquainted with them; but now, when they are known to every one, they are as often used for destroying life as any other drug. Such being the case, is there not every likelihood that the ergot of rye, at some future period, will be taken with the same design, and thus require the means of discovering what it really is before a court of justice? I will now proceed to examine it. Caustic potash has a very singular effect upon the tinct. secal. cornut. The following are the items:—to about half a drachm of water add two or three drops of the tincture of the ergot of rye, then let fall into the mixture three or four drops of caustic potassa, and it instantly assumes the colour of cape wine, lighter or darker, according as there is more or less of the tincture made use of. Apply this, in a test-tube to the flame of a spirit-lamp, and after it has boiled for about half a minute or less, nay, sometimes before it has reached the boiling point, reddish woolly flakes, according to the colour of the liquid, not unlike the detonating cyanate of silver, begin to make their appearance, moving about the fluid, and as it cools falling to the bottom. The stronger the mixture, the more flakes are produced; and the deeper the colour, the redder the precipitate. This is a very delicate test, and however highly the tincture may be diluted, providing enough of the alkali is added, the flakes are very perceptible. But the effects of this test do not entirely rest here. The change which the potash produces on the diluted tincture, in turning it from a nearly colourless liquid to the clear shade of the best cape wine, is not of the least consequence, and though it alters the colour of almost all tinctures more or less, yet none of them are so powerfully affected as this; add to this, heat strikes it still deeper. Nitro-muriate of gold, after standing for some time, throws down a cloudy yellow precipitate from the tinct. secal. cornut.

VARIOUS POISONOUS TINCTURES.

It may be well to notice what effect the two tests just mentioned have upon a few of the other poisonous tinctures, and we shall then be better able to judge of their value over any single one. The tinctures of hemlock and henbane are precipitated, on boiling with caustic potash, in the same manner as the ergot of rye, and there would be some difficulty in distinguishing the precipitate belonging to the latter from that of the ergot, but for the assistanee rendered by the muriate of gold. With this test both tinctures are precipitated; but that of the henbane is much longer in falling, and is perhaps not quite so plentiful. The hemlock is still more easily distinguished. The liquid, when the potash is added, has a greenish-yellow appearance, as have also the flakes, whereas the ergot-of-rye solution puts on a fine cape-wine shade, and the precipitate a reddish one. With the muriate of gold the tincture of hemlock is made turbid. None of the other tinctures are affected when boiled with potash, but the gold test has a rather singular effect upon most of them. From the tincture of digitalis, diluted till nearly colourless, it throws down a rather copious green precipitate, not unlike the powdered leaves of that plant, which being set aside for about a day, becomes of a dark-brick colour; from the tincture of hops a sediment is let fall, somewhat similar to that of the ergot of rye; and in the tincture of black hellebore, after standing a length of time, it causes a muddiness, and afterwards a purple precipitate is produced, which adheres to the sides of the glass. These three last poisons, however, are by no means affected when boiled with a few drops of cautic potash, consequently if their precipitates resembled in every point that of the ergot of rye, this difference would be enough to distinguish them. It would seem, from the above observations, that the vegetable poisons are not altogether unworthy of further research; and were it not that the limits of my paper are already extended much further than I at first designed, I would devote a page or two more to the subject.

Elland, near Halifax, June 19, 1832.

ESSAYS

ON

ANIMAL ACTION.

By William Dobson, *Esq., Surgeon, Pimlico.*

No. VI.

RESPIRATION.

It has been considered that the motions of the abdominal muscles are active in breathing; that it is by their agency that the diaphragm regains its position in the chest. Sir C. Bell asserts, that "when air is admitted into the lungs, that air is again expelled by the reaction of the abdominal muscles. These muscles compress the viscera, and by pushing them up, raise the relaxed diaphragm, preparing it for another effort of inspiration." And he goes on to say, "Is it not obvious, that if the power of the abdominal muscles be lost, the respiration must stop?" No: if the abdominal muscles of a dog be divided transversely so as to expose the diaphragm, we perceive that this organ descends into the abdomen, and ascends into the chest, as efficiently as if the abdominal parietes had remained entire. These phenomena I have frequently witnessed when dividing the par vaga below the diaphragm.

This is very cogent circumstantial evidence. It proves, that without the assistance of the abdominal muscles expiration or the return of the diaphragm into the thorax *can* be accomplished. And does it not tend to prove the passiveness of those muscles in ordinary expiration?

Another argument more powerful than the last is, that the abdominal muscles are supplied by nerves of the *voluntary class*—by the dorsal and lumbar which imbue other muscles in which they ramify with the voluntary characters *solely*. Therefore on these data, I conceive, we may boldly and fearlessly deny, that the abdominal muscles *do* or *can* maintain a perpetuity of action, that they are involuntary organs. I consider them as mere passive agents in breathing, being thrust forward during inspiration by the descent of the diaphragm throughout the medium of the intervening viscera, and returning *passively* from the ascent or relaxation of the diaphragm.

The abdominal muscles exert a considerable influence over the movements of the chest. But in what manner do they exert this influence? They check respiration, and stop it, in obedience to the will; the experiment every one can perform. In holding of the breath, they prevent the expansion of the base of the chest; for this operation is distinctly a muscular effort; and if the abdominal muscles be not the sole agents in this act, they are the principal ones.

When respiration is restrained for even a few seconds, a most uneasy sensation is experienced in the chest, and the seat of this anxiety is referable to the diaphragm. It appears to arise from an instinctive and irresistible desire of the diaphragm to resume its contraction or to return to a passive state. It thus counterbalances the restraining agents; the pressure of the muscles on the exterior of the chest.

No sooner does the fœtus emerge from its aqueous abode into an aërial atmosphere, than it begins to breathe.

During the fœtal period, it is presumed that no animal or visible action is performed; that none but the organic functions exist,—such as circulation, nutrition, growth, &c. But when the child is born it is brought into relation with the external world, and is subjected to its moral and physical influences; it is cut off from its mother, and has to subsist as an independent creature. A number of operations arise into existence, such as respiration and the voluntary movements, the external senses, and the various secretion.

Our next considerations, then, will be to ascertain, 1st. Why respiration commences instantly after birth. By what agency is the action induced? 2nd, And where does the consciousness of necessity reside? 3rd. The imaginative Buffon conceived that man might be made an amphibious creature. He commenced the elucidation on "puppies;" but experiment soon determined the fallacy of the notion. He decided that the life of quadrumanous animals could not be preserved without the inhalation of air. And it is incontestably established, that a regular supply of atmospheric air is essential to the existence of every animate being. Even the plant which is fixed to the surface of the earth, and the fish which dives in the ocean, and every animal that roams on the surface of the globe, all demand a constant supply of atmospheric air. But the means by which air is received and appropriated to the purposes of life, vary, as we rise or fall, in the scale of vital existence. Vegetables, and some of the lower orders of the animal creation, absorb the air through their external porous surfaces without any recognisable action or destined apparatus. The fish species imbibes the air into their branchiæ or gills, many of them without any active effort; but terrestrial animals and the amphibiæ possess an active respiratory apparatus. They have lungs enclosed in an elastic case, which alternately expands and contracts to admit the air, and expel it when its virtues are exhausted. The agent which excites these motions is the diaphragm, and this organ is endowed with a power of involuntary action.

Breathing must commence soon after birth, or death will ensue. Then "where does the consciousness of necessity reside?" Not in the blood, we presume, nor yet in the lungs; but I believe *in the diaphragm*? In the diaphragm, like the heart, there is a tendency to incessant motion during the life of the animal. But prior to the natal period, the heart exists in perpetual activity, whereas the diaphragm is motionless. Nature so economises her machines, that whenever there is no necessity for the actions of an organ she never expends it in vain. The heart appears to move and circulate the fluids from the earliest period of embryotic existence until the extinction of life. But respiration needs not to exist before birth. The mother supplies the embryo, or the fœtus, with blood, and in a fit condition for its growth and vitality. But when the materno-fœtal separation occurs, the child has to purify blood for itself. Being unconscious that it has to breathe there can be no desire to perform the operation, but so wisely and so designedly is the mechanism of nature constructed, that each individual part seems to know its office, and fulfils it without any compulsatory effort. For as soon as the fœtus has burst through the trammels of its uterine abode, the diaphragm bounds into motion with alacrity and vigour, and sometimes for a century continues this motion. Why did not the diaphragm play before birth? We have shown that the action is unnecessary. This ought to be sufficient to satisfy even the most sceptical mind. From deliberate and intense inquiry I am disposed to consider respiration as more a mechanical act than physiologists are accustomed to do; and that not only the performance and continuance of this function derive their influence from a mechanical agency, but that the very first motion derives its existence from a similar cause.

How slight a pressure on the thoracic parietes will arrest the heaving of the chest, even in the adult! How essential to the free motions of the chest that it should be immersed in an aërial medium; and, further, that the density of this medium should be of an invariable and definite character! When an animal is completely immersed in water, the diaphragm cannot expand the chest; not arising, however, it is presumed, from the surrounding pressure, but from deprivation of atmospheric air. But when only the chest is immersed in an aqueous medium, breathing is laborious: the "bather" feels the necessity of exerting his voluntary muscles to expand the chest, the power of the diaphragm being insufficient. When the fœtus is in the matrix, it is contained in a resisting medium, and surrounded by textures of such dense characters, as to be incompatible with motion of the fœtal chest. But when the child has emerged from the uterus, the thorax is relieved from the pressure which it had previously sustained. Life is evinced by motion of the limbs, and by an effort to breathe. But sometimes this effort is so feeble as not to be perceptible until a few seconds have elapsed, and frequently respiration is not fully established for a considerable time. Occasionally, however, the very first respiratory effort is loud and powerful. I conceive that there is a disposition in the diaphragm of the fœtus to contract, and thus expand the chest, but that this contraction is prohibited by the circumambient resistance, which no longer exists when the fœtus is expelled.

STRUCTURE OF THE

HUMAN PLACENTA,

AND ITS

CONNEXION WITH THE UTERUS.

(*Analysis of a Paper by Dr.* ROBERT LEE, *lately read before the Royal Society.*)

IN the year 1780 John Hunter presented a paper to the Royal Society in which he claimed the discovery of the true structure of the placenta, and of its communication with the vessels of the uterus. His conclusions were drawn from the dissection of a woman who died undelivered, near the full term of utero-gestation. The veins and arteries of the uterus were injected, and an incision made through its parietes at the part to which the placenta was attached. Between the uterus and placenta lay an irregular mass of injected matter; from this mass regular pieces of the wax passed obliquely between it and the uterus, which broke off, leaving part attached to that mass. On attentively examining the portions towards the uterus, they plainly appeared to be a continuation of the veins passing from it to this substance, which proved to be the placenta. Other vessels, about the size of a crow-quill, were seen passing in the same manner, but not so obliquely. These, when examined, were discovered to be continuations of the arteries of the uterus. The veins were next traced into the substance of what seemed to be placenta; but they soon lost the regularity of vessels, by terminating at once upon the surface of the placenta, in a very fine spongy substance, the interstices of which were filled with yellow injected matter. On tracing the arteries he found that, having made a close spiral turn upon themselves, they were lost on the surface of the placenta. On cutting into the placenta, he discovered in many places of its substance, yellow injection, in others red, &c.; in many others there were two colours mixed.

From these appearances Mr. Hunter inferred, "that the arteries which are not immediately employed in conveying nourishment to the uterus go on toward the placenta, and proceeding obliquely between it and the uterus, pass through the decidua without ramifying. Just before entering the placenta, after making two or three close spiral turns upon themselves, they open at once into its spongy substance, without any diminution of size, and without passing behind the surface as above described."

Dr. W. Hunter's description of the vascular connexion between the uterus and placenta, coincides with that of his brother, "for it seems incontestable," he observes, "that the human placenta, like that of the quadruped, is composed of two distinct parts; viz. an umbilical and a uterine; that each of these parts has its peculiar system of arteries and veins; and that the circulation through these two parts of the placenta differs in the following manner:—in the umbilical portion the arteries terminate in the veins by a continuity of canals, whereas in the uterine portion there are intermediate cells into which the arteries terminate, and from which the veins begin.

Noortwych, Rœderer, and Haller, had failed to determine in a satisfactory manner, the existence of a vascular connexion between the uterus and the cells of the placenta; and the opinions of the Hunters have been received by all anatomists to the present day. In the present paper Dr. Lee describes certain appearances, observed in the dissection of six gravid uteri, and many placentæ expelled in natural labour, which seem to demonstrate, that a cellular structure does not exist in the placenta, and that there is no connexion between this organ and the uterus, by great arteries and veins.

When an incision is made through the gravid uterus where the placenta does not adhere, numerous minute blood-vessels and fibres will be observed passing from the inner membrane of the uterus to the decidua. At the circumference of the placenta, the membrana decidua separates from the other membranes to pass between the uterus and placenta, and thus forms a complete septum interposed between these organs. The chorion and amnion cover the fœtal surface of the placenta, and between these two membranes and the decidua lie the ramifications of the umbilical vein and arteries, subdivided to an extent almost indefinite, and connected together by white slender filaments running in various directions. Such is the internal structure of the placenta, and it adheres to the fundus or some part of the uterus by innumerable flacculent fibres and vessels. With the fibres uniting the placental decidua to the uterus are mingled numerous small blood-vessels, proceeding from the inner membrane of the uterus to the decidua, and these vessels exist universally throughout the whole extent of the membrane. There is no vestige of the passage of any great blood-vessel, either artery or vein, through the intervening decidua, from the uterus to the placenta, nor has the appearance of the orifice of any vessel been discovered, even with the help of a magnifier on the uterine surface of the placenta; no cells are discernible in the structure by the minutest examination. At that part of the uterine surface to which the placenta has been adherent, there are observable a great number of openings, leading obliquely through the inner membrane of the uterus, and large enough to admit the point of the little finger. Over these openings in the inner membrane of the uterus, the placenta covered by deciduous membrane, is directly applied, and covers them in such a manner, that the maternal blood, as it flows in the uterine sinuses, cannot possibly escape, either into the cavity of the uterus, or into the substance of the placenta.

When air is thrown forcibly into the spermatic arteries or veins, the whole inner membrane of the uterus is raised by it; but none of the air passes across the deciduous membrane into the placenta, nor does it escape from the semilunar openings in the inner membrane of the uterus, until the attachment of the deciduous membrane to the uterus is destroyed. There are no openings in the deciduous membrane, corresponding to the valvular apertures now described in the inner membrane of the uterus.

If a placenta, recently separated without any artificial force, be examined, the surface will be found uniformly smooth, and covered with the deciduous membrane, which could not have been the case, did any large vessels connect it with the uterus.

The placenta in a great majority of cases is detached, after labour, with the least imaginable force, which would be impossible, if a union by large vessels, possessing the ordinary strength of arteries and veins, actually existed.

Haller, the Hunters, and Monro, do not seem to have examined the gravid uterus or its contents in the natural state, but after fluids had been forcibly thrown into the hypogastric and spermatic arteries. This artificial process was followed by a laceration of the deciduous membrane, covering the orifices of the uterine sinuses, and deposits of injection were formed in the substance of the placenta, giving rise to the deceptive appearance of cells. That such was the fact is rendered more than probable by the result of examinations made by Dr. Nimmo and Mr. Broughton, at Dr. Lee's request, of the preparations of the gravid uterus in the Hunterian museum at Glasgow. In many of them the deposits of injection, causing the appearance of cells, were evidently the result of extravasation.

No preparation in the collection seems to have been expressly made for the purpose of proving or disproving the fact, that the decidua passes over the uterine surface of the placenta; but in reference to the preparation R. R., No. 139, Dr. Nimmo remarks, that no vascular openings are visible in the membrane interposed between the uterus and placenta.

Dr. Lee examined a preparation of the gravid uterus in the museum of the Royal College of Surgeons of London, supposed to have been put up by John Hunter nearly fifty years ago. The deciduous membrane was everywhere covered with minute tortous blood-vessels, proceeding from the inner surface of the uterus, and filled with injection. There was no appearance of vessels of any size passing between the inner surface of the uterus and placenta; but flattened portions of the injection were observed in this situation, having in many parts the form of thin layers, which had obviously escaped from the orifices of the uterine veins. Elsewhere the injection had lacerated the deciduous membrane, and formed deposits in the vascular part of the placenta.

From these facts Dr. Lee concludes, that the human placenta does not consist of two parts, maternal and fœtal; that no cells exist in its substance; and that there is no connexion between the uterus and placenta by large arteries and veins. The deciduous membrane being interposed between the umbilical vessels and the uterus, whatever changes take place in the fœtal blood, must result from the indirect exposure of this fluid as it circulates through the placenta, to the maternal blood flowing in the great uterine sinuses.

COURT OF COMMON PLEAS,

WESTMINSTER, MONDAY, JUNE 25, 1832.

RAMADGE VERSUS WAKLEY.

Before MR. JUSTICE BOSANQUET, *and a* SPECIAL JURY *consisting of the following Gentlemen*:—

Benjamin Adam, Esq., Lower Homerton, Hackney, merchant.
Robert Henry Close, Esq., 10, Montagu Square.
Charles Cook, Esq., 45, Doughty Street, Pancras.
Abraham Cooper, Esq., 13, New Milman Street, Pancras.
T. Eldred, Esq., Highbury Terrace, Islington, merchant.
George Hadden, Esq., 15, Highbury Terrace, Islington, merchant.
John Hawkes, Esq., Upper Holloway, merchant.
Robert Mundell, Esq., Cheyne Walk, Chelsea.
Michael Oppenheim, Esq., Mansell Street, merchant.
John Rolls, Esq., King's Road, Chelsea.
William Thickbroom, Esq., Eaton Square, Hanover Square.
John Tracy, Esq., 10, Brudenell Place, merchant.

Counsel for the plaintiff Mr. Serjeant WILDE, Mr. Serjeant SPANKIE, Mr. ADOLPHUS, and Mr. WHITE.

Counsel for the defendant, Mr. Serjeant JONES and Mr. KELLY; but as Mr. WAKLEY addressed the jury and cross-examined the witnesses on his own behalf, both these gentlemen were prevented, by the etiquette observed at the bar, from rendering him any assistance openly in court.

Attorney for the plaintiff, Mr. WILLIAMS, Ely Place. Attorney for the defendant, Mr. LOFTY, King Street.

Plea, the *general issue—Not Guilty*.

In consequence of Mr. Wakley's absence, he having been unexpectedly detained in the country, the court was so obliging as to wait half an hour, but not having arrived at that time. Mr. Serjeant Wilde intimated his right to commence, and the Judge having assented, the learned gentleman opened the case in a very candid speech to the jury. Mr. Wakley entered the court exactly as the learned serjeant concluded his address.

Mr. Serjeant WILDE then called Dr. Hue to prove that the plaintiff was a fellow of the Royal College of Physicians. Mr. Wakley having declined to cross-examine the witness, Mr. Bradford, a surgeon and apothecary, residing in Fleet Street, was called on behalf of the plaintiff, and having been subjected to a long cross-examination, the alleged libel, originally published in the 419th Number of THE LANCET, was put in, and read by the officer of the court. It was substantially the same as in the following cause, but in the form of a letter, addressed To the Editor of the Lancet, and signed "One."

Mr. Serjeant WILDE then said—"That is my case, my Lord."

Mr. WAKLEY now rose, and expressed much astonishment at the learned serjeant's declaration that that was his case, inasmuch as the averments for the major part remained unproved.

The Judge, however, having decided that sufficient had been proved for the case to go to the jury, Mr. Wakley addressed the jury on the facts as they had been elicited in the evidence, and commented with much severity on the plaintiff's treatment of Miss Bullock, and on his unjustifiable advocacy of John Long, the individual found guilty on a trial at the Old Bailey, of the manslaughter of Miss Cashin.

Having concluded his address, the gentlemen of the jury turned round in the box to deliberate, and after a few minutes one of them addressed the Judge with a view of ascertaining if a verdict of "one shilling" would entitle the plaintiff to costs, and having been informed that it would, they again deliberated for a few minutes, and returned a verdict for the plaintiff—

"DAMAGES, ONE FARTHING."

RAMADGE *versus* RYAN AND OTHERS.

On the following day, Tuesday, the cause of Ramadge *v.* Ryan, Renshaw, and Rush, was called on for trial before Chief Justice Tindal and a special jury.

The four counsel for the plaintiff were the same as in the foregoing cause, and Mr. Serjeant Taddy and Mr. Erle appeared for the defendants; in this case the defendants put in, in addition to the plea of the *general issue*, a plea of *justification*.

Mr. Serjeant WILDE having opened the case, and called Mr. Bradford as a witness, who gave testimony similar to that on the preceding day, the libel was put in and read, as follows:—

"TWEEDIE *v.* RAMADGE.—Dr. Ramadge was in attendance on a case of typhus; the patient (a young lady) was bled from the arm on a Friday, and eight dozen leeches (96) applied to the head and neck. On Saturday both temporal arteries were opened, —the patient fainted, and the apothecary (who was likewise in attendance) left her; the nurse brought her round with wine and water. On the Sunday, another dozen leeches were applied, and immediately she became delirious; when Dr. Tweedie's advice was requested by the relatives.

"Dr. Tweedie having spoken apart with Dr. Ramadge, addressed Mrs. Reynolds, the sister of the patient, and said, 'That having attended before, he should be happy now to give his assistance to the young lady, but that Dr. Ramadge's conduct in a late correspondence with John Long had been such, that no medical man of respectability could call him in or consult with him without injuring himself in the eyes of his brethren. That he (Dr. Tweedie) bore no private pique against Dr. Ramadge,—he believed him indeed to be clever; but his character, as respected the above transaction, rendered it imperative for all medical men to decline acting with him; and Mrs. Reynolds must choose which she would entrust. Dr. Ramadge replied in great anger, that he was a gentleman by birth, education, and profession, but that Dr. Tweedie was neither. * * * Dr. Tweedie answered him by turning coolly on his heel and walking out of the room. Dr. Tweedie was retained, and cured the patient by exactly opposite treatment. Dr. Ramadge (it is said) frequently is at supper with John Long.—*Lancet*."

"Dr. Tweedie has honourably and faithfully discharged his duty to his medical brethren; and we hope every one else will do the same. We are well aware who it is —and a medical man to boot—that makes the trio in these family suppers. Let him be warned in time. He takes upon him to defend this nefarious quack and manslaughterer in the face of the whole profession: let him take warning, or we will not spare him.—*Ed. Med. Surg. Journal*."

Mr. Serjeant Taddy then addressed the jury for the defence, and in the course of his speech observed, that he was prepared to justify the statements in the alleged libel, although his clients had offered an apology to the plaintiff. The learned serjeant reminded the court that the objectionable article was first published in The Lancet, and that it had merely been copied into the defendant's journal.

At the close of this address Mr. Serjeant Taddy called Dr. James Johnson, Mr. Brodie, and other medical gentlemen, to state their opinions respecting the conduct of Dr. Ramadge as connected with his public vindication of John Long.

Chief Justice Tindal in summing up remarked, that since a justification had been pleaded, the apology formerly tendered to the plaintiff could not now avail the defendants. The jury having retired to consult for a short time, returned with a verdict for the plaintiff,—Damages, Four Hundred Pounds.

THE LANCET.

London, Saturday, June 30, 1832.

Two causes, involving questions of alleged libel in matters relating to medical journals and medical practice, were tried in the Court of Common Pleas, on Monday and Tuesday last. The disputed articles will be found at length in other parts of The Lancet, together with the respective awards of the juries. In the case tried on Monday, Dr. Ramadge was plaintiff, and the Editor of this journal defendant—plea, the general issue—when, after a trial which occupied the Court from ten o'clock until nearly four, a verdict was given for the plaintiff, with *one farthing* damages. The defendant, having pleaded his own cause, was deprived by the etiquette which is observed in courts of law, of the powerful aid which otherwise might have been openly rendered to him by his highly eminent counsel Mr. Serjeant Jones and Mr. Kelly. The defendant, however, acknowledges, with a grateful warmth of feeling, the assistance which he obtained from the learned gentlemen by their suggestions privately communicated during the progress of the cause.

Devotedly as we are attached to the liberty of the press, we might have yielded a few of our columns to remarks connected with the trial of Monday, had we not been suddenly and unexpectedly checked by what we cannot avoid deeming the extraordinary result of the deliberations of Tuesday. Entertaining towards Dr. Ryan neither a friendly nor an unfriendly feeling, the character of the alleged libel, and the circumstances under which it was published, compel us to look upon an award of four hundred pounds as an amount of damages unusually excessive. What a contrast! *One farthing* on the Monday, and *four hundred pounds* on the Tuesday, as a compensation in damages for an injury of precisely the same extent in its primitive character! But mark! The journal against which the lowest coin in the country was given, is circulated to an extent beyond comparison greater than that publication the Editor of which is cast in half the amount of damages laid in the declaration—the injury having been estimated by the plaintiff at eight hundred pounds in both instances. Here, then, we have the effects of what *some* would designate the "glorious uncertainty of the law." But the expression receives not our assent, because the difference furnished by the instances before us is surely attributable, *not* to "the uncertainty of the law," but to some essential variations in feeling and mind possessed by the gentlemen who sat as jurymen on the two causes.

Not having been present on the Tuesday, it is not in our power to speak, from personal observation, of the demeanour of the individuals who composed the jury on that day; but with respect to the gentlemen who officiated as jurymen on Monday, their names and addresses will be found in a brief notice of the trial in another column. Holding as they do such honourable stations in society, it would be impertinent were we to comment on their claims to respectability. We cannot, however, refrain from stating that these gentlemen paid the most scrupulous attention to the evidence, the arguments, and the summing up of the learned Judge, and that, whether drawing our inference from the pertinent questions proposed by them to the witnesses, or from the verdict of "*one farthing damages*," we believe there was not an individual in court who questioned either the purity of their intentions, or the soundness of their decision. And be it observed we make no exception here of the plaintiff, his attorney, or John Long, all of whom were present. Under such circumstances, who is to understand the grounds on which four hundred pounds were awarded against Dr. RYAN and his publishers? Those who approve of the verdict given on the Monday, will of course be strongly inclined to question the propriety of the Tuesday's award, because it cannot be forgotten, in estimating the amount of damages granted in the two cases, that the prosecuted article was *first* published in THE LANCET, and copied from that work into the journal of which Dr. RYAN is editor, and Messrs. RENSHAW and RUSH—the other defendants—publishers. There is however this important distinction to be drawn between the two cases. The Editor of THE LANCET did not offer to "*apologise*" to the plaintiff, and then go into court with a plea of *justification* upon the record. Here was a discrepancy in the conduct of Dr. RYAN, which his jury could not overlook without being utterly destitute of common sense. If an apology were called for, the offer of one was handsome, generous, and consistent; but surely it could not appear just to the upright minds of twelve impartial men to persist in an attempt to *justify* statements which, by the defendant's own admission, entitled the plaintiff to an apology. The jury, believing that Dr. RYAN would not have proposed to apologize without a conviction that he was in error in making the publication in question, were necessarily compelled to consider that a perseverance in the plea of justification was little short of an act of determined persecution. The defendant in this cause either *was*, or was *not*, warranted in his offer to apologize; if the former, his plea could not appear to the Court otherwise than an unjust one; if the latter, his proposal to apologize must have seemed to originate in a desire to screen himself from the consequences of an action for libel, rather than to render justice to the plaintiff, or to protect the public from injurious practices. Although, therefore, we are disposed to look upon the award of damages against Dr. RYAN, as excessive in a marked degree, yet we must confess that he did not occupy before the Court that position which alone can be filled by an unflinching member of an upright, independent, medical press.

In founding a Hospital in connexion with the University of London, it is to be hoped that the Council will not be unmindful of one glaring omission, so strikingly apparent in all the great eleemosynary medical institutions of this metropolis —we allude to the invariable absence of a *stipendiary* resident medical officer, always at hand to afford, at once, assistance to the sick, and clinical information to the student. Will the public credit the statement that—with the exception of the Fever Hospital, where cases of surgery are not admitted—there is no such officer in this capital? There are resident chaplains, resident stewards, resident brewers, resident cooks, and resident nurses, but resident *surgeons* to afford *immediate* assistance to the wounded and bleeding victims of accidents, there are none! Of course we take not into this account the dispensing apothecaries—the mere tools of the physicians, nor the "House-Surgeons," as they are styled, who *purchase* their places by the year from the superior officers of the establishment. If the endowed hospitals were poor—if they were almost destitute of funds, there might be some apology for the governors when they omit to provide competent resident officers; but when it is recollected that the income of Guy's Hospital, arising from the hospital estates, amounts to *fifty-thousand a year*, and that the hospitals of St. Thomas's and Bartholomew's, respectively, command sums reaching about *forty thousand pounds* per annum, it is a scandalous, a wanton, a cruel neglect in the application of such princely resources, to omit securing for the suffering sick and inquiring students, medical and surgical skill commensurate with the wants of both. An interference with the claims of patronage cannot, however, be readily tolerated in these "*ancient* institutions," but in an establishment like the London University, which has been thrown into public view by the increased light of modern times, it is to be hoped that no sacrilegious influence will be permitted to prevail between the sick and their wants, and between the students and the prosecution of their intellectual pursuits. The Council must not forget that men who shine as professors in the lecture-room, are often utterly incompetent to discharge the duties devolving upon hospital practitioners. The professor's chair, therefore—merely as such—should not be recognised as the legitimate and certain stepping-stone to office in the establishment for the sick, for, should this precedent be admitted in the new institution, the Council will only add another to the large number of human slaughter-houses already existing in this metropolis.

Well instructed from a sad and painful experience in the calamities which have arisen from the system of nepotism, which has so long flourished in our hospitals, the members of the medical profession are actively alive to every projected improvement in hospital government. Accordingly we find, that the proposal of the Council to erect a medical establishment, for the avowed double purpose of affording medical aid to the sick poor, and information to the pupils in the department of medicine, has elicited the most vivid feelings of satisfaction. But before the Council exert themselves with the public in furtherance of raising the necessary funds, the public demand the *regulations* which shall govern the bestowal of the money. "This," they say, "is a condition precedent, which a well-disposed and respectable body cannot refuse to furnish," and they require "that the elections to all the offices of trust in the hospital shall be influenced solely by the intellectual claims of the candidates, and that there shall be appointed a *resident*, properly-remunerated medical officer, competent to render immediate aid in all cases that may be admitted, whether medical or surgical." The subscribers, we apprehend, will resolve that it shall not be remarked of this institution,—as of St. Thomas', Guy's, and Bartholomew's Hospitals, that the physicians and surgeons appear to reside at the greatest possible distance from them, with apparently no other object in view than that of escaping from the discharge of duties, which humanity and science alike call upon them to fulfil. Deriving little or no emolument *directly* from the funds of these charities, the medical officers seem to consider their official obligations as of very slender tenure, and to regard the students as a species of game, which they have a right to pluck with an impunity not to be questioned.

The establishment of the proposed hospital furnishes the Council of the UNIVERSITY with a splendid opportunity for effecting in hospital government an efficient and noble reform. We call upon them, therefore, to reject every species of unworthy influence, to cast aside all sinister views, if any be entertained, and to come out at once frankly and boldly with a declaration that the hospital, when erected, shall be provided with a resident medical officer, and that that officer shall only be enabled to obtain his station by means of the unalloyed influence of his merits. The effect of such a pledge would act as a charm upon the tightened purse-strings of an intelligent community, and let loose a vast fund of wealth which may be applied for ages yet to come to the noblest purposes of humanity and science.

The Anatomy of the Thymus Gland. By Sir ASTLEY COOPER, Bart., F.R.S., &c., &c.

(Concluded.)

AFTER examining the structure of this curious organ in the lower animals, the Serjeant proceeds to its inspection in the human subject. He describes it as formed of a thoracic and cervical portion on each side, the former situated in the anterior mediastinum, and the latter in the neck just above the first bone of the sternum, and beneath the sterno-laryngeal muscles. Between two and three months of fœtal life, this gland is so small as to be scarcely perceptible. At three months its increase is in proportion to the magnitude of the fœtus, and thus it continues to the seventh month, when it enlarges out of proportion to its former growth. At eight months it is large, but at the ninth month it has undergone a sudden change, and become of a great size. It increases after birth, and continues large to the first year, when it slowly disappears, and the cavities cease to exist at the time of puberty. Sir Astley has seen two varieties in the relative position of this organ; the first was the vena innominata passing through the gland; and the second the same seen placed anteriorly to the cervical lobes. He never saw two glands precisely alike in form. Sometimes they are round, at other times of great length, and some are so thin that the serpentine arrangement of their lobes may be seen without dissection.

At birth the thymus gland reaches half way down the sternum, and thence extends into the neck as far nearly as the thyroid gland. It is connected to the sternum, and the origin of the sterno-laryngeal muscles, by cellular membrane. It adheres strongly by the same medium to the pericardium; before and beside it are the internal mammary arteries and veins.

Sir Astley Cooper thus describes the thoracic fascia:—

"This fascia is wanting immediately behind the upper bone of the sternum, and, leaving a space for the passage of the thymus gland, is attached to the edge of the first rib; one portion of it passes upwards and unites itself to the coats of the jugular veins, to the surface of the trachea, and joins the deep-seated fascia of the neck described by Burns.

"The thoracic portion descends upon and

surrounds the arteria innominata, left carotid, and left subclavian arteries, and extends itself on the coats of the aorta, covering, enveloping, and supporting each of the vessels, and inseparably connecting them with the bones which form the opening of the thorax. A slighter union also subsists between the fascia, the surface of the vena innominata, and the pericardium.

"This fascia is united to the edge of the first rib as far as its head, and is not only joined to the curvature of the aorta and its vessels, but also descends upon the trachea as far as the division into the bronchi.

"Of all the parts that enter the chest, the œsophagus is the least confined by this structure; as that tube requires that its capacity should frequently change in the act of deglutition, it was necessary that it should be but loosely connected to the surrounding parts to admit of the necessary changes in its diameter. To allow of this, the fascia forms a crescent from the first rib on each side to the spine, leaving a large space before the vertebræ for the passage of the œsophagus. In other parts the upper opening of the thorax is shut by the fascia, being united to the nerves, arteries, veins, and trachea."

The thymus gland is formed of two distinct bodies, which may be properly called a right and left thymus gland. This organ is connected to the adjacent parts by an envelop of coarse cellular tissue which not only fixes it in its situation, but also unites the two glands of which it is composed, so as to require a delicate dissection to separate them.

"When this membranous covering is removed, the substance of the gland is exposed, which is found to be of the conglomerate kind, being formed of numerous lobes which are connected together by a second covering of reticular tissue uniting the lobes to each other, and combining its parts by entering minutely into its interstices.

"The lobes of this gland differ in magnitude, but not one of them appears to be larger than a pea, and they vary from that of the head of a pin to the size abovementioned. When the form of the thymus is strictly investigated, the lobes are found to be disposed in a serpentine direction around a cavity hereafter to be described.

"The gland may be unravelled, and it will be found to be composed of a rope on each side of which the right and left thymus is constituted, and on each of these the large lobes form knots, and it appears like a necklace of beads, but even these lobes may be farther separated. In order to succeed in unravelling the gland, it is necessary to divide the arteries, veins, as well as mucous membrane, because the arteries, veins, and membrane, unite the lobes to each other.

"These ropes are disposed on a spiral course around the central, or nearly central, cavity, and this disposition of them is preserved by the arteries, veins, and mucous membrane, by the division of which the ropes are unravelled.

"The spiral rope which constitutes the gland on the right side has no communication with that on the left, although the two glands are combined into one by cellular tissue, yet in its usual formation, the glandular structure continues entirely separate. This rope or chain is composed of lobes of different sizes, connected together by membrane, and by smaller portions of gland which surround a large internal cavity. To proceed with the investigation, remove a very thin superficial slice of each lobe, or of several of these, and numerous little cavities will be seen, which may be set open after the organ has been hardened in spirits of wine, and these are the secretory cells producing the fluid which issues so abundantly. The lobes being farther examined, beside their cells, are found to contain a small pouch at their bases which leads into a reservoir, so that the secretion which escapes finds a ready entrance into the cavity of the gland from which it may be absorbed.

"If a pipe be introduced into the gland, and alcohol be injected, and the organ immersed in strong spirits, or a solution of alum, a large cavity will be filled, which I shall call the reservoir of the thymus. This forms a general communication between the different lobes; it begins from the inferior part of the thoracic portion, and thence extends into the extremity of the cervical. The reservoir does not maintain a straight course, but passes in a serpentine direction through the thoracic part of the gland, and is somewhat more direct in the cervical portion. With regard to its size, it varies in different places, but generally is the largest near the centre of the thoracic, and it is least at the communication of the cervical with the thoracic part of the gland. In the cervical portion it increases, but is less than in the thoracic. When opened after being injected and hardened, its internal surface appears to be lined by a smooth membrane; but if it be at once dissected in water, this lining membrane is found to be of the mucous kind, for it is rather villous than smooth, and instead of having a few red vessels, when filled with a vermilion injection, it is found to be highly vascular, and the arteries which are distributed to it may be seen meandering upon its surface, and minutely dividing so as to redden every part of it.

"Its interior forms ridges, which are produced by small ligamentous bands which cross the surface of the reservoir in various directions, and encircles the mouths of the pouches; these bands are formed for the purpose of keeping the lobes together, of preventing an injurious yielding of the parietes of the cavity, and to give strength to resist too great an accumulation of the secretion. When the reservoir is floated in water, a number of small openings appear upon its internal surface; and if a probe be introduced into these, it passes into the pouch at the root of the lobes, so that by these apertures the secreted fluid escapes into the reservoir. These orifices are not so numerous as the tubes themselves, the reason for which is, that each point communicates with more than one lobe.

"The boundaries or walls of the gland are full of secretory cells which are minute; they communicate with one another and open into the pouch of the lobes, and from the pouch into the reservoir."

Sir Astley describes the best mode of dissecting and preparing this organ, to be the injecting of the superior cava with one coloured fluid, and the aorta with another, by which means the blood-vessels of the thymus are filled; then to remove it from its connexions, and dissect off its envelope of coarse membrane. Its lobes being now distinctly visible, an injecting pipe is to be passed into the interior of the gland, and the reservoir filled with alcohol, when not only it, but many of the secretory cavities besides, will be distended. It is then to be put for two days into spirits of wine or alum solution, and it will become hardened. The anterior surface nearly to the middle of the gland being cut off, and through the thoracic and cervical portions, an excellent view is thus produced of all the parts of the thymus. Coloured gelatine, quicksilver, and coloured wax, have all been employed for the purpose of injection. We may sum up the anatomical facts ascertained, thus briefly:—1. It is composed of a gland on each side united by cellular tissue.—2. It is formed of two cords, which when unravelled are of considerable length.—3. These ropes are constituted of large and small lobes.—4. They are disposed in a spiral or serpentine course.—5. Each portion of the rope is a secretory structure.—6. The lobes contain secretory cavities or cells.—7. A pouch of communication exists between the lobes and the reservoir.—8. The gland has a central cavity or reservoir.—9. This cavity is serpentine.—10. The reservoir is lined by a very vascular mucous membrane.—11. The ropes of the gland pass in a spiral direction around the mucous membrane which lines and forms the reservoir, and these ropes, by uniting that membrane to each other, assist in forming the cavity.

The arteries of the thymus gland are derived from the internal mammary, and superior and inferipr thyroideal arteries. Its veins have a different course, for although the internal mammary and thyroideal veins receive small branches from this gland, yet the principal veins are those which end in the *vena innominata*.

Sir Astley has only once been able to inject in the human subject the absorbent vessels proceeding from an absorbent gland of the thymus. Absorbent glands are found at the upper part of the sternum in the mediastinum; also a small gland between the thoracic portions, and some at the junction of the thymus with the jugular and subclavian veins, where the principal trunks of the absorbent vessels at all periods of life terminate.

The nerves of this organ are very minute, they are derived from filaments from the par vagum, sympathetic, and phrenic nerves, and assume a plexiform arrangement.

In speaking of the physiology of the gland, the author thus proceeds:—

"That an important function must be performed by an organ so uniformly found, of a size so large, of a highly vascular structure, and secreting abundantly, no one who duly considers the subject, can for a moment hesitate to acknowledge; and for myself I cannot subscribe to the opinion of those who think this gland is designed merely to fill a space which the lungs in their expanded state after the birth of the fœtus may be destined to occupy in respiration.

"If this had been the case, it would not have been a secretory organ, nor do I believe that nature in her wisdom creates any part of the body upon such views or principles.

"I cannot agree with the opinion that the structure of the thymus and absorbent glands is similar; one is conglobate and the other conglomerate; one is firm and compact, and the other is loose and pulpy;

the one contains cells of considerable magnitude when in a distended state, whilst in the absorbent glands the cavities are small, and with so much difficulty traced that there is still a doubt if they be cellular or vascular.

" The office which the thymus gland is designed to perform is evidently connected with the fœtal stages of existence, as it gradually lessens soon after the child is born, and even when the gland remains of considerable bulk, its secretory cavities are much diminished.

" It has been already stated, that this gland secretes a great abundance of white fluid; that it is situated between the veins in which the great absorbent ducts of the body terminate; that to each cornu is attached a large absorbent duct in the fœtal calf, capable of being filled with coarse injection, and that this vessel terminates at the junction of the jugular veins in the near innominata. This fluid, though constantly found in the human fœtus, having the appearance of chyle, viz. white like cream, but with a small admixture of red globules, is not easily procured in sufficient quantity to make it the subject of chemical analysis; but from fœtal calves two or three ounces may be without difficulty collected, and an abundant opportunity afforded of ascertaining its composition.

" The fluid thus collected from the calf has the appearance of cream slightly tinged with blood. *Warm water* dissolves a portion of it. *Heat* and *alcohol* coagulate it. *Sulphuric acid* not only coagulates but chars it. *Nitric acid* coagulates it firmly, first turning it white and then yellow. *Nitric acid diluted* precipitates a white solid from its solution in water, giving it the appearance of milk. *Muriatic acid* coagulates it firmly, and turns it white. *Liquor potassæ* converts it into a muco-albuminous matter, which falls in long-extended threads, like the saliva in ranula."

This fluid was sent by the author to Dawler, of Richmond, to be analysed. The result of that gentleman's analysis was this:—100 parts of fluid were found to contain 16 of solid matter; incipient fibrin; albumen; mucous and muco-extractive matter; salts, consisting chiefly of muriate and phosphate of potash, and phosphate of soda; of phosphoric acid a trace.

" It was ascertained, that the presence of the very minute quantity of free acid destroyed the affinity of the fibrinous particles for each other, for as soon as the acid was either saturated with an alkali, or further diluted with water, they then adhered together. A portion of the fluid on being dropped into two or three times its weight of water, and gently stirred about, will unite with a part of it, and after a short time be converted into a gelatinous-looking mass. This mass consists of a solid and of a fluid part, and which may be separated from each other by mechanical means.

" If the fluid from the thymus gland be examined in a microscope, it is found to contain an immense number of white particles, and a very small quantity of this fluid in serum exhibits those particles in the most satisfactory manner. It appears by the analysis that this gland secretes all the component parts of the blood, viz. albumen, fibrin and particles, excepting that the particles, like those of chyle, are white instead of being red. As to the muco-extractive matter in the fluid, it is probably derived from the mucous membrane, which lines the reservoir, and secretory cavities, of the gland.

It appears to be an error, that this gland continues for some time after birth to perform the same office as that which it supported in the fœtal state:—" I injected the gland in a child of one month, and I found that the lobes had become quite thinned by absorption, although the reservoirs in part remained, but even one of these was partially obliterated. In a child of four months the reservoir was very small, broken into several portions, and the weight of the gland, which should have been in the fœtus of nine months 240 grains, weighed only 45. In a calf of four months, the gland is very large, yet the cells and reservoirs will not receive one-half of the injection, which will enter in the fœtus of nine months.

" I will, therefore, put the following query:—As the thymus secretes all the parts of the blood, viz. albumen, fibrin, and particles, is it not probable that the gland is designed to prepare a fluid well fitted for the fœtal growth and nourishment from the blood of the mother before the birth of the fœtus, and consequently before chyle is formed from food, and the process continues for a short time after birth, the quantity of fluid secreted from the thymus gradually declining as that of chylification becomes perfectly established?"

The author concludes his interesting pamphlet by a chapter on diseases of the gland, and states that during 40 years practice he had met with but one example, that of a young lady 19 years of age, who suffered under a severe dyspnœa, and could scarcely remain recumbent for a few minutes. A swelling occupied the inferior part of the neck, at the upper opening of the thorax, and projected above the clavicle upon each side. The swelling had existed for many years, but of late had suddenly increased. Antiphlogistic and soothing measures were resorted to, with only slight and transient relief; she grew daily weaker, her legs became œdematous, and she could not sleep but in the sitting posture, and then with her head inclined forwards; for the moment the head fell back, the pressure of the tumour on the trachea made the dyspnœa intolerable. She ultimately died, worn out by irritation. On examination of the body, the disease was discovered in the thymus gland, which appeared of a yellowish-white colour, and was divided into several large lobes, the reticular texture of the gland was found filled by a white pulpy substance. The disease appeared to be of the fungoid kind.

The plates by Child are excellent, and the work altogether is equally worthy the attention of the student of nature, and the regular book-worm.

BOTANIC GARDEN, CHELSEA.

Wednesday, 13th June, 1832.

One of the Messieurs Lowe Wheeler gives demonstrations of botany here every Wednesday. He is evidently endowed with a portion of good sense, and is upon the whole a fair lecturer. In the imagination of many people, however, this family is intimately associated with ideas of rainy weather, owing, perhaps, to the peculiarly gloomy and pluvious physiognomies which distinguish them. This is against the Lowe Wheeler before us, who has the character so strongly marked that if it be true that Medusa's head was a general solidifier, Mr. Wheeler's is nothing less than the universal solvent. As a lecturer, however, the owner has some good qualities,—a clear head, a good mode of expression, and some elocution. He is worth a host of such grandiloquent pedants as Gilbert Burnett and some others of that kind. He indulges, however, too much in digression upon topics only remotely relevant to his subject, such indeed as the young student cannot fully apprehend or remember.

He follows in his course the plan adopted in the elementary work of Sir James Smith, treating, first, of the physiology of plants; secondly, of their forms and classification; and, thirdly, of their economic and medicinal applications. There is some reason to question the excellence of this arrangement, even in an introductory book. For if, in imparting knowledge, it be wise to pursue the same track as in seeking it, it is obvious that the external forms and mutual resemblance, or contrast, of vegetable bodies, would first impress the imagination, and precede any inquiry into the more occult points of growth—of culinary or medicinal virtues. But in a course of what are professedly called "demonstrations," to make long-winded orations on physiological and vegeto-chemical phenomena at the commencement, is injudicious. How can a dissertation on the gravitation and centrifugal force of the planets, or on the ligature of the aorta in the human subject, be with propriety intruded into a demonstration of the roots of plants. If Mr. Wheeler wishes to display his reading, he ought not to do it at the expense of the student's time. The object of the course should be, to bring frequently before the pupil's eyes those botanical facts which he cannot learn elsewhere, under a methodical arrangement which will materially strengthen his remembrance of them.

The majority of students whom we meet with in this garden appear to conduct themselves in a very gentlemanly manner. A few of them, however, are complained of. Before the demonstration this morning, old Flora Macdonald, the master of the Company, spoke of the injuries inflicted on the plants by the heedless portion of the students, and stated that several rare and delicate specimens had, by these means, been completely lost. A valuable plant maimed in this way, was exhibited; the terminal appeared to have been cropped off. It is extremely vexatious to see such injuries inflicted on valuable plants; but it is possible that the mischief on the present occasion was accidental, and though deserving reproof, yet not meriting the term "depredation," the appellation bestowed on it probably in ignorance of its meaning by the guardian in question.

We shall occasionally refer to the botanical transactions in this garden.

CORRESPONDENTS, &c.

Several Correspondents shall receive private answers in the course of next week.

We are sure our Correspondent *Mr. D.* will excuse our non-insertion of his reply to the remarks which appeared in the obscure print that has attacked him. If worthy of his notice at all, the answer should appear in the journal which contained the remarks.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JULY 7. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXV.

THE MEMBRANES OF THE BRAIN—THE SIZE, FORM, AND COMPOSITION OF THE BRAIN—THE FUNCTIONS OF THE CINERITIOUS AND MEDULLARY PORTIONS, AND THE INTELLECTUAL PRINCIPLE IN ANIMALS.

The Dura Mater in the Horse.—On endeavouring to raise the roof of the skull in any of our patients, we find, adhering to the inner surface of the bone, and separated from it with difficulty, a dense and strong membrane, the *dura mater*, at once the internal periosteum of the bone, and the covering of the brain. It adheres to the bone by innumerable prolongations of its substance, and by minute vessels, and these are most plentiful along the sutures, and the ridges of the bones. If I macerate either of the bones composing the cranium in diluted muriatic acid, the earthy matter will be dissolved, and I shall have remaining the outer and the inner membrane—the pericranium and the internal periosteum—with a great deal of interposed cellular tissue, and numerous vessels of communication.

Nature of the Membrane.—This adhesion to the bone gives to the dura mater a roughened surface exteriorly, while it presents a polished, glistening face to the membranes below—thereby obviating injurious friction between them. The dura mater is united to the membranes below by many little chords or prolongations of its substance, and these particularly following the direction of the sinuses. Each of these little chords or doublings of the membrane carries a small blood-vessel, and penetrates through the other membranes, and enters into the substance of the brain, and is firmly tied down wherever it enters, thus answering the double purposes of carrying blood, and giving support to the brain or its membranes, and particularly in the direction of the great divisions or sinuses. This membrane consists of many laminæ, and it is lacerated with much difficulty.

The manner in which it covers the Brain.—The dura mater completely envelops the brain, but does not enter into any of its sinuosities or cavities; it accompanies the optic nerve out of the cranium, and a prolongation of it, but singularly modified, as I shall have hereafter to describe, and no longer adhering to the bone, passes through the foramen magnum, and encloses the spinal marrow through its whole extent.

The Longitudinal Sinus.—The dura mater dips deep into the scissure between the lobes of the brain, and that in a very curious manner. When it arrives at the central scissure on either side, it splits into two laminæ, or collections of laminæ. One of them passes over the scissure, and binds the lobes together; the other runs deeply into it on one side, lining, as it were, the lobe on that side and then being suddenly inflected or turned at the bottom of the scissure, it climbs up the other side of the sulcus to reach the external transverse portion, and thus forms a triangular cavity—the longitudinal sinus—the reservoir of the superficial venous blood of the brain, as will be hereafter more fully explained.

The Falx.—These laminæ approximate to each other as they descend between the lobes forming the apex of this inverted triangle, and at length uniting, pierce yet more deeply, under the denomination of the falx in the human being; but from the flatness of the brain of the quadruped it is much less developed, and scarcely deserves the name of a falx or scythe. A similar division and prolongation of this membrane is observable at the tentorium, dipping down beneath the bony part in the form of an irregular but narrow fringe. This duplicature of the dura mater is particularly extended in some animals that have not a bony tentorium. See this cranial cavity of the sheep, how perfectly it is partitioned into distinct chambers by these membranous curtains. In some quadrupeds, however, the falciform process of the cerebellum is altogether wanting.

The manner in which the proper falx and the tentorium meet each other at right angles, and the support they give to each other, and the manner in which both are rendered tense and effective, are worthy of observation. The lateral sinuses are likewise constructed of the laminæ of the tentorium; and the sphenoidal and cavernous sinuses are formed from the splitting of the dura mater.

The Dura Mater in other Domesticated Animals.—I do not know of any considerable difference of structure in this membrane in our other patients, except that the cavernous sinuses are more extensive in the ox, and that they likewise contain a kind of arterial reservoir derived from the carotids.

The Arachnoid Membrane.—Beneath the dura mater and separated from it by a small portion of fluid, or at least of moisture on the surface of the membranes, is the *arachnoid tunic,* so called from being as fine as a spider's web. It likewise envelops the brain. It is said to float loose between the dura mater and the pia mater, but it is tied down by those chords and vessels from the dura mater which penetrate through it in order to reach the brain, and at these points of union where we are occasionally enabled to make a happy dissection, we find a portion of cellular substance of almost inconceivable delicacy surrounding the chord, and giving some strength to the attachment of so fragile a membrane. The arachnoid tunic is most easily demonstrated at the base of the brain superiorly to the origins of the optic nerves. Of its use I cannot confidently speak; it may either be a secreting membrane to supply the fluid interposed between the meninges of the brain, or it may be destined in some measure to cut off the communication between the brain and the dura mater, and to prevent its too much sympathising with any inflammation or injury of the membranes of the cranium.

It does not appear that either the dura mater or the arachnoid membrane is endued with sensibility, and in none of our patients have we proof of that inflammation of the tunica arachnoidea of which medical men have spoken.

The Pia Mater.—Beneath these is the proper investing membrane of the brain—the *pia mater*—and which not only covers its external surface, but penetrates into every sulcus, lines every ventricle, and clothes every irregularity and portion and part of the brain. So far as we can judge of it by maceration, it is composed of delicate but condensed cellular tissue; and, like the cellular tissue generally, it penetrates everywhere, being the vehicle by means of which the blood-vessels and the absorbents are conveyed, by which strength is given to every part, and by which they are at once divided and united. The external surface of the pia mater is smooth and polished—the internal surface cannot be demonstrated—it may occasionally be torn off, but it cannot be dissected—it is, however, necessarily roughened by the multitude of vessels which it conveys into the substance of the brain. Both the pia mater and the arachnoid membrane are prolonged through the whole extent of the spinal cavity.

The Bulk of the Brain.—We now arrive at the brain itself, and the first thing which strikes us is its comparatively diminutive size in our domesticated animals. Look at this brain of the human being, how broad, and rounded, and bulky, compared with the elongated, flat one of the horse and the ox. The human brain is double the size of that of the horse, and, compared with the bulk of the animal, it is many times the size. Comparative anatomists have given us a kind of table of the proportionate weight of the brain. In the human being it has been calculated at $\frac{1}{35}$ part of the weight of the frame; in the dog, according to his size, it varies from $\frac{1}{40}$ to $\frac{1}{180}$ part; in the horse it is $\frac{1}{430}$; in the swine $\frac{1}{500}$; in the sheep $\frac{1}{750}$ (Carus strangely says $\frac{1}{85}$); and in the ox $\frac{1}{800}$. Do we trace any connexion between this relative bulk of brain and the quantum of intelligence? We seem to have been classing our patients pretty nearly in proportion to their degree of intelligence. But we are not yet prepared for this.

Form of the Brain.—The flatness of the brain of quadrupeds, compared with the rotundity of the human brain, is one of the first things, I have said, that will attract our attention; and the flatness, when we turn the brain over, is far more remarkable below than above. The convexity of the middle lobes is strangely diminished, while the posterior lobe is in a manner lost. These circumstances, and particularly the last of them, account for the great difference in the base of the cranium in the human being and the quadruped,—the deep concavities of the one, and the comparative flatness and regularity of the other. Observe the prolonged oval shape of the brain in the horse, the ox, and sheep; somewhat broader, indeed, posteriorly than anteriorly, and this assuming almost the form of a triangle in the dog.

Irregularities of Surface.—We recognise an irregularity of surface, as on the human brain—certain prominences and depressions, but not so marked. The surface of the brain in the quadruped is comparatively tame and inexpressive; nay, some of our occasional patients have no convolutions on the surface of the brain; they are not found in the hare, the rabbit, the rodentia generally, or the bird; and they are not so bold or so deep in the ox as in the horse, nor so much so, compared with the difference of bulk, in the horse as in the dog,—the swine holding an intermediate place between the two. We must not dare to connect this with the proportion of animal food by which each is nourished, although, in proportion as he is carnivorous, his brain is usually convoluted; but I am afraid that we do not know enough of the functions of any part of the brain of the quadruped, to associate these convolutions with anything else.

Some comparative anatomists, whose descriptions are generally distinguished for their accuracy, have told us, that "in the opposite hemispheres of the cerebrum in animals, the most exact correspondence may be traced between the dispositions of the convolutions in both." This is erroneous: compare together the hemispheres of these brains, and tell me whether you can find in either of them this "most exact correspondence." I am bound to confess, that after having been in former years a craniologist, my faith began to be shaken, and was at length overturned, by not always finding even "the general resemblance" that exists in the human brain, and certainly "no correspondence between the convolutions and the superficial structure of the brain," and no possibility of discovering it in the living animal, if such correspondence had existed.

The Divisions of the Brain.—The contents of the cranial cavity are divided in pretty nearly the same manner in all quadrupeds. There is the *cerebrum* and the *cerebellum*, the former divided into two lobes, and the latter into three, or rather into two, united by a belt. There is considerable difference, however, in the relative size of the cerebrum and cerebellum. In the human being the cerebellum is about one-ninth of the bulk of the brain; in the ox it bears the same proportion; in the dog it is one-eighth; in the horse one-seventh; and in the sheep one-fifth: this, however, depends upon the age, for it is only in the adult animal that the cerebellum is fully developed. We have not yet inquired into the functions of these two portions of the brain, and when we do, I fear that we shall not be able satisfactorily to account for this difference of bulk.

The relative situation of the cerebrum and cerebellum present, as I stated in my last lecture, only an apparent difference. In the quadruped the cerebellum is posterior and even superior to the cerebrum; in the human being it is inferior; but this depends upon the manner in which the head is connected with the atlas. In both the cerebellum is the portion nearest to the foramen magnum, and under which the crura cerebri must pass to arrive at the medulla oblongata and spinal chord.

The Difference in the Composition of the Brain.—If I cut into the brain, I find it composed of two substances very unlike in their appearance. The one from its situation on the outside of the brain is called the *cortical*, and from its reddish *ashen* colour, the *cineritious* substance; the other, from its pulpy nature, and found deeper in the brain, is termed the *medullary* substance. Long hardening in alcohol has proved them both to be of a fibrous structure, and has confirmed the old opinion that they are essentially distinct from each other, as well as of very different composition. Placed in apposition with each other, and seemingly mingling, they never change by degrees into one another, or run, as it were, into the same mass.

The fibres of the cineritious portion run in various ways: on the surface of the brain they are vertical,—deep within the brain, they take every possible direction. While they are unconnected with the medullary substance, they are in a manner isolated among each other. The medullary fibres are connected throughout the brain. They have all a direct or indirect tendency towards the centre and base of the brain; and from their common centre, or condensation of substance and influence and power, the nerves derive their origin. The nerves seem to be prolongations of the medullary substance. The cineritious portion is scattered every-where, chiefly distributed towards the outside of the brain, but found also deep in its centre; and striæ, or portions of it, at the root of every nerve. The medulla oblongata and the spinal chord are evident prolongations of it.

The Proportions of Medullary and Cineritious Portions in different Animals.—The facts that I have just stated led to the most interesting inquiry in which the physiologist can be engaged, viz. a comparison between the brains of various animals—the proportions of these two substances in each, and the character and destiny of the different animals; and the conclusion drawn from such an inquiry will be arrived at without difficulty, and will be perfectly satisfactory. Compare together these brains—that of the human being, the horse and the ox. Observe, in the first place, the development, greater beyond comparison, of the nerves of sense in the brains of the inferior animals;

compare the olfactory nerve in the human being and the quadruped, (I must maintain that it is a perfect nerve, although our best comparative anatomists have degraded it into a mere mammillary process,) that belonging to the human brain is not one-fourth the size of the same nerve in the horse, nor a sixth part so large as that in the ox. Compare the optic nerve in the biped and the quadruped, the size of the tractus and the bulk of the nerve itself—the auditory nerve affords as satisfactory a point of comparison. Examine that which I have described as the common centre of the medullary substance or influence, that which gives origin to every cerebral nerve of sensation and of motion; first, the crura cerebri, mere chords in the human being, compared with the broad and bulky pillars of the quadruped. Observe the medulla oblongata, the centre or the condensation of the medullary matter of the cerebellum as well as the cerebrum—the origin or commencement of the spinal chord—connected with loco-motion, with strength and common sensibility—connected too, as we shall hereafter see, with the powers of organic as well as animal life. How striking is the difference!—the breadth of the medulla oblongata in the human being is but one-seventh part of that of the brain; in the horse and ox it is nearly a third; in the dog it is more than a half; in the hog it is more than two-thirds. Notwithstanding this disproportionate bulk of medullary matter, the brain of the horse, the ox, the swine, and the dog, is comparatively less, much less than that of the human being. The brain of the dog is scarcely a fourth part of the proportionate weight of the human being; the brain of the ox is not a twentieth part. Then how is this difference made up? Why in the cortical or cineritious parts. That preponderates as much in the human brain as the medullary substance does in the brain of the quadruped.

The Functions of the Medullary and Cineritious Parts in the Human Being and the Quadruped.—Now then, Gentlemen, a little light begins to break in upon us. In the quadruped the medullary portion preponderates; in the human being, the cineritious. In the former, the senses are acute and the muscular power is great; in the other there is or ought to be a stronger mental principle. The hunter will recognise the cry of the hounds, and his ears will be erect, and he will be all spirit and impatience, long before his rider is conscious of the least sound. We soon find in the exercise of our profession, that when all is dark and black around us, we can trust to the keener vision of our horse, who, if we resign ourselves to his guidance, will carry us safely through the intricacies and dangers of the road, and we must have carelessly observed him, if we have not been taught that the sense of smell supplies in him the place of the sense of touch and lessons of experience. To this it must be added, that in power or quickness of muscular action, according to the kind of service that we require, he is all that we could wish. In the human being, the senses are comparatively dull and obtuse—neither in hearing, sight, or smell, can we vie with the meanest brute, but we have the power of thought to arrange and to compare the intimations which our senses afford us, and to extract from them all that is necessary for our existence and comfort, and to combine with the impressions that we receive from them many an association which renders them productive of intense delight; and, best and noblest of all, to make them the foundation of intellectual improvement and moral worth.

Comparison between the Cineritious and Medullary Substance in different Quadrupeds.—Observe these brains—that of the horse and that of the ox. The brain of the horse is considerably the larger of the two—its relative weight is nearly double. How is this difference made up? In some parts of the base of the brain there is more medullary substance in the ox than in the horse—the medulla oblongata and the spinal chord are larger—the additional bulk of brain in the horse is composed of cineritious matter. Observe again these brains—that of the dog and of the sheep. The dog was not half the size of the sheep, and yet his brain is larger, although, if we examine the base of both, there is proportionally more medullary matter in the sheep than in the dog. Here again the additional bulk of brain is composed of cineritious substance. How different is the character of these animals!—the sluggish stupid ox, and the intelligent horse—the silly sheep, and the intellectual, companionable dog. I must not use decided language on such a subject, but the connexion between the cineritious part of the brain and the intellectual principle, and that between the medullary portion and the mere animal principle, does seem at least highly probable. The latter is the medium through which the impression is conveyed, or the motion is effected—the former is the substance to which that impression is referred, where it is received, registered, and compared, and by which the operation of the motor nerves is influenced and governed; the one is providing materials to work upon—often rude and useless beyond the present moment—the latter is surveying and arranging them, and building up the fabric of intellectual and moral worth.

The Preponderance of Animal Power in Brutes.—The substances of which the brain is composed are the same in all our patients and in every vertebrated animal. They differ only in the mode of arrangement, and in their relative quantities. It is what we should expect, after experience has taught us that the difference between the human being and the brute, in intellectual and in moral worth, consists in degree and not in kind. The animal portion of the brain preponderates in each—and in each with beautiful adaptation to the situation in which he is placed, and his connexion with man. Look once more at the olfactory nerve, bearing in development an invariable proportion to the necessity for acute scent, not only for the purposes of the animal individually, but of man. Observe this nerve in the horse, how large compared with that of the human being—larger in the ox, who is not so much domesticated, and oftener sent into the field to shift for himself—larger still in the swine, who is to search for a portion of his food buried under the soil, and immersed deeply in refuse and filth, and largest of all in the dog, the acuteness of whose scent is so much connected with our pleasures. As we advance in our subject, the organic nerves will pass in review before us, and there we shall observe the same comparative difference, both in those of the spine, and the deeper-seated ganglia—the motor, and the secretory or chemical organic nerves—the worth of the horse depends upon his wind, and that of the ox on his disposition to fatten.

The Diminution, yet existence, of Intellectual Power in the Brute.—Then this *cortical* part—once more observe it, compare it. Forgive a little repetition here. How different its relative quantity—whether compared with the human brain, or those of various animals! Once more observe it in the dog, the horse, the sheep, the ox—comparatively small in each—diminishing in each according to the evident proportion of intelligence. Small we say in each, for in their wild state the brutes have no concern, and no idea beyond their food and reproduction—in their domesticated state they are destined to be the servants of man. The acuteness of their senses—the preponderance of the animal power, qualify them for this service, but were proportionate intellectual capacity added to this—were they made conscious of their strength, they would burst their bonds, and man would in his turn be the victim and the slave. Still however the cortical part is found in each, prevailing in each as it would seem to be needed for our purpose that intelligence should be added to animal power. In each the organisation, the connexions, appear to be the same; and the distinction in the intellectual—aye, and even the moral qualities in the biped and the quadruped, is in degree and not in kind—nay, it may so happen, that the one being advanced and improved by circumstances, and the other retarded or contaminated, there may be more difference between the cultivated and good man, and the savage and depraved one, than between the biped and the brute.

Conclusion.—If we had time I might trace in our patients the palpable existence of every mental faculty—attention, memory, association, imagination, judgment; and I might trace too, shall I say, the germ—no, the full development of many a virtue—parental, filial, social affection—courage, fidelity, gratitude, disinterestedness, and a consciousness of right and wrong. This would be a pleasing relief to our severer inquiries into anatomy and disease—it would reconcile us to our profession—it would teach us to admire and to love those whose medical guardians and friends we had undertaken to be—it would give that impelling principle, that aim and object, which would ensure our professional success. To this subject let your attention be often directed, every day will afford you pleasing illustration of it, and the result will be that your professional fame will be established on the only sure basis—science and humanity.

LONDON HOSPITAL.

CLINICAL LECTURE

BY

DR. BILLING,

Delivered Jan. 21, 1832.

TUBERCULAR CONSUMPTION.

In the lecture to-day, Gentlemen, I shall take the opportunity, in tracing the progress of cases of that scourge of our country, tubercular consumption of the lungs, to point out to you those diagnostic symptoms, which you will be constantly and anxiously required to decide upon in the future exercise of your profession; and at the same time that I show you the inflexible character of the disease, and how slight the chance is of recovery from it, let me impress upon you the possibility of temporary, and even of permanent cure, and the rational mode of attempting it. In order to illustrate the symptoms of the stages of this formidable disease, I have laid on the table a series of specimens of the tubercles of the lungs at different periods of their progress, and of the destruction of the lung subsequently, besides that which has been preserved from one of the patients in question after death, but which is even less satisfactory to the eye than those which have been put up, suspended in a clear fluid.

The Tubercles.

In speaking of consumption, I confine that term to the disease depending on the generation, growth, softening, and evacuation of tubercles, and do not apply it to any of the sequelæ of the common inflammations, catarrh, peripneumony, and pleurisy, although they also may produce hectic, and prove fatal; but tubercles are a specific morbid growth in the lungs, not produced by common inflammation, but arising like scrofula, if not identical with scrofula, and which, so far from being produced by inflammation, do produce inflammation themselves only when they have grown to some size, though inflammation may incidentally occur simultaneously with them. These little light-coloured grains, not larger than the head of a pin, are the first state or germination of the tubercles in the lungs, which have the name of miliary tubercles from their resemblance to millet seed; in this state it is my opinion, that they may remain for some time without producing any symptom whatever; the person may have a cough at the same time, from catarrhal affection, or sympathetic irritation, but I can give you an instance that they may be in the lungs without producing cough, though it is very seldom that you have an opportunity of seeing this confirmed. You ought, however, when any person dies from an accident in the surgical wards, to search for young tubercles in the lungs. You may also obtain miliary tubercles in lungs which have old tubercles, as it is their nature to come on in successive crops, so that you may find young ones when a patient has died of the old.

The case to which I have above briefly alluded, was that of a young female who was under treatment for amenorrhœa in the hospital; she went out on leave of absence on an inclement day, and got an attack of peritonitis, of which she died in a few days; she had not had cough, though it is so common a complaint with amenorrhœal females, yet on examination of the viscera, there were found a great many miliary tubercles, such as you see before you, in one lung. These miliary tubercles are more easily felt than seen, except when the lung is injected with red, as they are of a semi-transparent grey, and when cut into, or bruised with the nail, have very much the appearance of the lymphatic ganglia; they grow larger by a deposition of a whitish substance resembling coagulated lymph, only denser, which seems to be deposited within them, rather than that they are so converted, and this substance afterwards softens to the consistence of curd, this curd-like matter not differing in appearance from what is always formed in strumous enlargements. Whilst this white substance is firm, the tubercle is called crude or unripe, affording this firm white appearance when cut into. Such being the second stage, as the term maturation is applied to the third stage, having been supposed to be suppuration, and the term maturation or ripening applied to abscesses, was applied also to the softening of the tubercles; but this melted-down tubercular matter is different from pus, which is formed fluid in the first instance, as you see every day on the surface of granulating wounds or ulcers. This melting down of the tubercles is the effort of nature towards a cure; by their thus becoming liquid, the creamy matter makes its way into the bronchial tubes, by an ulcerative process, during which, that is whilst there is that degree of inflammation previous to the breaking, as well as whilst they are emptying, there is hectic fever, and according as the tubercles are evacuated, these cavities, or a honey-combed appearance, are left in the lung, and, as you see, the tubercles are often in masses or clusters, there will be often these large pouches left, when all the white matter is expectorated; and provided there have been but few tubercles in the first instance, the cavities left get a kind of new skin or lining to them, and the patient may live on, which is an actual recovery from actual phthisis, which has by some been thought impossible.

Now the nature of tubercles is not yet decided. My opinion is, that they are a strumous disease of the minute lymphatics of the lungs, growing like other tumours by the addition of coagulable lymph, which assumes various grades of organisation, and follows the course which I have just described. The analogy of the morbid process is in favour of this, and also the most successful treatment in the few cases which recover after the tubercles have ripened. Considering this simple statement of the disease, you might be surprised that more do not recover, but you must recollect that these strumous tumours being within the lungs, must, during their progress, induce, and be complicated with, more or less peripneumony, pleurisy, and catarrh; besides the diminution of the pulmonary tissue so necessary to life, and the hectic wear of the constitution.

Besides the distinct tubercles in a crude state, there sometimes is found a mass or masses of this crude tubercular matter, to which Laennec gives the name of tubercular infiltration; I believe it to be just similar to the other, a deposit of lymph produced by the tubercles, but coalescing into a mass according to circumstances of greater or less inflammatory action, &c.—a very similar deposit taking place in cases of peripneumony, to which Laennec has given the name of purulent infiltration, but which is by no means pus, but coagulable lymph, solid at first, and which afterwards melts down, like the tubercular deposit, the tubercular being the firmer of the two, perhaps from its more gradual formation.

Now the tubercles at commencement do not necessarily produce any symptoms, no more than a chain of strumous knots on the lymphatics, which may frequently be felt in the side of the neck, but produce no uneasiness whatever, and are often reabsorbed without any remedy being used; and we have no proof whatever that tubercles in the lungs are not reabsorbed, and I rather suspect they sometimes are; the difficulty that we have in ascertaining their existence in the early stage, is still very great, unless they be in such number that there is little hope of recovery. Yet the progress of auscultation has been such, that we may hope for still further additions to our power of diagnosis, and the more thorough knowledge we have of the nature of the disease, the more power we shall have to combat it. Hitherto the application of remedies in phthisis has been in many cases quite empirical, and often inert, sometimes mischievously active. You must make up your mind that it is a disease of tremendous power, and that there are but few cases within the reach of art; but be certain that some are, otherwise you will lose that persevering energy which it is the duty of every medical man to exert, as long as there is life; I do not say as long as there is hope, for oftentimes is there recovery from various diseases, after all hopes had been relinquished.

The Treatment.

If you understand the treatment of strumous cases, you have the groundwork of the treatment of phthisis, modifying that by calculating the nature of the organ in which the tumours are formed, and throughout making every effort to support the strength, not merely to preserve the vital powers, but for the purpose of promoting the kindly healing of the sores; for you must have abundant opportunities of seeing in the surgical wards, how soon strumous and other ulcers become worse, as the patient becomes weaker; hence one of the great difficulties in phthisis is, that constantly the presence of the tubercles brings on inflammation, that being in the chest is peripneumony, or pleurisy, calling for antiphlogistic treatment, and the risk is, that in reducing the acute inflammation you reduce the power of the constitution, and so increase the chronic or strumoid disease. Here you have an explanation of the benefit experienced by some in the use of digitalis, which keeps down the pulse and acute inflammation, without wasting the vital fluid, or depressing the system, except in some cases where it disagrees with the stomach, and then of course does mischief by weakening; but because digitalis was found of use, it was recommended and used empirically, that is indiscriminately; and because it was used indiscriminately in such a fatal disease, it of course lost its character, except with those who could understand in what states it was occasionally beneficial, and would only use it in those; for in the cases of phthisis with a feeble pulse, and no tendency to acute inflammatory action, it could do nought but harm. Again, the constant repetition of emetics has done good on a similar principle in phthisis, as well as in abscesses, by checking the deposition of new matter, and facilitating the removal of old, as elaterium is useful in dropsy by its emetic as well as purgative properties; but the repetition of emetics is so distressing, and the chances of cure of phthisis so few, that few persons now prescribe this mode of treatment, which was at one time much praised and resorted to in this disease, as well as by surgeons for the removal of abscesses.

Some years ago a gentleman of the name of Stewart adopted a rational mode of treatment with which he had considerable success, but because he could not work miracles his plan was depreciated; his method was entirely tonic, and especially the cautious use of cold and tepid ablutions of the skin, a modification of cold bathing, a remedy which is found so uniformly beneficial in promoting the resolution of strumous tumours. Some patients with phthisical symptoms recovered strength, under his directions, for a time. Many had a recurrence of their symptoms, and died; but this should not be held an argument against the propriety of his treatment, which brought the patient through one crop of tubercles, because a second or a third crop proved fatal. The hydrocyanic acid, again, has been of great use in some possible cases, but not being able to effect impossibilities, it has shared the fate of digitalis, and of Mr. Stewart's plan.

Case, with Recoveries.

About nine years ago, a young married lady, who had two children, came under my care with all the symptoms of confirmed consumption, cough, and muco-purulent expectoration. She had occasionally spit a little blood; there were night sweats and colliquative diarrhœa. I supported her strength with animal food, and some fermented liquor, whenever her pulse could bear it; gentle exercise in the open air, and free admission of air into her rooms. I restrained the diarrhœa by catechu, logwood, and sometimes opiates; sometimes applied half-a-dozen leeches and blisters, and gave digitalis for a few days, when there was appearance of acute inflammation; sometimes gave bark and soda,—sometimes quinine with diluted sulphuric acid, which restrained the sweats. Beyond my hopes, she got well, and continued so for above five years, having one child more in that time. I think that during the attack she had expectorated a crop of tubercles, but I cannot be certain, as I had not then practised auscultation. However, a little more than three years ago, she had a renewal of all the symptoms, and I know that she then had cavities in the lungs; there was crepitation, pectoriloquy, and cavernous respiration and rhonchus. From this she recovered again; and when she regained her strength had another child. Six or seven months ago she had another return of the same symptoms, and died,—the lungs, upon examination, being more completely excavated than I have ever seen, except in one of those specimens from the museum. The process of utero-gestation is said to suspend the progress of phthisis, but the period of one pregnancy bears a small proportion to the interval between the first and second attack.

Auscultation.

One great advantage of auscultation is, that it enables us to decide whether any cases of consumption are cured or not. Previous to its discovery, if a case got well, there was always a doubt whether the patient had been really consumptive, as in Stewart's cases, whilst all who died were put to the account of consumption without any reserve. I may here recapitulate a few of the auscultatory observations made in the wards. If there are but a few miliary, or even crude tubercles, you have no evidence from auscultation or percussion; if there are many, or tubercular infiltration, and you may almost always expect to find them near the clavicles, there is diminution of respiratory murmur; but from the solidification you will have more or less bronchial respiration or bronchophony, and some diminution of sound on percussion. Whereas, if the cough for which you are consulted be produced by catarrh, the sound on percussion will not be dull, and there will not be broncophony; and if the murmur be diminished by emphysema, the sound from percussion will, on the contrary, be extra loud. In the progress of the disease, there is no alteration in these auscultatory signs, except augmentation, until the tubercular matter, having ripened, begins to make its way into the spongy texture of the lungs and bronchia, when a kind of crepitation is heard, being a mixture of rhonchus mucosus with the rhonchus crepitans, or what Laennec calls subcrepitans, and partaking more of the pure crepitation, in proportion as, at about this time, the lungs may become in a peripneumonic state just round the tubercles, or mixed up with œdematous crepitation when the consumption has brought on dropsical symptoms; besides which you find occasional admixture of rhonchus sonorus gravis, or sibilans, which you must learn not to confound with the diagnostic signs, but which we cannot analyze in a clinical lecture, as may be done with facility and advantage at the bedside, and you have but too many opportunities in the wards. I must remind you again and again, when you do not hear sounds satisfactorily during respiration, to make the patient cough, so as to remove any mucus which may be obstructing the tubes, and so prevent the real state from being heard. After the crepitation has lasted some time, you begin to have new sounds, according as the cysts of the tubercles get cleared out; when they are quite empty they produce cavernous respiration and pectoriloquy; when they contain some soft tubercular matter, and pus or mucus, you hear cavernous guggling rhonchus. The sound from percussion at that period alters, becoming louder again on account of the hollowness from the excavations.

Pectoriloquy is a certain evidence of the existence of a cavity; it produces pectoriloquy distinctly, if not larger than a nutmeg, or even less. In one instance, we had a patient with aneurism of the aorta, in whom, during examination of the chest, pectoriloquy was observed in one spot only, which was between the scapula and spine, and which sometimes ceased to be discoverable for a day. After death, it was found that disease of a vertebra, close to the head of a rib, had caused a small abscess, which, instead of pointing externally, had made its way into the lung, through which the pus was evacuated; this cyst, not so large as a nutmeg, gave pectoriloquy when empty, but when full of pus, as when he had been lying quiet, gave no sound.

Having prefaced so much, I may now enter on the minutiæ of the case which I have selected, as showing most of the usual phenomena of this disease, the progress of which you have witnessed.

CASE I.

M. C., ætat. 25, unmarried, October 27th, 1831, has had cough for twelve months, which she attributes to wet feet; feels very weak, and says she has fallen away, but still cannot be said to be emaciated. She complains of debility and tightness in the chest; from the commencement she has had pain at the sternum, which sometimes subsides, and returns when she catches cold, as from the least dampness of feet; had stitches in the left side at first, at which time she was bled without relief, and again in June, with no better result; ex illo tempore catamenia usque abfuerunt. Has colour in the cheeks, not circumscribed; tongue nearly clean, but too florid, and cracked, and with a clear white fur, or pellicle, at the back; very little appetite, and none for animal food; has had tendency to diarrhœa for the last fortnight; has chills, succeeded by flushes of heat, and night sweats producing debility; skin at present natural; pulse 108, soft. The cough is loose; expectoration greenish-yellow and mucose, as from tubercular excavations; she once expectorated some blood. There is crepitation in various parts, especially at the upper part of right lung, where there is bronchial respiration and also cavernous respiration and pectoriloquy.

Mist. ferri comp. ℥iss *ter die;*
Lohock empl. cantharidis sterno. Milk diet.

In this case nosological symptoms were not sufficient to establish the existence of tubercular consumption, for the whole train mentioned might have occurred as the production of the cause alleged, that is, catching cold from wet feet; for it appears that it did actually produce a pleuritic attack in the first instance; and the sequelæ of a severe pleurisy are, not unfrequently, hectic, with cough and expectoration, and we know well that the description of the expectoration given in the statement of the case is but presumptive evidence. The expectoration of blood, too, is by no means inconsistent with the supposition of the disease being only pleuritic in the first instance; and I must here take the opportunity of observing to you, that though there are not many cases of phthisis in which there is not some expectoration of blood at some period, still that many cases of hæmoptysis occur without being connected with tubercular disease, and more especially in females. With respect to the natural state of the skin, and colour of the cheeks being not circumscribed, as stated in the first report, this is worth your attention, as showing how perfect an intermission there was in the forenoon, though the hectic was fully formed, so as to produce hot dry skin and circumscribed hectic flush in the evenings, followed by night sweats, or rather morning sweats, for the patient is generally hot, dry, and restless till four or five o'clock, when a sleep comes on, from which patients are soon awoke by an uncomfortable state of perspiration. She had an aphthous state of the mouth, the fauces being inclined to be sore, as well as the back of the tongue, with a whitish pellicle; the tongue florid and glazed as if skinned; but neither this thrush in the mouth, nor night sweats, are diagnostic of tubercles, they being met with in hectic from any cause, dysentery, for instance, or from abscess in the liver, or groin, or psoas abscess, &c. But when, added to the preceding symptoms, we have the signs recorded from auscultation, all doubt was removed, and we had only to trust to the means of supporting the strength, and allaying the irritation. With a pulse 108, and soft, and the symptoms enumerated, there was, to my judgment, no indication for bleeding of any kind, nor digitalis, nor antimony, nor any debilitating or contra-stimulant medicine. The case was hopeless, but our duty remains the same, to follow up a rational plan to the last. I gave the mist. ferri co., the Griffith's mixture, once celebrated in consumption, and nothing more suitable here, as the myrrh and iron both would help to restrain the diarrhœa, and give some tone to the primæ viæ, which evinced loss of it by the want of appetite; the linctus, which has a minute portion of tinct. opii in it, lubricates the fauces, and allays the cough, and the blister was indicated to relieve the chronic pleuritic state which existed; she could not take animal food, and milk was the next best nourishment for her. The next day the report was as good as could be expected; the action of the bowels was restrained, and the breathing was relieved by the blister. On the next visit, before going up to her, I mentioned to the pupils that she would give a good account of herself, it being the peculiar characteristic of the disease to be buoyant with hope and confidence to the last. November 1st. She said at once cheerfully, that she was much better, but she could not specify any amelioration, except less uneasiness in chest and better spirits. She felt stronger from the effect of the tonic, but her disease was unabated; and upon being questioned, she said her cough was not better, she was no stronger, and had no better appetite; night sweats continued, and though she said at first that she slept well, she acknowledged that she was disturbed by the cough.

After this the hectic increased in the daytime, the other symptoms advanced gradually; emaciation went on, and diarrhœa returned, yet in each report the first words are, "feels much better," though our subsequent cross examination tells the too steady progress of the malady. At one time she had bark and soda by day, and an opiate at night, which relieved her temporarily; at another time she took ʒi hydrocyanic acid every two hours, for a few days, to allay an irritable state of the stomach, which it succeeded in doing, but made her feel weaker. She then had Griffith's mixture again, but it is unnecessary to state more than that aphthæ increased, and the diarrhœa was not to be restrained. About three days before her death, her voice became inaudible from the thrush, and delirium came on in the night-time; she returned to her senses in the day-time until the 15th instant, when she died, making at the last no complaint, except of soreness of the larynx.

On examination after death were observed, the whole upper part of the right lung adherent to the pleura costalis by its own pleura in a state of recent inflammation, and which formed part of the parietes of a large excavation, without any pulmonary substance being left to intervene. On the left side old adhesions of the pleura, this being the site of the first pleuritic attack mentioned in the case.

The right lung had multilocular cavities throughout. In the left lung there were several cavities superiorly. In various parts of both lungs there were tubercles in different stages, all which appearances you have now before you, explanatory of the symptoms, and also these ulcerated appearances in the intestines at the termination of the ilium, and about the sigmoid flexure of the colon, explanatory of the distressing diarrhœa which constantly occurs in these cases in the latter stages.

The thrush in the mouth is generally coeval with, and indicative of, this ulcerated aphthous or thrushy state of the bowels; but you must not imagine that when there is thrush, especially in young persons, there is always ulceration of the bowels, for children have thrush very frequently when debilitated by irritative diseases, such as from teething or worms, from which they rapidly recover when the cause is removed.

CASE II.

The next case is one of those which, before the diagnosis established by Laennec, used to be thought phthisis laryngea; affording false hopes that counter-irritation on the throat, &c. might effect a cure. She had also symptoms, which used to get many a case of phthisis the name of a liver cough, that is, the pain in the right hypochondrium, costiveness, and indigestion; but you will find when I read the auscultatory symptoms set down, that they tell too truly the state of the lungs.

M. T., ætat. 30, married. 19th January. Complains of cough, shortness of breathing, and pain in the right hypochondrium. Says that ten months ago she had rheumatic fever, from which she did not recover for five months, when cough and dyspnœa commenced, which have increased up to the present time; expectoration puriform; she perspires much at night, and is losing flesh; pulse 120; tongue clean; bowels confined; she sometimes brings up her food; voice hoarse and weak; cavernous respiration; pectoriloquy under both clavicles; crepitation. *Hirudines* x *lateri dextro et postea vesicatorium. Haustus cathart. statim. Milk diet.* Yesterday she was slightly relieved, but there is too much mischief in the chest, and a great deal of pleuritic inflammation, going on, with a hard pulse. I ordered leeches again with ʒxv *tinct. digitalis ex mist. salina antim.* ℥iss *ter die.* To-day the pleurisy is relieved, but the auscultatory signs forbid us to be flattered by that.

Concluding Remarks.

I have here presented you, Gentlemen, with exemplifications of the nature and treatment of the disease, phthisis. In many cases there occurs, the super-addition of peripneumony, pleurisy, hæmoptysis, or catarrh, or complications with disease of other viscera, and often dropsy comes on at the last, and hastens the fatal terminations, or that pleuritic effusion into the chest, which Laennec calls empyema. When there is hæmoptysis, peripneumony, or pleurisy, or any complication requiring antiphlogistic treatment, recollect the analogy between the phthisical and strumous constitutions, and save the force of the patient as much as possible. I would not have you bleed patients beyond what is absolutely necessary in any case, but in a sound constitution, with peripneumony or pleurisy, you err on the safe side by knocking down the inflammation. In a consumptive habit, you must avoid knocking down the constitution if you can help it. But, I must confess, you are placed between two evils, and so choose the less as well as you can judge. One thing I am convinced of is, that the true principle of treating consumption is, to support the patient's strength to the utmost, and that though occasional complications may call for antiphlogistic treatment, tubercular disease, by itself, does not. I must caution you against shutting up phthisical patients in warm rooms. I am satisfied that the want of exercise induces a languor, which makes them wear out faster than if permitted to ride or walk according to their strength in the open air; at every exacerbation of their complaint phthisical patients say they have caught fresh cold, but the same thing occurs when the experiment has been tried of keeping them in rooms graduated by a thermometer. A mild climate is useful, by permitting more free exercise in the open air, but when you look at the specimens on the table, you may judge whether a warm climate could regenerate such lungs.

PETECHIAL FEVER.

We have two new cases of the petechial synocha this week; and in one you have had the advantage of seeing the progress of the disease from its commencement, as it occurred in a patient who was convalescent, and on the point of leaving the hospital,—that young woman whose case of severe erysipelas of the leg I explained to you at a former lecture (p. 585), and who remained some time, after her general health was re-established, on account of a weakness of the limb, which was relieved by the support of strapping with emplast. plumbi.

CASE I.

S. J., ætat. 19, unmarried. 20th January. She was attacked on the evening of the 17th inst. with chills and shiverings, succeeded by severe pains in the head, back, and limbs, accompanied by vertigo; skin hot and dry; face flushed; nurse said she had remarked her being languid for some time; tongue white; pulse full, strong and frequent. Mr. Williams gave her an emetic, and afterwards some opening medicine. Yesterday, the symptoms continuing, petechiæ out, bowels open, she was bled to ℥xvi. To-day she complains of nausea; had no sleep, and felt her head confused in the night; it is clear at present, but aches; pulse 120, not strong but hard; tongue white, furred; skin hot and dry, but perspiring at some parts; epistaxis in the night; bowels not open; abdomen rumbling, not tender on pressure; throat sore; some cough; rhonchus sibilans.

Abrad. capillitium; *hirud.* x *temporibus*;
Enema cathart. statim.
Contin. sulph. quinine.

In this case there was no obscurity of symptoms; the fever was synocha in full force, requiring the antiphlogistic treatment which was adopted. You know that I do not consider tonics stimulant; she had been taking sulphate of quinine before this attack, and I allowed her to continue it. I have often observed to you that it has little influence in continued fever, though so efficacious in intermittent or remittent; here, fever came on whilst she was charged with it. I am satisfied it does no harm in continued fever, and that the moment the fever has turned, the sooner it promotes convalescence. The head is the part most affected in this case, the chest less than in most of the others who have had this epidemic; from the rumbling state of the bowels I have ordered only an enema, as you have seen that in several of these cases when the bowels are set going, they run on too fast.

I find to-day that the bowels are not open, she has had a restless night, and complains of her head, but this you must expect whilst the fever holds, which we cannot stop though we may control it. I have ordered

Hirud. x *temporibus*; *and*
Magnesiæ sulph. ʒj;
Ant. tart. gr. $\frac{1}{8}$;
Ex aquæ ℥iss, *4tis horis dum alvus soluta fuerit.*

The next case is more severe, as having been of longer duration without remedy, and besides the usual affection of the head, considerable complication of inflammation both in chest and abdomen.

CASE II.

M. H., ætat. 15, unmarried. Jan. 19, 1 p. m. Complains of headach, cough, pain in the abdomen, and of its feeling hard and distended; took to her bed on the 14th inst., but was ailing some days previously with headach; only one scanty motion since 14th; eyes dull and rather suffused; cough frequent, dry; pulse 120, weak; tongue white; skin hot, and moist in some parts; abdomen tender on pressure.

V. S. ad ℥xvi. *Enema cath. statim.*
Antim. turt. gr. $\frac{1}{8}$;
Magnesiæ sulph. ʒj;
Ex aq. ℥j, *4tis horis.*

Seven p. m. Head relieved from pain and sense of weight by the VS. She has had five copious motions since the enemā.

20. Feels better, head relieved, but still complains of the abdomen, which feels tumid and tender on pressure, and of the cough hurting the abdomen; no bronchial rhonchus; thirst, skin, and eyes, as yesterday; petechiæ on the face and body; pulse 120, soft; tongue has a white fur; no motions since last evening; urine high-coloured.

Rep. enema.
Antim. tart. gr. $\frac{1}{4}$;
Ex aquæ ℥ss, *alternis horis*;
Hirud. xx *epigastrio.*

To-day the bowels are freely open, and the abdominal sensations relieved, but there is still troublesome cough, and pain in the chest, and there is an inflammatory crepitation in the left side of the chest, for which I have ordered *ten more leeches* to be applied; the pulse is to-day of a moderate strength—that is to say, it is not so weak, for hitherto it might have passed for a typhous pulse, in consequence of the oppression of the system from the extent of inflammation of the viscera, pleuro-peripneumony, and enteritis, complicated with the epidemic fever, of which the reddish-brown petechiæ which showed themselves yesterday are diagnostic. I say peripneumony, not catarrh, as there was crepitation not catarrhal rhonchus, and the costiveness was indicative of enteritis, not catarrh of the mucous membranes of the bowels. I consider her still very seriously ill, notwithstanding the diminution of symptoms; the most favourable circumstance I consider the rising of the pulse under the treatment of bleeding, antimony, and opening enemata.

I must call your attention once more to the analogy which this epidemic has with the exanthemata. This is the seventh or eighth case you have seen amongst my patients, independent of others which have occurred; and on referring to the journal, you will see that they have points of resemblance with measles, scarlatina, and variola—that is, fever, with eruption of spots, some sore throat, more or less cough, sometimes tenderness in the epigastrium, irregular sweating, and just as you have in the exanthemata, a good full synocha pulse, except when there is so much mischief in the viscera as to cause a sinking of the system, and these cases will encourage you to adopt antiphlogistic treatment in bad cases of the exanthemata.

REPLY TO SOME OBJECTIONS

OFFERED TO THE PRACTICE OF

VENOUS INJECTIONS IN CHOLERA.

To the Editor of The Lancet.

Sir,—It is certainly much to be lamented that we should have members in the profession guilty of scribbling on medical subjects in the newspapers of the day. This is a crime for which they have no excuse, for if they have aught of such importance to communicate to the profession, they have free access to a respectable medical press established for this purpose. Indeed no respectable practitioner can possibly be guilty of such empiricism, for, certainly, quackery it is, in the humblest sense of that degrading term. Why shall a man act so unprofessionally as to bring himself before the wide world in a matter of which so large a proportion of the community is ignorant—is utterly unqualified to judge? By doing so he ranks with the quacks of the day, for his only motive can be to acquire a little notice, or perhaps give vent to a little venomous spite.

The introduction of the injection of saline fluid into the veins, as a remedy in the collapsed stage of cholera, has afforded opportunity to some for such indulgence; and as attempts have been made by them to depreciate this powerful remedy, I crave that you will honour the following remarks with a place in your much-esteemed paper.

The first assault is made by Dr. Craigie of Leith, in the *Courant* of the 14th June, in a long letter on the nature and treatment of cholera; he not only attacks the practice, but what is equally unpardonable, makes an attempt to appropriate a discovery to which he is by no means entitled. At the outset he, by way of excusing himself for introducing his letter into the *Courant*, says, "The *Edinburgh Medical Journal* being only published quarterly, I deem this the best mode of communicating speedily and extensively a notion of what we have begun here." A notion of what? "Of the wonderful effects of injecting saline solutions into the veins in the stage of collapse." This is followed by a statement, that *the faculty are already in possession of this information,* his *cases and several documents by Drs. Latta and Lewins having been published in* The Lancet *and another medical Journal, to which for particulars he begs to refer.* Was the information the doctor wished to communicate, not already spread from the Dan to the Beersheba of the land, nay, even far beyond the bounds of the *Courant*'s circulation? Why then repeat such information in a daily newspaper? The reason is but too evident from what follows. After letting out some sagacious stuff of his own in the shape of theory of cholera, in opposition to "the doctrines held by all the writers, both Indian and European," he states, "As to the treatment, my attention has been particularly turned latterly to the various means of introducing into the circulating system, that is, through the medium of the absorbents, the saline fluid which is awanting in the blood. As the injection by the veins must in the nature of things be hazardous, as indeed we have already experienced here, in this I am happy to say I have succeeded in a remarkable degree." And what is the remarkable discovery? That a couple of spoonfuls of salt, dissolved in two tea-cupfulls of cold water, could, on being swallowed, remove the phenomena of collapse in cholera! In the case quoted, "reaction was fairly established in four or five minutes." The honour of such a discovery is worth having, and lest that should be diminished, the remedy having previously been used elsewhere, the candid doctor declares, "I am quite aware that salt and water was recommended before, both in India and Russia, but it was then distinctly given with a view to promote free vomiting and purging, and as soon as that was accomplished it was given up, whereas I hold the good effects in this, arose distinctly from the absorption of the fluids into the blood." Two teacupfuls absorbed in four or five minutes, and that sufficient to establish reaction fairly! Is not this discovery a direct emanation from Apollo himself, made out of respect to the erudite doctor, and of humanity to mankind? All insignificant as it is, it might have been entitled to some respect, as, in the present crisis, everything new connected with cholera is. Is it not a fact, that ever since I was aware of Dr. O'Shaughnessy's analysis of cholera blood, I have used the saline solution here by the mouth and rectum too? Is it not a fact that Dr. Craigie was aware of such practice, and often conversed with me on the subject? Nay, he must be aware that such practice has for some time been general every-where; but the editor of a newspaper is not expected to know so much of medical practice as to be aware of this; and though he did, he is entitled to insert any advertisement, no matter how absurd, if he is paid for it. As for the doctor's strictures on the venous saline injection, "which he considers hazardous, as indeed we have already experienced here," I will only state, that the safety and simplicity of the practice have the honourable testimony of the most scientific members of the profession here. It produces instant relief from a load of suffering, and in no case where I have used it, has it produced any bad consequences. In many instances it has prolonged life for days and even weeks, and has been the chief remedy in restoring a respectable proportion of cases to health, where every other means had utterly failed. No doubt the meanest operation in surgery may in some hands prove "hazardous," but where lies the censure? And so it is with venous injections, the operator may want discrimination when it is to be practised, and may even be destitute of the better tact necessary for the proper application of the apparatus, which may lead him to use unwarrantable freedom with the veins; but if it ever arises from such, where lies the censure?

But notwithstanding the excellence of this remedy when successfully applied, Dr. Craigie thinks "it must be subjected to important modifications and precautions which experience alone can point out. The saline injections into the veins were never set off as a specific in the treatment of cholera, but only as an indispensable auxiliary in bringing about reaction in the stage of collapse. In that disease some subtle agent seems to be in operation, the effects of which often cannot be counteracted by all the powers of the best-directed practice. The saline injection, though one of the most powerful remedies yet discovered, requires the prompt assistance of every other means in our power; the nature of these means will altogether depend on the notions we have formed of the various phenomena of the disease; these conceptions, like the doctor's, may be quite unscientific, unsupported by one fact in physiology, and may introduce modifications of practice inconsistent with the recovery of the sick. No doubt attempts have been made to modify the practice, and *warm water, and pure blood, and solutions of quinine, of morphia, and of albumen*, have been tried, but all such modifications have been laid aside, and the plan I originally introduced re-adopted; nor is it possible that such should be subjected to much modification, since according to analysis it is the remedy which nature herself points out. The blood has lost a quantity of its natural salts and water, and her operations are in consequence impeded, and is it natural to expect that her healthy action will be restored if that deficiency be supplied by anything different from whats he has lost? And as for the method of applying it, that must be either through the absorbents or directly by the veins. In very mild cases the important discovery of Dr. Craigie may be very useful, for if the power of absorption is not entirely gone, copious draughts of saline solution, and large enemata of the same, may by absorption prevent farther mischief till choleric influence has subsided. In extreme cases, however, when, in consequence of the peculiarities of the disease, intestinal absorption is arrested, and must in a great measure continue so long as the vena porta continues gorged with stagnant viscid blood,—so long as the return of venous blood from the intestines is prevented, not only must absorption be nearly at a stand, but every addition of water to the blood must be speedily thrown off by the bowels. In such cases I have found the saline solution administered by the intestines injurious. Thus if a substitute for the lost serum in the blood is absolutely necessary, how are we to accomplish this in severe cases, but by venous injection? Such I have practised with safety and advantage, and would recommend it to every brother who knows his profession, feeling confident that in its use he will not be disappointed.

Another newspaper scribbler contrives to get his paragraph inserted in the *Scotsman* of Wednesday the 21st inst., and quoting from a medical journal the sentiments of an Edinburgh physician, who has witnessed little of the practice, says—"The London practitioners are less sanguine and more alive to the possible dangers of the treatment. And why? Because the said physician says, great care should be taken that it be not precipitately adopted, as it carries with it a few sources of danger, and produces pneumonia, of which the patients may die, even if the remedy should enable them to struggle through the immediate effects of the disease." This is not true. In all the cases in which I have tried it, it has been after all the ordinary means had failed, and I have had to blame myself for trusting too long to such inefficient means. Every injected case has been a desperate one, and far advanced, and without the use of venous injection would have inevitably died, and so added to the fearful list of mortality; but though they were cases of such a hopeless cast, yet one-third of my hospital patients have been restored to the world. Notwithstanding the anonymous writer in the *Scotsman* says the average mortality of cholera will be but little affected by it, he seems versant in every circumstance connected with our hospital practice, but like the rest of his tribe, notices only those which are calculated to throw discredit on venous injection, and he delivers his sentiments in such a form as does not tend to show forth the truth, but only to gratify his own malevolence. "In the Leith Hospital," says he, "it has been tried in 22 cases, and only four survive. Of these, one is discharged well, one is in a dangerous state of dropsy, and the other two are progressing slowly to amendment. In my observations on such a piece of malice, I will only notice that in which I am personally interested,—my own portion of these patients. Nine of these were treated by myself; and be it remembered I had no precedent for my guide, and every case was an extreme one. Most of them were extensively diseased, and in some the pulse was gone, even in the axilla. Two of these were actually moribund, yet, notwithstanding, three have recovered, three that would have been in their graves long ago, if trusted to the effects of ordinary practice. Of these, one recovered in four days, and had no consecutive fever; the other two went through such fever, but it was comparatively slight, and they were dismissed cured a considerable time ago. Yet our informant says, "they are progressing slowly to amendment." Honest, honest Iago! By the way, in his hurry to depreciate the practice, he seems to have given the number of fatal cases, so as to include one under treatment by Dr. Lewins which has *unfortunately* recovered, and that too without consecutive fever. Now, Sir, say if by such practice the average mortality of cholera will be but little affected. That it will most certainly be decidedly lessened, is sufficiently evident if we contrast with it the ordinary practice of the day. In our hospital, including cases of cholera of every description, some having only the premonitory symptoms, only one-third have survived. Scarcely one has recovered from a pulseless state. Of the 22 injected cases noticed in the *Scotsman*, very nearly the whole were subjected to ordinary treatment, and were injected not till the latest particle of life's sand was about to drop, and yet these are set down as failures of the remedy!

I have seen cases injected with admirable effect, and yet have been lost from want of the vigorous reapplication of the remedy, while some have failed from the fluid having been too sparingly applied. I am, Sir, your most obedient servant,

THOMAS LATTA, M.D.

Leith, June 27, 1832.

SECOND LETTER ON

M. HEURTELOUP'S PERCUSSION INSTRUMENT,

ADDRESSED BY MR. COSTELLO

To the PRESIDENT *of the* ACADEMY *of* SCIENCES *of* PARIS.

SIR,—I see in a journal (*the Temps*), which gives an account of the sitting of the Academy of the 4th inst., that I am accused of having given an unfaithful relation of an operation of lithotrity performed by M. Heurteloup, by means of an instrument of which he styles himself the inventor, and which he designates by the name of the *curved percuteur and hammer.*

This accusation is sustained by a certificate from Messrs. Hume and Brodie. This I consider to be a serious charge, for no person can entertain a greater respect than myself for anything that emanates from those justly respected and eminent individuals. This certificate states, *that in the case* of Colonel Rankin, "*there was by no means a question of extracting from the bladder of this patient a broken instrument; that merely the branches of the percussion instrument had been separated to the extent of about three lines, and could not be closed.*" I confess, Sir, I cannot comprehend why the reproach for infidelity in my account of the operation should be addressed to me, for I distinctly stated the account was not mine, and I did not make mention of a broken instrument. Now it is obvious, that to merit such an accusation, a contradiction must exist between the certificate of these gentlemen and my relation. Such a contradiction will be sought for in vain in my letter to the Academy!!! I have stated precisely what Messrs. Brodie and Hume have certified. I did not state that the instrument had been broken in the bladder, but that it had been bent to such a degree that it could not be withdrawn without making an incision in perineum. It is of little consequence that the derangement was but to the extent of three lines only. It will be recollected that this instrument is of such a size as to fill up the urethra completely, and consequently that the necessity of withdrawing it by an incision in the perineum, was as great for a deviation of three lines, as if it had bent to the extent of an inch.

So much for the principal fact of the certificate. Now, allow me to add, that I cannot divine the object for which it has been invoked. Assuredly M. Heurteloup cannot have any wish to conceal from the Academy, notwithstanding his candidateship for a prize, that which every one knows here. In the account which I had the honour to transmit to you, I confined myself strictly to what had been told me by a highly scientific and honourable man. If I had allowed myself to be led away by mere rumour or hearsay accounts, I should also have spoken of instruments broken in the bladder. But I did not do so. I did not attribute the death of the patient to the instrumentation to which he was subjected in the hands of M. Heurteloup. I merely stated the facts, without interpreting them. All I said was, that the loss of blood was very great, that the patient had been a long time on the table, and that he subsequently died, and all this is true. I repeat and confirm it on the authority of the celebrated surgeon who had been called in consultation on this case, a man whose name is European, and whose talents as a surgeon and physiologist are universally recognised. I did not criticise the conduct of, nor impute anything in blame personally to, M. Heurteloup on this occasion. I merely spoke of an instrument which he considers a good one. I expressed a contrary opinion, and I gave my reasons for that opinion. I carefully drew a distinction between the individual and the thing, and of the former I spoke with respect. Of what use then was the certificate? The fact subsists, and as I stated it. If any one had a right to complain, it was I, since my confrère assured you, that he had operated with success with the percussion instrument on two patients on whom I had made fruitless attempts, a statement which was incorrect, as I have proved it to be.

In my former letter, I asserted that this instrument was not new, and I quoted in support of my assertion, the name of Dr. Fisher. Another proof that this instrument was not invented by M. Heurteloup, has since come to my knowledge. Weiss, a cutler of London, made this instrument in 1824, for breaking up calculi in the bladder, for Mr. Fletcher of Glamorganshire. He has shown me the instrument which he then made, and the only difference which exists between the two instruments is, that Weiss's acts by means of a screw, which advances the anterior branch gradually on the foreign body, this movement being effected by turning the manual portion of the instrument on its axis, while that of M. Heurteloup acts by a process more violent and less safe,—that of percussion. In the former, namely, pressure gradually exerted, if the calculus does not yield, the operator may, without having incurred any risk, release it from his grasp and withdraw the instrument, the circumstances, as far as the patient is concerned, being the same as before. In the latter, namely, percussion, the operator has no choice; something must yield, either the stone or the instrument. The result no longer depends on his will, for while he fancies the stone is yielding, it may be, that the branch of the instrument is merely bending. The case of Colonel Rankin is a proof of this.

Which of the two would a prudent man prefer,—that which in the case of non-success would not render the circumstances of the case worse, or that which would risk all for all, while he could avail himself of an alternative, the safety and efficacy of which were confirmed by oft and oft repeated experience?

The curved instrument of the Danish surgeon Jacobson, composed of two branches, which acts by pressure, is in my opinion preferable to the percussion instrument, and yet this instrument is scarcely applicable, but to small and friable stones.

The manufacturer Weiss has assured me, that M. Heurteloup had copied his instrument, and that he acknowledged to him that he had done so. Has M. Heurteloup been equally candid with the Academy?

If M. Heurteloup can lay claim to any merit in this matter, it can be only that of having proposed percussion with a hammer, instead of pressure with a screw. If it be a merit (and that is the entire question), I shall grant it to him willingly. What matters it to me, whether it be M. Heurteloup or M. Civiale, or any other person who is the author of an operative process, which really possesses advantages over that which has already been so usefully and so extensively employed? I can use it surely as well as they. All I desire is, that these advantages shall be real and well-established, not chimerical and full of risk, and that at each announcement of a new instrument, or a new process, we may not have to deplore some new experimental mishap produced by this mania of innovation. Lithotrity encounters sufficient resistance, not to augment the force of it by misfortunes, and it is only because I feel a proper anxiety to protect it against such occurrences, that I take part in this discussion. I entertain no feeling of hostility towards M. Heurteloup personally. If M. Civiale had proposed this process, I should have made the same objections to it; nay, more, if my own brother had been the author of it, I should have acted in the same manner, for it is the duty of every conscientious surgeon to oppose the personal interests, even of his dearest friends, whenever he believes them to be contrary to the interests of humanity and science. Knowledge produces good actions, and I respect it as earnestly as I abhor empty disputes.

M. Heurteloup perhaps imagines that we are not on an equal footing. He calls himself the inventor of many things, while he is pleased to qualify me merely as *a surgeon who undertakes this branch of surgery*. Does he not know, or does he wish not to know, that during the last six years I have made in the lithotritic apparatus useful modifications, some of which will remain in the science when others more vaunted will have been forgotten? If I have said little on the subject of these modifications, it is because first of all, I desired to be myself convinced of their importance, and that too by experience, before I made any effort for the sake of mere empty fame, to impose that belief on others whose conviction could be obtained by words.

Being myself animated with the desire of doing good, I also followed with interest the labours of others, even of persons not connected with our profession, consequently I could not overlook those of M. Heurteloup. I do not wish to say anything disobliging to him, but I am compelled to remark, despite of all the fine promises of improvements in the apparatus, or ameliorations in the work of lithotrity, that it is still in the same position in which M. Civiale has placed it. It is an error on his part to suppose that he is the only person who has aimed at improving. I belong also to the *party of the movement*, and as soon as any amelioration shall really be established, I shall applaud it as readily coming from him as from any one else.

I persist in my objections against this instrument. They are, 1st, the difficulty of properly seizing and fixing the stone; 2nd, if the stone be placed obliquely with respect to the branches, the anterior moveable branch is liable to become deviated; 3rd, even supposing the stone to be properly placed between the branches, the anterior branch against which the maximum of the force is directed is liable to be broken or bent by the shocks of the hammer, the occurrence which took place in the case of Colonel Rankin; 4th, the stone or its fragments are made, by the shocks of the hammer, to fly from the branches of the instrument with a degree of violence which may often dangerously contuse or wound the bladder.

My letter was already too long, perhaps, when it occurred to me, that as a certificate had been forwarded to the Academy for M. Heurteloup, your learned assembly might now require some further attestation than my own words in support of what I have stated. In consequence, I waited this morning (13th inst.) on Sir Charles Bell, the surgeon, to whom allusion is made in the beginning of this letter. I read to him my first letter to the Academy. I recalled the conversation which I had with him concerning this patient. After which he observed, "Your account is a correct one; what I said on this subject, was uttered in conversation with yourself, and not in opposition to Mr. Brodie. This," said Sir Charles, "should be stated to the Academy." He then added, "Let me recapitulate the circumstances. It was presumed that something had gone wrong when it was found that the instrument could not be withdrawn. It was, however, drawn into the prostate; but it was impossible to carry it through the bulb of the urethra. In that situation M. Heurteloup cut down on the instrument. The artery of the bulb was divided, and bled profusely; the branches of the instrument were made to project through the wound; they were then pressed together by a strong forceps and withdrawn through the urethra; after which Mr. Brodie proceeded to perform the lateral operation, and extracted the stone. The loss of blood and the shock of two operations were more than the patient could bear. The symptoms which preceded his death, were those of extreme debility. In a word, he died in the same way as we see persons who have lost a great quantity of blood. His death took place, as well as I am able to recollect, about the fourth week after the operation." This is the statement which Sir Charles Bell made to me this morning. I have endeavoured to give it as nearly as I could in the very terms he employed. He has given me full permission to support the statement I made to the Academy, with all the authority of his name; and I do it with the greatest satisfaction on this account, that his honourable character and brilliant reputation are known to you, and that what he states does not clash with the certificate of those honourable men which M. Heurteloup has caused to be presented to you.

I am not now in the habit of writing French; this, I hope, will be an excuse for any obscurity in my language.

I have the honour to be,
Monsieur le President,
Yours, &c.
William B. Costello.

Westminster, No. 7, Parliament-street.
June 15, 1832.

THE LANCET.

London, Saturday, July 7th, 1832.

Our contemporaries of the morning and evening press have, by their injudicious notices, swelled the late trials in the Common Pleas into affairs of prodigious importance. Calculating upon correct data, their conclusions are not likely to prove of utility to the public. When it is considered that opinions given by the press are expected to be cautiously weighed, it is to be regretted that fallacious grounds are seized hold of in order to found upon them doctrines which bear a general reference to proceedings in our courts of judicature. "Actions," say some of our contemporaries —"having been brought against The Lancet, and also against Ryan, Renshaw, and Rush, for the publication of the *same libel*. We have an exemplification of the excellence and consistence of the Trial by Jury in the award of *one farthing* damages in the one case, and *four hundred* pounds in the other, the smallest coin in the realm being the estimate of the amount of injury in one instance, whilst a little fortune is required to compensate for the amount of damage inflicted for the secondary publication of the same libel." Such is the specious trash with which public writers attempt to poison the public ear, in directing their covertly-malignant attacks against the noblest, the purest institution of the country. It would be beneath their dignity, and far beside the usual course of their duties, to stop to inquire whether there are not differences in the facts of the two cases to justify the, at first sight, apparent differences in the verdicts. Oh, no! a first glance is quite sufficient, if it present any unfavourable hue to urge on a certain class of critics in making their thrusts against Trial by Jury. Affecting to admire the press—affecting to regard the press as the greatest bulwark of the liberty of the subject,—avowedly looking upon the press as the grand shield of public morals, these short-sighted, perverse-thinking, or corrupt-hearted commentators, strike at the very existence of an independent press, when they throw out an insinuation only against the capability, the integrity, the purity, of Trial by Jury.

Proceed we, then, to ascertain whether the jury which awarded a farthing damage in the action brought against The Lancet by Francis Ramadge, estimated the injury at too low a rate; and whether the jury which estimated the damages incurred by him at four hundred pounds, made their assessment at too high a rate. At least, let us inquire whether there were not circumstances which justified the two juries in deciding upon two very opposite awards. First, however, let us premise, that we resume the subject on this occasion, in consequence of numerous inquiries from many respectable correspondents, and that the character of the observations elicited by the trials may be comprehended by our more distant readers, we select the following from a large number of communications on the same subject from correspondents in or near London.

"*To the Editor of* The Lancet.

"Sir,—In your Lancet, published this week, I expected to have found a more full account of the trial that took place in the Court of Common Pleas on Monday last, and I do hope that you will publish as full an account as from the circumstances you are able. The arguments were not very long, nor was the evidence; and I am sure your defence, for its ability and manliness would be acceptable to every reader of your valuable Journal. Your arguments on objections taken to your cross-examination relative to the character of the plaintiff, and your defence as bearing upon that character, ought not to be lost sight of. I have no doubt how it was that you had a verdict against you for a farthing, and the defendant in the second action one for 400*l.* * * * If no perfect report of your speech be printed, nobody will or can understand the verdict in your case, and many may think the latter verdict to be correct and just.

"AN EYE-WITNESS."

As to publishing a report of the trial, that is absolutely impossible, for we did not consider the cause of sufficient importance to require the attendance of a shorthand writer; besides, we are not disposed, in the present state of the law of libel, to bestow upon RAMADGE and JOHN LONG those epithets and terms of reproach which we thought it our *duty* to apply to them in a court of justice. However, we will now touch upon two of the most important of the points of law which were raised during the argument, and then proceed, as briefly as possible, to explain still more distinctly than in our last Number, the cause of the difference in the two verdicts. Hence, then, it becomes necessary to copy the first two paragraphs of the plaintiff's "declaration," a portion of the third, and the whole of the paragraph with which the document is made to conclude. We find, then—

"That plaintiff, long before and at the times of the committing of the grievances by said defendant, hereinafter mentioned, was, and from thence hitherto hath been, and *still is, a physician*; and during all that time the plaintiff exercised and carried on, and still doth exercise and carry on, the profession of a physician, with *skill, care,* and *integrity*; and the plaintiff hath always *conducted himself in his said profession*, and otherwise, in an *upright, honourable, and proper manner,* to wit in the county aforesaid, by means of which premises the plaintiff, before, and until the times of the committing of the grievances hereinafter mentioned, *had deservedly obtained* the good opinion and esteem of the public, *and enjoyed great credit and reputation in his said profession, and was much consulted and employed therein,* and by means thereof had acquired, and was then *daily obtaining*, great gains, profit, and advantages, in his said profession, to the comfortable support of himself and his family, to wit in the county aforesaid.

"That before the committing of the grievances by the said defendant hereinafter mentioned, to wit on the 17th of July in the year 1831, and on divers days then following in the county aforesaid, said plaintiff in the way of his said profession had been required to attend, and had attended, one Emma Bullock, who was then and there afflicted with a *certain malady* and illness, for the purpose, and in order, that the said plaintiff might give his advice in that behalf, and might endeavour to heal and cure the said Emma Bullock in regard to such malady and illness, and upon that occasion said plaintiff as such physician gave certain *proper advice and directions* with respect to such malady and illness, and the treatment thereof, and behaved and conducted himself in that behalf, and in his treatment of such malady and illness of said Emma Bullock, in a *skilful, careful, proper,* and attentive manner, to wit in the county aforesaid.

"That the treatment and conduct falsely alleged and pretended, in the libel in this court after mentioned, to have been pursued and observed by or under the advice and directions of the said plaintiff, in his said profession, towards the said Emma Bullock, in regard to the malady and illness with which it is in such libel alleged that said Emma Bullock was afflicted when attended by plaintiff as aforesaid, would, if adopted, have been *grossly injudicious, unskilful,* and *culpable,* and *manifestly improper* in that behalf, &c. &c.

* * * *

"By means of the committing of which several grievances by the defendant as aforesaid, the plaintiff hath been and is greatly injured in his said good name, *fame, reputation,* and *credit,* and brought into public scandal, infamy, and disgrace, insomuch that divers persons, neighbours, and subjects, have, on account of the committing of said grievances by defendant as aforesaid, from thence hitherto suspected and believed, and still do suspect and believe, plaintiff to have been and to be, a person guilty of the offences and misconduct so as aforesaid charged upon and imputed to him by defendant, and to have acted in a dishonourable and improper manner, and to have been, and to be, unfit, and unworthy to be consulted and employed in his said profession, and have, by reason of the committing of the said grievances by the defendant as aforesaid, from thence hitherto wholly refused and still do refuse to consult or employ, or have any transaction or discourse with, the plaintiff in his aforesaid profession, or otherwise as they otherwise would have had, and the plaintiff is, by means of the premises, damnified, to wit, &c. Breach to plaintiff's damage—*Eight Hundred Pounds.*"

In consequence of these averments, which Mr. WAKLEY contended were not technical but ought wholly to be proved, he questioned the witness, in cross-examination, respecting the professional *character* of the plaintiff, as founded upon his supposed connexion with JOHN LONG. He inquired of the witness what he himself had at various times said of the conduct of the plaintiff in that behalf, to which questions the plaintiff's counsel offered the most decided objections, which were allowed by the judge—the Court deciding that the character of the plaintiff was not the question at issue, that his "skill, care, integrity, upright, honourable, and proper manner and attention," were not averments requiring proof unless a plea of justification had been put upon the record by the defendant! Mr. WAKLEY, on the other hand, contended, that if it were not necessary to prove these averments, it was not necessary to prove any of them, and that, in a word, the cause might be tried without any witness whatever; that if it were necessary to prove to the jury that the plaintiff had "attended" Miss EMMA BULLOCK, so was it required of him to prove that he had given her "proper advice and directions," and that if it were essential for him to call a former registrar of the College of Physicians to prove that he obtained his diploma and "still is a physician," so was it required of him by his pleas to satisfy the jury that he "had always conducted himself in his profession in an upright, honourable, and proper manner, and had deservedly obtained the good opinion and esteem of the public, and enjoyed great credit and reputation in his said profession,"—the very words of his averments in the declaration. The defendant cited cases bearing upon these points, and instanced a point analogous to this:—That supposing a horse to have been introduced in place of the word "reputation," and, according to the doctrine of the Court, it had been pleaded by the plaintiff that he possessed a useful animal of that species, value sixty pounds, and that the defendant had wilfully and maliciously on a certain day broken the creature's leg, it would not be necessary—if he understood the opinion of the Court—for the plaintiff to prove the *possession* of the horse, the *value* of the animal, or the damage done to it by the alleged accident; but that the jury might be called upon to award damages when no injury had been inflicted, and to give compensation for the fractured leg of an animal which might have owed its existence to the fancy of the plaintiff or the ingenuity of his attorney. Because, if it were not necessary for the plaintiff to prove that he possessed a reputation when he sought for damages for its alleged injury, it could not be necessary, on the other hand, that he should prove that he possessed a horse in order to derive compensation for his broken leg. However, having pushed the cross-examination to the witness's own conversations with Dr. RAMADGE, in relation to the publishing of the letter in favour of LONG, Mr. BRADFORD stated, without hesitation, that he had told Dr. RAMADGE that he had disgraced himself, and that he despised his conduct as much as any one; and here, of course, the defendant's inquiries terminated.

The remaining important point bearing upon the question of evidence, arose from the circumstance of Mrs. REYNOLDS, the sister of the deceased having been examined under a new act of parliament before the prothonotary. The statute is entitled "An Act to enable courts of law to order the examination of witnesses upon interrogatories and otherwise." It was framed by Lord Tenterden, and passed the legislature under the auspices of that profound and upright judge. The act is simple in its construction, clear in its provisions, and bears the strongest signs of the just intentions of an experienced judge. The debated point was this. The witness having been examined by the plaintiff before the prothonotary, the defendant contended, that he had a right to make use of the evidence which on that occasion he had elicited in cross-examination. The demand was not allowed by the Judge, and Mr. Serjeant Wilde remarked—on Mr. Wakley's having alleged that it opened a wide door to abuse,—that defendant could himself, if he pleased, put in the cross-examination as his own evidence. This, however, did not suit the object of the defendant, who still insisted upon his right to read the cross-examination to the jury, and observed that his capability to put in the evidence was no answer to the obvious meaning of the Act of Parliament, wherein was not to be found a single word which required the plaintiff or defendant to give notice to the opposite party, in the event of examining a witness before the prothonotary. Both these points having been urged with some warmth of feeling, if not with force of argument, must have had some weight with the sound understandings of the Jury, although they appeared to be wholly lost when directed against the fortification of technicalities which seemed to render the court wholly impregnable to the batteries of common sense.

Having weighed all these circumstances, —having observed with what scrupulous care the learned counsel, employed for the plaintiff, endeavoured to screen the "reputation" of their client from all analytical or catechetical inquiry, the Jury were compelled, if they gave any damages at all, to award *one farthing*. The propriety of that verdict we will not here dispute; the professional "fracas" occurred in a private room, and twelve English gentlemen may have questioned—and wisely questioned—the propriety of rendering it an affair of public notoriety. Certainly the alleged libel itself, treating as it did of an occurrence which took place in a private dwelling, never would have found a place in the pages of The Lancet had it not appeared to us, that Dr. Tweedie's refusal to meet Dr. Ramadge in consultation, furnished a practical public commentary on the professional conduct of the latter in relation to the slaughterer, John Long. If the gentlemen of the jury thought that the letter itself ought not to have been published—that it was in itself essentially libellous, inasmuch as it was calculated to spread more widely those angry feelings which had been displayed in a private apartment, we will not now dispute with them the propriety of their decision, fully believing, as we do, that a more prudent, a more sacredly just verdict, was never delivered in a court of justice. "But," it will be asked, "if the award of *one farthing* be just, as against The Lancet, what do you say of the *four hundred pounds* given against Messrs. Ryan, Renshaw, and Rush, for the publication of the same libel?" We have already stated our conviction, that the damages were excessive; still, many of the circumstances were presented to the two juries in such really opposite colours, that a similarity of decision in the two cases could not have been expected. The most striking discrepancies were but two or three in number, they are too long to be explained in this article; we, therefore, shall resume the subject next week, and explain *one* cause of misfortune to Dr. Ryan which has not yet been noticed. Of that misfortune, however, it will be seen that Dr. Ryan stands the undisputed author.

It will be seen, from an advertisement on the cover of this week's LANCET, that a public meeting is about to be held at WORCESTER, for the purpose of taking into consideration the establishment of a projected medical association in that city. The objects in view are, we understand—

1st. The collection of useful information, whether speculative or practical, through original essays or reports of provincial hospitals, infirmaries, or dispensaries, or of private practice.

2nd. Increase of knowledge of the medical topography of England, through statistical, meteorological, geological, and botanical inquiries.

3rd. Investigation of the modifications of endemic and epidemic diseases, in different situations and at various periods, thus to trace, so far as the present imperfect state of the art will permit, their connexions with peculiarities of soil or climate, or with the localities, habits, or occupations of the people.

4th. Advancement of medico-legal science, through succinct reports of whatever cases may occur in the provincial courts of judicature.

5th. Maintenance of the honour and respectability of the profession, generally, in the provinces, by promoting the friendly intercourse and free communication of its members.

The scheme is masculine and comprehensive. It lays a foundation for the congenial union of scientific research and professional honour, and addressed as it is to provincial practitioners—not by any means the least independent or least skilful members of the profession, who can doubt of its success, or, observing it, can fail to desire that its benevolent and intellectual promoters may live to witness this excellent example taken up and followed throughout the whole of the British dominions?

WE concluded our recent observations on the *saline* treatment of cholera with the firm and decided conviction, that the friendly and reasonable advice offered by us to the gentlemen engaged in this practice, would forthwith produce the effect which we described as so desirable to the public, and so essential to the reputation of the parties by whom it is recommended. We scarcely believed that a day would be suffered to pass, ere the application for a commission to scrutinize into, and report on, the alleged merits of Dr. STEVENS' practice, was made to the proper authorities. But we have been deceived in this expectation. No application has been made, and we have reason to believe that no application will be made, on the subject.

Meanwhile, as we predicted, professional scepticism and displeasure, or, in the language of Dr. STEVENS' friends, slander, envy, and detraction, are busily concerned in the discussion of the character of his statements. It is openly stated, that no serious cases of cholera have ever been successfully treated by this method. It is further maintained, that many cases have been reported as cholera by Dr. STEVENS and his assistants, although the patients did not present a single symptom of that disease. The subjoined correspondence, extracted from the public journals, relative to an alleged irruption of the epidemic in the Coldbath-Fields prison, may serve to illustrate the nature of these various reports and insinuations:—

"CHOLERA MORBUS IN THE HOUSE OF CORRECTION, COLDBATH-FIELDS.—The circular, of which the following is a copy, was on Thursday sent to the several police-offices in the metropolis:—

"'Gentlemen,—The visiting justices assembled this day at the House of Correction, consider it to be their duty to inform you, that the cholera morbus prevails at this time to a very great extent in this prison.—I am, gentlemen, your most obedient servant,

"'THOS. STIRLING,
"'Clerk to the visiting magistrates.
"'House of Correction, Coldbath-fields,
June 28.'"

"Seventeen prisoners, it is said, died in the course of Thursday, in the prison."—*Morning Paper.*

Our readers will be a little perplexed to reconcile this terrible manifesto, with the annexed official commentary which it forthwith elicited from the Central Board of Health.

"A letter having appeared in this paper on the 30th ult., signed "Thomas Stirling," addressed to the magistrates, police-office, Worship-street, notifying cholera to great extent in Coldbath-fields prison, we have authority from the Central Board of Health to state, that there were not more than three cases of decided cholera in that establishment on Friday last, and that from the official reports received at the Council-office, they trust there is no serious ground for alarm as to the supposed increase of the disease in London to any material extent. —July 1."—*Morning Paper.*

Such a strange and extraordinary discrepancy on a subject of such vital importance, imposed on us the duty of making as minute inquiries into the actual facts as our opportunities afforded us, and we have succeeded in obtaining from a gentleman who visited the prison at the time alluded to, the annexed statement, on the truth of which our readers may implicitly rely.

Our informant, who has, we believe, always entertained a high opinion of Dr. Stevens' integrity and professional skill, states, that on his visit to the prison there were upwards of fifty patients who were *reported* as labouring under cholera, and who were confined to their beds;—that of all these patients not one was pulseless or livid;—that during his visit there was no vomiting or purging in the wards;—that no patient was cramped, or had the choleric voice, or made the usual distressing complaints which are peculiar to this awful malady. He adds, that he did notice two patients who had chilly, but not cold, tongues;—that one of these suffered considerably from jactitation;—that both were externally cool and moist;—that the features of each of the two were decidedly choleric, but that the pulse was perfectly distinct.

Such was the clinical state of the infirmary as regards the genuine or primary period of the cholera at two p. m., on the day preceding the date of Mr. T. Stirling's letter. Our informant further affirms, that he saw no case which he could recognise as one of decided secondary fever.

The summary of our information, therefore, amounts to this, that at the period of the visit there were but two, or at most three, recognisable cases of cholera in the prison, and that even these were not of a malignant description. The visit too was made on a day in which many "new cases" had been reported. Convalescence could therefore be scarcely assigned as the cause of the trivial character of the symptoms then observed. It is, consequently, evident, that, even according to the assumption most favourable to Dr. Stevens, the great majority of the crowd of patients reported in the prison books to be affected with cholera were, at most, labouring only under slight and dubious *premonitory* symptoms of that disease—symptoms certainly insufficient to warrant the magistrates to terrify the public, and endanger commercial interests, by such a proclamation—and symptoms equally insufficient to entitle the medical attendants to claim the least degree of credit for their alleviation or cure. How indeed, we may ask, can the cure of *premonitory* symptoms in any disease be relied upon as the effect of the remedy employed, when it is utterly impossible to say that these symptoms would ever have arrived at a serious pitch of intensity?

We do not wish to be considered, by the preceding observations, as having entered upon the discussion of Dr. Stevens' theories, or on the nature of the facts on which he founds them. Neither do we wish, in the most minute degree, to question his ability, his sincerity, or his wish, to do good to his suffering fellow men. We speak but of the facts involved in the sanitary condition of the Cold-Bath-Fields Prison, and it would be equally irrational and unfair to presume that Dr. STEVENS' cholera patients elsewhere, were only affected with an equally tractable degree of indisposition. The facts do not entitle us to draw such an inference. The question therefore still remains open for discussion—Can Dr. STEVENS, as he affirms, successfully treat the malignant cholera in its various stages in every case not advanced beyond the possibility of rendering efficient medical assistance? The profession must remember that the assertion of the physician immediately interested in this question, is not that of an illiterate quack, or a scheming pretender. It comes from a gentleman of many acquirements and good repute. His statements therefore imperatively demand the rigid investigation which we have prescribed for them. If he possess so certain a remedy, our fellow-countrymen must not, on the one hand, be permitted to perish through a want of confidence in its propounder; and, on the other, if Dr. STEVENS be deceiving himself, and leading others also astray, it is time that something was done to check the delusion, and direct professional zeal towards the elucidation of some more promising mode of treatment.

We do not hesitate, therefore, strongly to recommend, that a petition be forthwith presented from the medical profession to the Lords of the Privy Council, entreating them for the interests of humanity and science to appoint a medical commission to inquire minutely into the merits of Dr. STEVENS' practice. If this measure be not adopted, we fear no future successes of Dr. STEVENS and his assistants will be sufficient to obviate the impression which the affair of the Cold-Bath-Fields Prison has, unfortunately, produced on the public mind.

SINGULAR OCCURRENCE OF

CHOLERA AMONG POULTRY

AT CHOISI-LE-ROI.

THE following account of an irruption of well-marked cholera amongst the poultry at Choisi-le-Roi, is well deserving the attention of all observers of epidemic diseases. Our readers are, doubtless, aware, that vague and obscure anecdotes of similar events have been frequently related during the progress of cholera, from Astrakhan to the British Islands. We have alluded to some alleged occurrences of this description in our article of the 19th November, 1831, a reference to which will sufficiently show the great importance of such events in enabling us to approach a rational decision on the questions relative to the mode of propagation of epidemics. It will also be seen on consulting that number of THE LANCET, that up to the time of its publication, no well-authenticated or conclusive record existed of the occurrence of *cholera* in the inferior tribes of animals. It is true that enormous quantities of fish had suddenly perished in the ponds at Marienverder in Prussia, while cholera raged in that district; but the almost insuperable difficulty of obtaining the evidence of symptoms, or pathology, in the diseases of fishes, was sufficient to prevent the deduction of any important inference from the occurrence. We have now, however, before us, ample and well-authenticated details of a true cholera epizootic. The symptoms are as marked as in the human subject; the pathology is as closely similar as the difference of physical configuration between man and the gallinacea permits. The event, we repeat, is deeply interesting, and it is fortunate that it has met with so well-qualified an historian as M. CARRERE.

*On the Epizootic Disease at Choisi-le-Roi.**
by M. CARRERE, *late Interne des Hôpitaux.*

During the disastrous progress of cholera in Paris, the village of Choisi-le-Roi, while perfectly free from the epidemic, was the scene of an epizootic disease, of which domestic poultry were the only victims,

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* Choisi is situated about five miles from Paris on the banks of the Seine. It is considered a remarkably healthy village.

In the history of many other epidemics we find coincidences of this description of peculiar diseases affecting the lower animals, while pestilences were decimating mankind. Sometimes horned cattle, at other times horses, have been especially attacked; but there have not been recorded more than two or three examples of epizootics among birds. Chabert and Boronio have, it is true, described some diseases of birds, observed in France and in Lombardy, but the characters of the affections they describe are totally different from those observed at Choisi.

The cholera had scarcely appeared at Paris, when it was generally reported that a disease of most destructive mortality was raging among the poultry throughout the commune. Here, as at Paris, the cry of "poisoning" was loudly made; all persons who were persuaded that the food and drink of mankind were mixed with poison, found no difficulty in convincing themselves that similar villany was practised in the poultry yards. But the mortality soon reached such a pitch, that this idea was abandoned, and then it was generally reported, that the cholera was the cause of the epizootic.

Wishing to arrive at the source of these rumours, I learned, that since the 3rd of April, a vast number of fowls had perished in several houses situated in different quarters of the hamlet. During the first days of the disease, the number of deaths had been very considerable, after which period the birds were killed by the owners on the occurrence of the first symptoms. In one fowl-yard, of eighty cases, one or two alone recovered. Many remedies, amongst others bleeding under the wings, had been in vain resorted to. *A considerable quantity of the diseased fowl had been eaten by the inhabitants without any bad effect.*

The causes of this malady appear to me altogether unknown, and I saw no reason for supposing it to be contagious. Nevertheless, when a single death occurred in a fowl-yard, the mortality only ceased when it had no more victims to destroy. The most cleanly poultry feeders suffered as severely as the most filthy. The kind of food had no influence on the disease. The fowls at large in the streets of Choisi were attacked with equal severity with those perpetually confined or occupied in incubation. *Rabbits, geese, and ducks, however, lived with impunity in the same yards where the hens were universally perishing, and three turkeys only were affected.*

The disease, generally speaking, commenced in the morning. The hens were noticed to be dull and weak, their wings drooping, and their crops distended with undigested food. In a few cases the disease commenced during the day, and lasted four-and-twenty hours. The respiration was short and hurried, the motions of the heart accelerated, and diminished in force in proportion to their increase in velocity. In almost every instance there had been numerous whitish liquid dejections. The gullet was distended with thready mucus, which escaped from the beak. The combs were of a livid red colour, and the tint deepened to a violet as death drew near. After the disease had lasted from two to five hours, convulsions usually finished the sufferings of the animal, and death was rapid in proportion to the quantity of the evacuations. In many cases I have learned, that the coldness of the sick birds was very remarkable. A few recoveries were noticed towards the termination of the epizootic. As far as I can find out, about 500 fowls died of the disease, or were killed in consequence of the development of its symptoms.

After death the colour of the skin was the same as in fowls strangled without being bled. The bodies were warm for at least three hours, and the cadaveric rigidity was very remarkable.

I have taken much pains in seeking for any pathological alterations which might explain the cause of the disease, but my research was quite in vain. The brain was white, and free from congestion. The heart was bloodless, and of its usual consistence. The aorta contained fluid blood. The lungs were rosy and crepitating. The mucous membrane of the œsophagus frequently showed little papillæ, surmounted here and there with a white point, like a minute grain of sand, adhering to the centre of the papilla. The crop always contained food; the gizzard was strongly contracted; the intestine presented occasional reddish patches, especially in the situations where little parcels of worms were found. The liver was gorged with black and tarry blood; the gall-bladder distended with thick green bile.

This epizootic is quite different from the "*maladie charbonneuse*" of Chabert, and from that described by Boronio. Neither has it any analogy to the "pip," for the tongue was always in a natural state.—*Journal Hebdomadaire.*

DISCOVERY OF

AIR IN THE HEART

AFTER VENOUS INJECTION.

To the Editor of THE LANCET.

SIR,—The interest excited by the experiments performed in London, of the introduction of saline substances in the fluid

state into the blood of patients labouring under cholera, has not been lessened by the result of the trials recently [made by Dr. Venables, and other practitioners, without success. The post-mortem appearances in two individuals treated in this manner under the superintendence of Dr. Venables, have been fully detailed in the last number of THE LANCET. In both, *air* was found in some quantity in the cavities of the heart. This circumstance has been supposed by many to account in a satisfactory manner for the patient's death, but I have been assured by Dr. Venables, (and I can place full reliance on his capacity for conducting the operation, and the veracity of his report,) that every care was taken to exclude the access of atmospheric air, not a particle of which was introduced in the course of the operation. That, however, air may be generated spontaneously in the heart, was proved to me yesterday while examining the body of a cholera patient. Mrs. R., the wife of a man in poor circumstances, died of this disease on Sunday last, July 1st, after twelve hours illness. On the following Wednesday, at noon, I assisted my friend, Dr. Hingeston, of the South London Infirmary, in inspecting the body. The general anatomical appearances were those which I have invariably found, but the contents of the stomach and intestines were acid (the woman had been strongly purged). The heart was soft and distended, and when a puncture was made into it, a good deal of air escaped, followed by fluid blood mixed with bubbles of air. The treatment consisted in the exhibition of calomel, Cayenne pepper, and other stimulants in a fluid form. I do not remember to have seen so great a quantity of air in the heart so soon after death in any other instance. I submit the question whether the peculiar condition of the blood could have influenced this early separation of gas in the case of this woman. I am, Sir, your obedient servant,
P. H. GREEN.
July 4th, 1832.

DR. JOHNSON AND DR. RAMADGE.

To the Editor of THE LANCET.

SIR,—The character of that worthy "Fellow" and associate of St. John Long, both of whom you so well castigated in the Court of Common Pleas, is illustrated by the following facts, which the Doctor and his counsel tried hard to couple together as cause and effect, to weaken my testimony on the late trial. When I first settled in London, and before I was admitted by the College of Physicians, Dr. Ramadge and myself came into consultation in the Strand, and the learned and experienced "Fellow" expressed great apprehension lest he should be censured by the College of Physicians, if he were known to consult with me prior to my admission by that body! He, however, waived the objection when there appeared some danger of losing a fee by being too scrupulous of his honour and dignity. The matter occasioned no quarrel, and never induced an angry feeling on my part, though I often related the anecdote as an illustration of the Doctor's punctilious adhesion to professional etiquette. More than seven years afterwards, the learned Doctor took an opportunity, *in my absence*, to impugn some statement of a case which I had related in the London Medical Society; but at the next meeting, when I confronted him, he retracted his insinuations, and I fully proved the truth of my former statement. Who would imagine that Dr. Ramadge and his counsel would have the hardihood to couple these two occurrences together, and attempt to prove that Dr. Ramadge refused to consult with me in 1819 or 1820, *in consequence of his having* accused me of making false statements of a case in 1827 or 1828? Yet such they did attempt to do, till I exposed the falsity of their assumptions.

I am, Sir,
Your obedient servant,
JAMES JOHNSON.
Suffolk Place, Pall Mall,
June 28, 1832.*

ST. THOMAS'S HOSPITAL.

TREATMENT OF VOMITING BY THE ADMINISTRATION OF HYDROCYANIC ACID.

THE following case possesses interest for two reasons, first, that after death nothing was found to account in any way for some of the symptoms present; and, secondly, as it affords another proof, if any such can be wanting, of the efficacy of the hydrocyanic acid in checking vomiting, which it has been shown to do in almost every case, from whatever cause arising. To bear us out in this statement, we have only to refer our readers to Dr. Elliotson's excellent work on the properties and uses of this acid, which supplies everything that is known on the subject.

John Pindar, ætat. 30, was admitted into William's Ward under the care of Dr. Elliotson on the 1st of March. He stated that he was stoker in a brewhouse, and had been in the daily habit of drinking large quantities of porter. Eighteen months since he had an attack of rheumatism, for which he was treated in this hospital. During the last month he has experienced pain in the region of the stomach about ten minutes after taking food, and this is immediately followed by vomiting. The whole abdomen is hard and tympanitic, but there is no pain on pressure; the bowels are regular; the appetite good; the pulse small and quick.

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* Received too late for last week's Number,—ED.

March 2. Was seen to-day by Dr. Elliotson, who ordered *ext. stramonium*, gr. j, *nocte maneque*.

6. Says he is better, but has a sensation of nausea. *Ext. stram.*, gr. jss.

8. Vomited about two hours after dinner. The ejected matter is of a greenish-yellow colour. He is harassed by thirst.

9. *Ext. stram.*, gr. ij.

10. Vomiting two hours after breakfast, with much pain. Pulse sharp and quick.

11. Vomiting three hours after eating.

12. Vomiting two hours after eating.

13. Ditto.

14. Is losing flesh, which is particularly apparent in the face. No vomiting to-day. *Ext. stram.* gr. ijss.

20. Vomited three times since the 14th.

Acid. hydrocy, M. ij, *post prandium*.

Is much emaciated; cheek bones very prominent. There is a flush in the upper part of the cheek, which, on examination, consists of a number of minute, though distinct vessels. No pain in any part of abdomen, even on strong pressure. Tongue clean; pulse 144.

23. Vomited about the same time after eating on the 21st and 22d. *Acid. hydrocy.* M. iij.

27. Vomits daily at the usual time after eating. *Acid hydrocy.* M. iv *ter die.*

30. M. v *ter die.*

April 3. *Acid. hydrocy.* M. vss.

6. *Acid. hydrocy.* M. vi. From the 2nd to the 6th he vomited but twice at the usual hour after food.

10. *A. h.* M. vij. Mutton-chop daily.

17. Does not vomit so frequently; is much emaciated, and the skin is very dry. Wine ℥iv daily.

24. *A. h.* M. viij.

27. Emaciation increases rapidly. *A. h.* M. viijss.

May 4. *A. h.* M. ix *ter die*. Vomits but seldom. Thinks he is better, though weaker.

8. *A. h.* M. ixss *ter die*. Porter Oj daily.

June 4. Vomited but twice since last report, and now makes no complaint but of debility. He takes meat daily, and the wine has been increased to ℥vj. Ordered

Ferri sulph. gr. v;
Opii gr. ½ *ter die.*

Emaciation is extreme.

11. Feels much weaker, and nearly fainted this morning on slight exertion.

On the 20th he left the hospital for the purpose of seeing his wife who was just confined, and he died two days after.

Post-mortem Examination.

The body was inspected, though not very minutely, after death. The intestines and stomach were glued together, so as in fact almost to obliterate the cavity of the abdomen. The stomach was perfectly healthy, as were the intestines throughout their course. The mesenteric glands were likewise healthy. With the exception of the glueing together above stated, the viscera were healthy, and the emaciation, if not the vomiting, remain wholly unaccounted for. The viscera of the chest were also healthy; the head was not examined. In this case Dr. Elliotson suspected organic disease of the stomach, though he never would give a decided opinion on the subject.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

BY

MR. BRODIE,

Delivered June 2d, 1832.

FRACTURE OF A CATHETER IN THE BLADDER.

A MAN was admitted on Tuesday, May 29th, with a portion of elastic catheter in his bladder. He was sent to bed, and as his sufferings were very great, tinct. opii m. xl, were given him, which had the effect of greatly relieving the pain. On the following day Mr. Brodie saw him; the man was in great pain, and had constant micturition. He was ordered to be kept quiet, and to have a dose of castor oil on the following morning.

June 1. On Mr. Brodie visiting him to-day, he was found to be much the same in all his symptoms. A sound was passed, which met with some obstruction on entering the neck of the bladder, from the foreign body in it. The man complained of great pain in the bladder, and Mr. Brodie said he could feel the foreign body coiled up within the cavity of that viscus. On withdrawing the sound, Mr. Brodie said that he thought it would be better to perform the operation on the man, as for lithotomy, on the following day, and gave directions that the surgeons of the hospital should be summoned for that purpose.

2d. This day, at 2 p.m., the operation was performed. The man was placed on the table, and bound as for lithotomy, and a grooved sound was introduced into the bladder. Mr. Brodie with one incision cut through the perineum, prostate, and neck of the bladder, down upon it, and introduced the forceps (having the index finger for his guide) into the wound. The sound was withdrawn, but the forceps failed to grasp the foreign body, which was extracted in a readier manner by the fingers. The wound made by the operator was very small, and the man bore it very well. It lasted scarcely two minutes. A piece of dry lint was placed in the wound, and the man was sent to bed. Mr. Brodie then addressed the pupils :—

"Gentlemen,—It appears that this man has had enlargement of the prostate gland, which has caused occasional retention of urine, for the relief of which he has been instructed to use the catheter; this, by some means or other, got broke on his passing it, a few days ago. One part of it was drawn out through the method used by some medical man whom he consulted before he came in hither, and the other part remained in the bladder. Well now, I suppose he must have used this catheter for some time, for it is only old catheters that do break, and you know that in the practice of our art we do not use old catheters for that very reason. It is a very rare thing for a catheter to break in the bladder. I do not recollect to have ever heard of or seen such a case before. The only one at all analogous to it that I know of, was that of a gardener who had stricture of the urethra. Well, this poor fellow did not know what stricture was, or whether a surgeon could relieve him, or where a surgeon could be found, for I believe there was not one in the immediate neighbourhood where he lived. Well, but, in this dilemma, he proved himself a very cunning, clever fellow, and he certainly made use of a very strange remedy to relieve himself. He had no bougie or catheter, so whenever the difficulty of passing his water came on, he used to cut a good strong flower-stalk from his master's garden, and pass it along the urethra, and thus relieve himself. Well, he continued this for a long time, until, in an unlucky moment, the flower-stalk broke in his bladder. The poor fellow suffered very great agony from it, and at last he came to the hospital, and Sir Everard Home cut into the bladder and extracted the flower-stalk, which was very much encrusted with the phosphates, and which had in fact formed a long calculus. Well now, this portion of catheter which I have extracted from this man's bladder is, as you see, very much encrusted, and if it had been left longer in, would, I have no doubt, formed a long stone, as in the case which I have just related. Now, this incrustation need not be chemically examined to tell what it is. It is the phosphate of lime which is always deposited on these foreign bodies first; and if they remain in the bladder for a longer time the urine secreted in the kidney forms the triple phosphate upon the phosphate of lime, thus forming, as you know, the fusible calculus. The man's urine was loaded with thick ropy mucus, which, you know, is caused by low chronic inflammation of the bladder; but he complained so much of pain above the pubes, that I am inclined to think that the inflammation must have been more of an acute character than is usual in these cases. I did think of using Weiss's urethra forceps to extract this portion of catheter, but, on reflection, I thought that it would only irritate, and if it did not succeed, (of which there was a chance,) I should have, after all, to cut into the bladder; besides, too, this man had enlarged prostate gland, which was another reason against my using the forceps, so that I determined, after consulting with Mr. Hawkins and Mr. Babington upon the subject, to operate on the man in the way that we commonly operate for lithotomy. You have seen how it has been performed. He has borne the operation very well, and I think bids fair to recover."

June 4. The man is doing very well today, his pulse and tongue are good, his spirits cheerful; bowels moved greatly by castor oil, and part of the urine comes away *per urethram*. Mr. Brodie remarked that forty-eight hours had now elapsed since the operation without any bad symptom occurring, and it was very rare if any did occur, that they came on in less than that time.

The last time we saw this patient he had been moved down into one of the lower wards of the hospital, and was doing extremely well.

FRACTURE OF THE NECK OF THE FEMUR.

An old woman, Margaret Creed, was admitted May 26, having been knocked down by a horse in the street and run over. She had a severe contusion of the left leg, and on examining her more closely, Mr. Fernandez discovered that there was a fracture of the neck of the femur of the opposite side. On rotating it, a crepitus can be distinctly heard, and the trochanter major rolls in a larger arc of a circle than it naturally should; there is however not much shortening of the limb, and Mr. Brodie remarked that this latter fact was a frequent cause why fracture of the neck of the femur was overlooked, "for," added Mr. Brodie, "the shortening of the limb in these cases seldom or ever takes place before a fortnight or three weeks have elapsed since the receipt of the injury. Sir A. Cooper however states the contrary to this, and says the shortening takes place directly after the injury, but I do not know that I have ever seen this take place. In this case, Gentlemen, you see the patient is very old, and therefore I really do not think that it is worth her while at her time of life to submit to the great inconvenience of tight bandaging, &c., which she must do in order to bring the fractured ends of the bone into anything like contact. She had therefore better remain in bed for the present; rest will do her good; she will be able to walk tolerably well, and the only inconvenience to her will be that of having a sort of double joint."

On Thursday, June 14, the operation of amputation was performed by Mr. Walker, the assistant surgeon to the hospital, on the patient whose case of scrofulous affection of the ankle joint we spoke of in a former number. On the same day Mr. Walker extirpated a tumour from the neck, but in a manner not at all calculated to raise him in the estimation of a profession which boasts of dexterity and science in the performance of all its operations.

We are happy however to add that they are both doing very well.

LONDON HOSPITAL.

STRANGULATED FEMORAL HERNIA.

B. G., ætat. 50, of spare habit of body, was admitted into this hospital about twelve o'clock on the 9th June, labouring under strangulated femoral hernia of the left side. The man was placed in Gloucester ward, the accident ward for the week, under the care of Mr. Andrews, who visited the patient very shortly after his admission. The patient stated that he had been the subject of hernia from his childhood, but that not having suffered any inconvenience from it, he had never paid any attention to it until it descended on the morning of the 7th, when violent pain came on, which induced him to call in the assistance of a medical man, who finding all his attempts to return it unsuccessful, recommended his being brought hither. On his admission, the taxis was again employed without success, when Mr. Andrews informed him of the necessity of submitting to the operation, to which he consented, and he was accordingly taken to the operating theatre. The tumour was about the size of a duck's egg.

The operation was performed by Mr. Andrews about one o'clock, he commenced by making an incision over the tumour, and carrying downwards to the lower part of the swelling, in this part of the operation cutting through the integuments and cellular membrane, then dissecting carefully through the layers of fascia by means of a scalpel and director. Having reached the hernial sac, it was pinched up, and a small opening carefully made into it, from which a small quantity of serous fluid immediately flowed; the sac was then completely laid open by means of the bistoury and director. The contents of the hernial sac being thus exposed, were found to be a portion of omentum, and behind this a knuckle of dark-coloured intestine; the stricture was then divided with a bistoury, after which the intestine was readily returned. On examining the omentum, a portion was found to be adherent to the sac. Mr. Andrews then determined on removing the protruded portion of omentum; he dissected it from its adhesions, and then cut through it, at its upper part; it was necessary to secure one vessel with a ligature; the edges of the wound were then brought together, and retained by a suture and straps of adhesive plaster, over which a compress of lint was placed. He was ordered the following:—

℞ *Magnes. sulphat.* ʒj
Aquæ menthæ pip. ℥j. *2nd quaq. hora*
sumend. donec alvus respond.

In the evening he complained of considerable tenderness of the abdomen, for which twelve leeches were applied; his bowels not having been opened by the medicine, he was ordered an enema of castor oil, common salt, and gruel.

10. Passed a very restless night, and has considerable tenderness of the abdomen; bowels opened three times; pulse 76, and somewhat sharp; tongue covered with a thin white fur.

Hirudines xv, *to the abdomen;*
Hydr. c. creta gr. v. *ter die sumend.*

11. Passed a tolerably comfortably night; bowels opened once last night; pulse 68, moderately full; tenderness of the abdomen much diminished; tongue furred; skin hot.

℞ *Magnes. sulph.* ʒj;
Aq. M. p. ℥j *quant. quaq. hor.*
sumend. donec alvus respond.

His bowels not having been relieved by the evening the enema was repeated, which had the effect of relieving his bowels freely.

12th. Passed a comfortable night; bowels opened twice; tenderness of abdomen gone; can bear pressure over the abdomen without feeling pain; pulse 68, full; tongue clean; skin cool; free from thirst.

13th. Had several hours comfortable sleep during the night; skin cool; pulse 76, full, free from hardness; bowels not relieved since yesterday; tongue furred; is rather thirsty; complains of tenderness of the abdomen, to which 12 leeches were applied, and a common purgative enema was administered.

14. Slept tolerably well, and feels quite comfortable; pulse 70, moderately full; skin cool and moist; tenderness of abdomen gone; bowels open; tongue very much furred.

To take the *hydr. c. creta*, night and morning. In the evening his skin became hot, and he complained of flying pains in the abdomen, for which 12 leeches were applied.

15. Passed a restless night owing to his bowels having been very much relaxed; pulse quiet; skin cool; tongue furred.

16. Had a very good night, and this morning feels quite comfortable; bowels open; skin cool; tongue cleaner; pulse quiet.

18. Slept tolerably well last night; bowels open; tongue clean; free from pain; in every respect doing well; the wound looks healthy.

HYDROPHOBIA FOLLOWING THE BITE OF A FOX.

E. B., a man about 46 years of age, was brought to this hospital between eight and nine o'clock of the evening of the 10th inst., labouring under the symptoms of hydrophobia. His friends stated he was frequently in the habit of indulging himself with drinking freely of malt liquors and spirits, but at the time the accident occurred he was quite sober. It appeared he kept a tame fox for his amusement, but about six weeks since, being about to dispose of it, he wished to show it to its intended purchaser, and on the animal not readily obeying his call he put his hand into its house for the purpose of bringing it out, when the animal bit him, but not severely, in the back of the hand, and also in the palm. He took no notice of the bites, which readily healed up, and he continued in the enjoyment of his accustomed health until the day previous to his admission, when he complained of pain in the bitten part running up the arm to the axilla, with a general feeling of illness without being able to mention any particular part affected. Towards night he became extremely irritable, and was unable to rest in bed. On Sunday morning, while taking breakfast, after having drunk some tea, he declared he could not drink any more, for if he did he was sure it would choke him, at which time he felt a great tightness and sense of suffocation about the throat. His friends becoming alarmed by these symptoms, called in a medical man, who recommended his immediate removal to this hospital.

On his admission, his look was extremely distressed. He complained of great uneasiness and a feeling of oppression about the chest, more particularly in the region of the diaphragm; breathing hurried; pulse about 100. Almost immediately on entering the doors of the hospital he ran into the garden, and could only be got into the ward by entering through a window, as he declared his inability to pass along the passage, the slightest breath of air being sufficient to cause considerable distress; he was ordered carbonate of ammonia and opium, ten grains of the former and one grain of the latter, in the form of a pill, every hour, but which he refused to swallow. At this time he was perfectly sensible, and was ordered a blister to the spine, and an enema, containing an ounce of laudanum, to be administered.

During the night the spasms became extremely violent and more frequent, starting up convulsively every minute. About eight o'clock in the morning he was extremely violent, so much so as to require five men to hold him to prevent his doing some mischief to himself. At this time the convulsions were very violent, and the sense of suffocation extremely distressing. About nine oclock, the enema ordered last night, but which could not be administered before, owing to the violence of the convulsive struggles, was administered, and at about ten o'clock another containing six drachms was thrown up. About half an hour afterwards another was administered, and ordered to be repeated every half hour.

An ointment composed of two drachms of the acetate of morphia, with one ounce of hog's-lard, was directed to be rubbed over the spine, but this failed in producing any beneficial effect; on the contrary, we thought the spasms were more violent during the friction; shortly afterwards he became less violent, but this was probably owing more to the exhaustion of nature than to any effect produced by the remedies employed. His pulse became extremely rapid until his death, which took place about two o'clock.

The animal died a few days after inflicting the bite, but he attributed this to some person having given it poison, not thinking for one instant of its being in a rabid state.

Inspectio cadaveris, twenty-four hours post-mortem.

On removing the calvarium, there was found to be an effusion of serum between the pia mater and tunica arachnoides, and on cutting into the substance of the brain a greater number of red points were seen than usual. About half an ounce of serum in the lateral ventricles. On cutting through the theca vertebralis, a small quantity of serous fluid issued out; the spinal marrow appeared more vascular than natural, and opposite the cervical vertebræ it presented a brownish appearance. The tongue was examined, but no pustules, as described by some authors, could be discovered, the glands on the dorsum of the tongue were in a state of enlargement as were several of the lymphatic glands of the neck. The lining membrane of the pharynx highly vascular, the vascularity extending into the trachea; lungs healthy but gorged with blood; the sympathetic, eighth pair, and phrenic nerves, did not exhibit any appearance of disease; we thought them rather smaller than might have been expected in a stout man; œsophagus healthy; nothing particular in the stomach excepting a few very red spots beneath the mucous membrane. Intestines healthy, with the exception of the jejunum, a small portion of which was highly vascular. The back of the hand in the vicinity of the bite was carefully dissected, when several twigs, apparently branches of the superficialis nerve, were seen close to the cicatrices, but they did not appear to be diseased.

HYDROPHOBIA FOLLOWING THE BITE OF A DOG.

On Wednesday the 13th June, about four o'clock in the afternoon, another case of hydrophobia was brought into this hospital.

The patient, a fine boy about four years of age, had been bitten in the face by a dog about seven weeks since. A medical man who saw the child soon after it was bitten, recommended the excision of the part, but this the parents of the child would not allow. The symptoms began to present themselves a few days previous to his admission, by drowsiness, and general disinclination to move; about twenty-four hours previous to his being brought here, decided symptoms of hydrophobia presented themselves.

The treatment adopted in this case, consisted in applying six leeches to the throat, which appeared to afford decided relief for a time, and grain doses of the superacetate of lead every half hour, until eighteen grains had been given, when violent colicky pains came on; the lead was accordingly discontinued, and clysters containing 40 drops of laudanum with castor oil, were ordered, two of which were administered, and which appeared to afford some transient relief. The little sufferer lingered on till about four o'clock the following afternoon, when death put a period to his sufferings. We believe no post-mortem examination was made in this case, owing to the parents' refusing their consent.

PHLEGMONOUS ABSCESS OF THE THIGH.

W. E., seaman, ætat. 26, was admitted on the 26th May, under the care of Mr. Scott, with an extensive phlegmonous swelling of the left thigh.

He states that a short time since he came from Plymouth in a steam-ship, and at night laid himself down to sleep upon the deck near the boiler, without any other covering than the clothes he wore in the day time. When he awoke in the morning, he felt himself very chilly, with pain in his limbs, and considerable lassitude; on his returned home to his friends, he felt extremely unwell, and the thigh became red, and very much swollen, with considerable pain in the part. He was attended for a day or two at home by a medical man, who bled him largely, and applied a number of leeches to the thigh, but finding the swelling increase, he recommended his being brought hither.

On his admission, the thigh was found to be of nearly double the size of the sound limb, extremely red and painful. He was ordered to have forty leaches applied immediately; the thigh afterwards to be well fomented, and then spirit wash constantly applied; his pulse was quick and weak; countenance anxious. Was ordered the following medicine:—

> ℞ *Ammon. carbon.* gr. v;
> *Tincture opii* ♏v;
> *Mist. camphoræ* ℥j; *ft. haustus 2nd quaq. hora sumend.*;
> *Tinct. opii* ♏xxx, *hora somni sumend.*

27th. Slept tolerably well during the night, and feels comfortable; pulse about 100, and weak; skin hot; tongue rather furred; has less pain in the thigh, which appears to be rather smaller than last night; no fluctuation can at present be detected; bowels not opened since his admission; he was ordered a small quantity of castor oil, which procured one motion. To repeat the leeches and fomentation as well as the spirit lotion, and continue the draughts and anodyne at night.

To have a pint of porter daily.

28. Passed a comfortable night, but complains of great weakness; has very little appetite; bowels regular; skin hot; complains of considerable pain in the thigh, to which the spirit lotion continues to be applied. To continue the draughts, with the addition of five minims more of the laudanum to each draught, and to have eight ounces of wine, in addition to the pint of porter, daily.

29. Passed an indifferent night, and does not feel so well as yesterday; countenance anxious; pulse 96, small and weak; skin covered with perspiration; bowels opened; swelling of the thigh rather more than yesterday. Mr. Scott considering he could detect fluctuation, made an incision with a scalpel about four inches in length in the back of the thigh, which gave exit to a small quantity of purulent matter.

To continue the draughts and anodyne and the thigh to be enveloped in a poultice.

31. Passed a restless night, and feels very weak and low; tongue furred; bowels open; has considerable sweats at night; countenance anxious; the limb discharges a considerable quantity of purulent matter daily.

The quantity of wine was to-day increased to ten ounces; and the limb ordered to be rolled from the foot to the hip, leaving an opening over the incision for the exit of the discharge.

June 2. The discharge is very much increased, and he feels much exhausted; the swelling of the thigh is much diminished, being now of nearly the natural size; sleeps tolerably well; bowels open without the use of aperient medicine. To have the quantity of wine increased to sixteen ounces, and continue the porter; takes the draughts and anodyne as before.

4. Appears much exhausted; the exertion attendant on having the thigh dressed causes very great fatigue; the discharge is profuse, requiring his bed-clothes to be changed twice a day; tongue clean and moist, but tremulous; bowels natural; appetite bad; pulse 110, small and weak; the night sweats continue very profuse. The quantity of wine was to-day increased to twenty ounces.

7. Is much exhausted; countenance indicative of great debility; bowels open; pulse quick and small; tongue partially covered with a brownish fur; profuse sweats; discharge very considerable; is allowed twenty-four ounces of wine, with two pints of porter and two mutton chops daily, and desired to ask for any nourishment he could relish.

To continue the draughts with the anodyne at night.

9. Much the same as on the 7th. To-day the porter was exchanged for ale at his request.

12. Has become much exhausted; passed a restless night; and this morning could not take his draughts; the discharge continues much as before, as do the other symptoms. Mr. Scott ordered him to discontinue the draughts, and take the following:—

℞ *Quininæ sulphat.* gr. v;
Acid. sulph. dil. ♏ x;
Tinct. opii ♏ v;
Mist. camphoræ ℥j; *ft. haust 6tis hor. sumend. Rep. haust. anodyn. h.s.s.*

Since the above report was written some little amendment has taken place; the night sweats are less profuse, but the discharge still continues very considerable; every means of supporting his strength are resorted to; he takes the quinine three times a day, and is allowed anything nourishing he desires; his daily allowance consists of two pints of port wine and two pints of ale, with beef steaks.

July 3. The poor fellow kept gradually sinking until yesterday morning, when he expired.

OPERATION FOR THE REMOVAL OF CATARACT.

June 12. Mr. Scott operated for the removal of cataract on G. E., ætat. 66. This man stated he had been the subject of this disease for the last five or six years, and cannot assign any cause for its formation; his occupation has been that of a silk dyer.

On an examination, both eyes were found to be affected with the disease. The cataracts are of an amber colour, and the motions of the iris free; there was some degree of vision; the patient could distinguish large and bright objects, and could see more distinctly in a weak than in a strong light; thus, towards evening he could see objects which in strong daylight he could not perceive; by these symptoms, coupled with the age of the patient, the cataract was judged to be of the firm kind.

The patient being laid upon a table, Mr. Scott made an incision in the cornea of the right eye with Beer's knife, passing it through the anterior chamber to the opposite side, then introducing the curved needle, after which, by making a little gentle pressure on the globe, the cataract was readily removed. After the operation, a compress of linen, wet with cold water, was applied to the eyes, and fixed by a bandage, and the patient was directed to be placed in bed in a darkened room, with his back to the light, and to be allowed nothing in the way of food but slops, and those of the blandest kind.

13. Passed a restless night, but does not complain of pain; tongue rather furred; bowels not open; has very little inclination for food, and what he takes consists of a little tea; pulse 80, moderately full; skin cool.

14. Passed a restless night, but feels inclined to doze this morning; has no pain;

pulse quiet; skin cool; his bowels not being relieved, was ordered a dose of castor oil, which opened his bowels twice; tongue clean.

15. Slept very well during the night, and appears to be quite comfortable; bowels open; skin cool; tongue clean; pulse quiet; free from pain; the incision in the cornea appears to be united.

20. In every respect doing well; the section of the cornea has perfectly united; it will be seen that no active treatment has been required in this case; the whole that has been done for him has been to carefully exclude the light, and pay proper attention to his bowels; he has not complained at any time since the operation of the least pain in the head, and now enjoys very good vision.

COMPOUND FRACTURE OF BOTH LEGS.

— Lynch, a sailor, aged 50, was admitted on the morning of the 11th May, under the care of Sir William Blizard, with compound fracture of both legs. The injuries were occasioned by the poor fellow falling down the hold of a ship in the St. Catharine Docks, and falling on his feet on some iron ballast, his right leg passing between the pieces of iron, while a quantity of rope fell upon his left leg, by which the ends of the bones were driven through the integuments.

On examination, a very extensive compound fracture of both legs was found, the lower portion of each fibula was protruding; there was considerable laceration of integument; the bones were completely smashed. At the time of his admission there was considerable hemorrhage, particularly from the left leg, which was supposed to come from the anterior tibial artery, but which on a more minute examination was found not to be the case. A consultation was held, when it was determined to defer amputation until more urgent symptoms arose, and thus give him a chance of recovery without amputation, the anterior and posterior tibial arteries having apparently escaped being injured, as they could be felt pulsating in the foot. One ligature was applied, which had the effect of controlling the hemorrhage; the integuments were brought together by strips of adhesive plaster, and the legs placed in splints, and ordered to be kept constantly wet with cold wash.

12. Slept for about two hours; pulse 80, full; skin cool, complains of a sense of coldness over the whole body; has pain in the feet, particularly the right foot; there has been very little hemorrhage; tongue furred; his bowels were opened just before the accident. To take the

Mist. salinæ acetat. ℥ss. *4tis horis.*

13. Passed a very restless night; pulse quick; tongue furred; skin hot; countenance anxious; appetite bad; takes a little tea; the limbs swollen and very painful.

14. Slept for nearly five hours during the night, and feels much more comfortable; pulse 84, small; tongue furred; free from thirst; skin cool and moist; bowels have not been opened.

15. Was extremely restless until about one o'clock this morning, when the dresser ordered him thirty drops of laudanum, which had the effect of giving him several hours comfortable sleep; expresses himself much more comfortable this morning; had considerable pain during the early part of the night in both extremities, particularly the left; running from the feet to the groin; pulse 64, small; skin cool. The pads were changed to-day, and the limbs look very well, the fractured portions being in apposition.

16. Passed a very restless night, which he attributes to his legs being moved yesterday; complains of a burning and throbbing pain in his legs, which is much more severe in the left; bowels open twice; tenderness in epigastric; skin hot; free from thirst; has no appetite; was ordered two pints of porter daily, and to take

Decoct. cinchonæ ℥jss *ter die.*

17. Had a very bad night, not having slept more than half an hour during the whole night; bowels have not been opened; skin hot; pulse 84, full; pain in the left leg, which is hot and swollen.

18. Passed a sleepless night, owing to the severe pain in the left leg extending to the groin; bowels open; skin hot and dry; pulse 90, full, with a degree of sharpness; appetite none; is thirsty; tongue slightly furred; the splints were removed again to-day; the legs are hot and swollen.

19. Passed a very tranquil night, and this morning feels much more comfortable; bowels opened once; tongue clean; appetite improving; pulse 84, moderately full. Continues taking the bark.

22. Much as on the 19th.

CORRESPONDENTS, &c.

Mr. Trustram. The plate and the formula have both been given in back numbers of THE LANCET.

"*Owen Danvelto*" is, we presume, an *anonymous* signature.

We should certainly have given *Dr. Ayre's* paper, received this week, insertion in our present number, but circumstances render its appearance until our next impossible.

Has *W. C.*, who wrote from one of the Borough Hospitals, any leisure?

The replies to Mr. Costello's letters next week.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JULY 14. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXVI.

THE BRAINS OF DOMESTICATED ANIMALS.
(*Continued.*)

In the following rapid description of the form and convolutions and structure, both external and internal, of the brain, I shall principally confine myself to that of the horse, merely glancing at the points of difference as we pass along. It will, however, be, I fear, a most unsatisfactory detail, a mortifying proof of human ignorance. To very few of the internal irregularities and recesses and tubercles of this organ, shall I be able to assign any definite function; and with regard to the great majority of them, I shall be compelled to confess utter ignorance. The common sensorium, the very seat of intelligence, is enveloped in darkness and mystery, through which human ken has not yet been enabled to penetrate.

The Structure of the Brain.—I am first struck with the manifest difference between the substance of the brain, and that of any other part of the frame. I can find little or no cellular texture within the brain. Although, after long maceration, and by the power of the lens, I can resolve the whole into a collection of fibres, the tissue which connects these fibres together still escapes my observation. There is no relative motion,—no relative change of place between the parts,—no elasticity required, and the interposition of any considerable quantity of cellular substance would not only be useless, but an evil. There is no interposition of adipose matter in the brain;—that would necessarily be connected with the existence of considerable cellular texture, and would either imply, or cause, a certain degree of motion between the parts of the brain; and, more than this, the adipose substance is most of all under the power of the absorbent vessels, and rapidly increased or diminished, according to the healthy or diseased state of the frame. The accumulation or absorption of the adeps is a matter of little consequence elsewhere; but in sickness and in health the bulk of the brain must remain unchanged; there must be no increase of substance to press upon the origins of the nerves,—there must be no loss of substance to create a vacuum, or to produce engorgement, or perhaps a rupture of the vessels.

The Chemical Composition of the Brain.—I find an apparently pulpy mass which has little interposed cellular or adipose matter. When I triturate this pulpy matter, I can form a perfect emulsion, that will pass through the finest sieve. I coagulate it by heat or by acids, and I find that the coagulum is different from that which can be produced from any other similar substance; it consists of mingled albumen and oil, and with none of that which we almost regard as the principle of animal matter,—nitrogen. In place of this, I discover something which no other part of the frame will yield—uncombined phosphorus; and this albumen and oil, with one-twentieth part of the various salts of potassa, sodium, lime, and magnesia, and one-sixtieth part of osmazome, make up the solid part of the brain. The cerebral mass is composed of one part of all these solids combined, and four parts of water. While, therefore, I trace to the brain a peculiar function, I find a peculiar composition. There seems to be little or no difference in the chemical composition of the cortical and the medullary parts. I have ventured to assign to them a difference in function. This theory, however plausible and interesting it may appear at present, and firmly as I may now be disposed to believe in it, may hereafter be discovered to be as untenable as that which assumed that the cortical portion consisted of a congeries of blood-vessels for the production and nourishment of the medullary part, whereas the latter had almost attained its full development, before a trace of the former could be discovered. The microscope has also ascertained, that instead of the larger arterial branches being found in the cineritious, and their minute ramifications dipping into the medullary, the vessels of the medullary portion are considarably larger than those of the cortical. But I must proceed to the enumeration (it will be little more) of the tubercles and cavities and irregularities of the brain.

The Corpus Callosum.—Having stripped off the dura mater, and holding the hemispheres a little apart, I observe a white convex body occupying the bottom of the sulcus. It has the appearance of the crown of a compressed arch, and so it is: it is the central portion of the roof of two cavities, which I shall have presently to describe—the *lateral ventricles*. This is the *corpus callosum*, or hard medullary body; it is of firmer consistence than the medullary portion of the brain generally. I can plainly see the direction of the sides or supports of this arch, and by a little dissection I can clearly trace them. The anterior support or pillar I can follow down to between the corpora striata; and the posterior ones are continuous with the fornix and the cornua ammonis. When I examine it more closely, I perceive that it is not only the roof of the ventricles, but a bond of union, or a connecting medium between the medullary matter of the superior part of the brain on each side. I make a transvere incision horizontally across it, and continued over the ventricle on each side, and I can see the fibres or bands of medullary matter, running from one hemisphere to the other. Therefore it is that human anatomists call it *the great commissure*. Running along the centre of this body is a little depression or channel. It is occupied by one of the cerebral arteries, and is worthy of notice, as being the central line of the brain,—that through which it might be divided longitudinally into two equal parts.

Is of different Size in different Animals.—This corpus callosum, or great commissure, is, as you will perceive, very different in size in our different patients. It is, as we have seen, a body or band of very considerable bulk in the horse. In the ox it is considerably smaller, smaller even in proportion to the size of the two brains. In the dog it is larger than in the sheep; it seems to bear a relative proportion to the development of the cerebral hemispheres. It increases in size in proportion to the rotundity of the brain and the intelligence of the animal, and is largest of all in man. It is developed in a direct ratio with the cerebellum in every animal, and also the tuber annulare and optic thalami, but it *is* in inverse proportion to the corpora quadrigemina, and the spinal chord.

Not found in some Animals.—The corpus callosum is said to be wanting in the hare, the rabbit, the rodentia generally, and in birds. There are no lateral ventricles in these animals of which it can become the roof, yet it exists in smaller size, as the commissure or bond of union between the medullary matter in the superior portion of each hemisphere. The fibres or bands running from one hemisphere to the other, are more remarkable than where the corpus seems to have a double duty to perform.

The Septum Lucidum.—Either dropping from the under side of the corpus callosum or rising from the fornix, are two medullary laminæ partly united together by their interior surfaces, and called from their semi-transparency the *septum lucidum*. It is a medullary curtain let down between the lateral ventricles, and dividing them from each other, a communication between them being left only under the arch of the fornix. An incision being made along the central sulcus, these laminæ are easily separated from each other; and between them is found a little fissure or cavity, called by some the *fifth* cavity, and by others the *fossa sylvii*. It usually contains a moisture or dew within it; but I have never seen the abundant serous fluid of which some have spoken.

The Fornix.—The fornix is now brought into sight, the French call it the medullary or cerebral triangle; there is an indistinct resemblance to a triangle, with one of the angles presenting anteriorly, and the other two posteriorly; its lower surface is toward the third ventricle, and is spread over the optic thalami,—a beautiful vascular membrane is interposed between them,—and the impression of the vessels of this membrane seem to be formed upon it;—this is the psalterium or harp, and on its upper surface rests the septum lucidum. Of its four irregular extremities or *crura*, the *anterior* or *inferior* ones may be traced from the corpus albicans—winding round the thalami—bending upwards behind, and becoming connected with the anterior commissure; then approximating towards each other, and, erroneously supposed to unite, forming the arch or body of the fornix within the ventricles;—the *posterior* or *superior crura* are prolonged into the hippocampi, and running along the crura of the hippocampi form the *corpora fimbriata*, by their thin fringed floating edges.

The Lateral Ventricles.—They are first brought into view when we are dividing the septum lucidum to demonstrate the fifth ventricle, and on reflecting back the roof we have their irregular shape and strangely-irregular floor fully exposed. The floor consists mainly of two convex, pyriform substances, and a central channel. That which appears outwardly and anteriorly is the *corpus striatum.* It may be said indeed to occupy almost the whole of the outer portion of the floor, being narrow posteriorly, and assuming a broad pear-like form anteriorly. It is covered by a lamina of medullary matter, so thin that the colour of the cortical substance beneath is seen through it. If this cortical substance is cut into, it presents an appearance more or less striated, differing in different species of animals, and often varying in animals of the same species; more evident usually in the herbivorous than the carnivorous animals, with the exception of those that hunt by scent. The striæ take a direction from above downwards, and from without inwards; and the broad extremities of the corpus striatum in each ventricle lying in approximation with each other, the striæ from each seem to unite under the anterior commissure. I have never seen the changing direction of the striæ which is spoken of in the human being. The corpus striatum is visible again at the base of the brain, where it evidently becomes one of the origins of the olfactory nerve as well as the commencement of the mamillary process. In the rodentia and edentata the corpora striata are much larger than they are in the carnivora, the ruminantia, or the solipeda. In the bird they are larger still, and the olfactory nerve plainly arises from their anterior extremity. The corpus striatum varies in form in the different species of birds, but in almost all of them it constitutes nearly the whole of the hemisphere.

The Hippocampi—The convex body which occupies the inner and posterior portion of the floor is the *hippocampus*—narrower and more prominent than the corpus striatum, and consising of cortical or striated matter, covered by a thicker lamina of medullary substance. As this body proceeds superiorly, outwardly, and presently downwards, and narrows as it goes, it assumes the name of the *crus hippocampi,* and it descends into a blind pouch or hollow,—the superior cornu,—where it ends in a distinct bulb, the *pes hippocampi.* In none of our domesticated animals have I observed the ventricle in the pes hippocampi which exists in the human being. The medullary covering is derived from the corpus callosum, the striated portion is obtained from the very centre of the hemispheres.

The Corpus Fimbriatum.—Along the side of the hippocampus, of considerable width anteriorly, and narrowing as we trace it posteriorly, is the *corpus fimbriatum,* of which I have already spoken, skirted by the plexus choroides, and partly covering it. It is the margin of the fornix prolonged. It enters with the hippocampus into the posterior cornu, and terminates acutely near the pes hippocampi.

The Plexus Choroides.—Occupying the channel between the hippocampus and the corpus striatum, we find the *plexus choroides,* a considerable collection of minute blood-vessels, both arterial and venous, enveloped in folds of the pia mater. It enters, or the arteries of which it is composed, enter, the ventricle under the arch of the fornix, then taking a direction anteriorly, it speedily terminates in a kind of bulb; but, being reflected backwards, a portion skirts the whole edge of the corpus fimbriatum, and dips deeply into the superior cornu, where it also terminates, but rather acutely. After it has discharged some important office within the ventricle, the veins escape by the same opening; and those from the opposite ventricles uniting form the *vena Galeni.* There can be little doubt, I apprehend, with regard to the function of the choroides. It secretes the fluid contained in the ventricles. It is occasionally subject to disease; a small enlargement is observed, and a gritty matter is not unfrequently detected in it, and sometimes the vessels appear to be agglutinated together, and an ovoid tumour appears considerably larger than an almond. Here are two extraordinary specimens; of the history of one I am ignorant—in the other specimen each plexus, when first taken from the brain, weighed nearly ten drachms; the ventricles were distended with serum, and the disease was marked by the principal symptoms of staggers.

Use of the Ventricles.—Much dispute has arisen with regard to the use of the ventricles. Even in the healthy living animal they contain some portion of fluid. If the horse is destroyed and the cranium opened as speedily as possible, serum is found in the lateral ventricles. Some physiologists have contented themselves with observing that the ventricles are merely cavities left in certain parts of the brain where irregular surfaces are opposed to each other; others as plausibly have imagined that they are formed by the foldings of the convolutions of the brain—while some have supposed that they were intended to preserve that full and perfect occupation of the cranial cavity which seems to be necessary to the safety of the brain in the natural motions of the animal. If a portion should be diseased and absorbed, the plexus choroides or the membrane of the ventricles may secrete a proportionate quantity of fluid: to which we may add these spaces left in the centre of the brain or filled only with fluid, are adapted to neutralise any concussion to which the brain may be exposed.

The Cornua of the Ventricles.—The channel running along the floor of the ventricle terminates posteriorly in a blind pouch leading downwards towards the centre of the brain, and in which are found the pes hippocampi, and the acute termination of the plexus choroides—this is *the superior cornu*: the *inferior cornu* is more complicated; it takes a direction also towards the base of the brain, and is continued even into the æthmoidal ventricle, which I shall have presently to describe. From the winding course of both these canals, the name of *cornua* has been given to them.

The Thalami Nervorum Opticorum.—Having turned back the posterior angle of the fornix, and lifted the interposed membrane or veil which is stretched beneath it, we recognise two white prominent bodies; they are broad and somewhat divergent from each other posteriorly, but they approximate as they proceed forwards, and gradually contract into two medullary bands, which wind round the crura cerebri, or accumulated mass of medullary matter below, and are the origins of the optic nerves—they will be more particularly described hereafter, and they are now particularly pointed out to you as illustrations of the origins of the sensitive nerves deep in the centre of the brain. We observe considerable difference in the development of these tubercles: in proportion as the lobes of the cerebellum are enlarged, so are the optic thalami—in proportion as other tubercles which I shall have presently to describe, are enlarged, the optic tubercles are diminished. When we cut into the thalami we find a considerable quantity of cineritious matter, as in all the other irregular prominences at the base of the brain.

The Commissura Mollis.—These thalami are united together by a medullary mass so soft and pulpy as to assume the name of *the soft commissure.* It yields to the slightest attempt to separate the thalami; in fact it is a continuation of the cineritious matter under the medullary covering, and of which the thalami are composed.

The Tænia Semicircularis.—Between the thalami and the corpora striata, and partly concealed by the plexus choroides, we observe a singular tract of medullary matter running from the hippocampi down to the anterior commissure, and connecting together the posterior and anterior parts of the brain as the anterior commissure from one corpus striatum to the other, and the posterior commissure above the commissure mollis connect together the sides of the brain. The use of all these links will be ascertained by some future physiologist.

The Foramen Commune Inferius.—There is a curious connexion between the cavities as well as the substance of the brain, of which we have here an evident proof. Observe this triangular-formed hole below the soft commissure, and between it and the inferior one; it conducts to a sulcus between the thalami, formed by their divergence. We call it a ventricle, but it is nothing more than a little gutter, having the thalami, the fornix, and the velum, as its roof, and the crura cerebri as its floor. It is continued backward under the posterior commissure to a wider canal, the fourth ventricle, lying under the cerebellum, and having it as its roof, and the medulla oblongata as its floor.

The Infundibulum.—At the bottom of the anterior part of the third ventricle, and under the foramen commune inferius, is another funnel-shaped opening. It is comparatively large in the ventricle, but rapidly narrows, and conducts towards the pituitary gland, seated immediately beneath. Of its office, or rather that of the pituitary gland, I shall immediately speak.

The Pineal Gland.—Between the commissuræ mollis and superior, is another foramen, which leads down to the third ventricle, and would likewise form a communication between the lateral and third and fourth, if it were not closed by the velum interpositum. Placed over this foramen, and between the thalami and behind the quadrigemina, is the *pineal gland.* It is a small pear-shaped reddish-grey body, enveloped in folds of the pia mater, and, not only by means of the pia mater, but distinct pedunculi proceeding from itself, connected not only with almost every neighbouring part but some distant tracts of the brain. Its little peduncles are traced without much difficulty; they look like nerves proceeding from it. Observe them running over the thalami, and penetrating even to the crura of the fornix, and then consider with how many and how important parts of the brain these are connected. We term it a gland, for it is surrounded by a plexus of vessels, with which it is intimately associated, and which may be traced into and from it. Galen thought that it was the very seat of the soul; others have assigned an inferior office to it, namely, that of a manufactory or reservoir of serous fluid separated from the blood. I must honestly confess that I know nothing about it: but when I see it so connected, so curiously organised, and so securely defended, I am persuaded that it does discharge some important function, which a more fortunate inquirer may probably be hereafter enabled to develop. In the hare and rabbit it seems to be more decidedly vascular than in any other domesticated animal, and although I have not been able to find in the horse the gritty, sandy matter of which human physiologists speak, this is abundant enough in some of the ruminantia, and particularly of the deer kind.

The Corpora Quadrigemina.—Behind the pineal gland, the fornix, and the corpus callosum, are four eminences, the *corpora quadrigemina.* We know not their function, but it must be an important one, for they are the first portions of the medullary matter that appear. They are earliest seen in the form of two little tubercles, distinct from each other; by degrees a well-defined channel appears between them, and, at a still more advanced period of utero-gestation, a transverse line is discovered dividing each of them into two, and completing the corpora *quadrigemina.* They are important, however, in comparative anatomy, as unerringly pointing out the class to which the animal belongs.

The Nates and Testes, considered with reference to Comparative Anatomy.—The tubercles are divided into the *nates* and *testes,* of which you may perhaps form some idea from a reference to the following singular fancy of anatomists. They supposed this portion of the human brain to bear some resemblance to the posteriors of an hermaphrodite,—the vulva is the foramen commune anterius, the anus is the foramen between the soft and the superior commissures, and then the division of the tubercles will answer to the relative situation of the nates and testes; the uppermost are the nates, the lower ones the testes. The transverse line is differently placed in different animals. In herbivorous animals it is placed lower, so that the nates are most developed; in carnivorous animals, it is found higher, so that the testes preponderate, and in omnivorous animals, as in the human being, the transverse line is nearly in the centre, and there is little difference in the size of the tubercles. The observation of these brains of the sheep, the dog, and the swine, will impress this on your minds. You need only to see the tubercula quadrigemina in order to decide on the character of the animal as designated by his food.

Proportion to other parts of the Brain.—These tubercles present us with another lesson of comparative anatomy. Observe how much larger they are, compared with the bulk of the brain, in the ox than in the horse, and particularly how much more so than in the dog. Their development is in proportion to that of the optic nerve and the spinal chord, and in an inverse proportion to that of the tuber annulare. Connected with this, it is evident that in proportion to the bulk of these tubercles, that of the cerebral hemispheres is diminished, or the animal principle preponderates over the intellectual. We have already seen, that the optic thalami bear also an inverse proportion to the tubercles. On these points of coincidence and of difference, we shall probably be hereafter enabled to found a rational theory of the functions of the various parts of the brain. In the hare and rabbit, and the rodentia generally, these tubercles have an extraordinary development. In these animals there are no lateral ventricle, and the quadrigemina are hollowed out to form lesser ventricles, that probably answer the purpose of the lateral ones in others. An incision into these tubercles will bring to view numerous faint striæ of cineritious and medullary matter. At the base of the testes is a medullary band or tract, which reaches as far as the optic thalami, and connects these prominences together; and also a medullary curtain or screen, somewhat resembling the septum lucidum, rises up, connecting itself with the crura cerebelli, and forming a kind of roof for the anterior part of the fourth ventricle.

The Crura Cerebri.—I now turn to the base of the brain, and there I observe in every domesticated quadruped the great preponderance of medullary matter. I can distinctly trace three lobes on each side; on the two anterior or inferior ones. I recognise the mamillary processes resting upon the æthmoid bone, and abutting upon the cribriform plate of the same bone, and also the distinct and largely-developed bulk of the olfactory nerves. In the middle lobes, resting upon the same bone, I observe the ovoid projection of the corpora striata, and the origins of the olfactory nerves from them; and my attention is more particularly directed to the *crura cerebri,* or condensation of the medullary matter of the base of the brain. They are large white chords or pillars, beginning to appear opposite to the anterior portion of the corpora striata, and pursuing on each side a direction inwardly and posteriorly towards each other. They contain some grey and dark-brown-coloured cineritious substance, in flakes and in masses, but no longer in striæ. The crura, resting upon the wings of the sphenoid bone, gradually approximate, and a little before their point of union there is interposed between them a small round medullary body.

The Corpus Albicans.—When I say a small round medullary body, I am describing the *corpus albicans,* as it exists in the horse and in all ruminants and herbivorous animals. In the omnivora and carnivora it is divided into two. This is another lesson of comparative anatomy. The corpus albicans is placed immediately posterior to the lower opening of the infundibulum. It would seem half to surround it, as if by its elevated edge to turn away, and direct to the anterior portion of the base of the brain, any fluid that might descend the infundibulum. But of this we know nothing.

The Pituitary Gland.—Placed immediately under the lower opening of the infundibulum, and separated from it by a lamina of dura mater, is the *pituitary gland*. This is a dark-reddish body of a compressed circular form, occupying the sella turcica of the æthmoid bone. It is flat inferiorly, and convex towards the brain, and has every appearance of a glandular structure. The profusion of arterial vessels around it is almost incredible, and veins have been clearly traced from it into the cavernous sinuses on each side. Being placed immediately under the infundibulum, we are naturally induced to trace some connexion between it and the infundibulum, and likewise the other sinuses with which the infundibulum communicates; but there has been no anatomical proof of this: it has never been shown that the lamina of the dura mater is actually pierced, nor has any channel been pointed out through which the moisture so derived might be conveyed away. The pituitary gland has rarely seemed to be affected by any disease in the ventricles, or accumulation of fluid there. Here, again, we must confess our utter ignorance. There is no difference in the construction of the pituitary gland in different animals, except that it seems to be proportionally larger in them than in the human being, and large in proportion to the quantity of medullary matter; while these specimens will show you that it varies materially in bulk in animals of the same species.

Immediately anteriorly to the pituitary gland, the tractus optici cross the crura cerebri, and creep into sight from under the corpora striata, in order to form the optic nerves, with which indeed they are continuous.

Posteriorly to the pituitary gland, the crura cerebri are found in perfect approximation, and so they proceed to the tuber annulare, giving out in their way the third pair of nerves. At the tuber they are joined by two large medullary chords from the lateral lobes of the cerebellum; but the nature of the union which there takes place cannot be understood until we have inquired into the structure and function of the cerebellum.

Use of Potassium as a Caustic.—Potassium burns on coming into contact with water. To apply it as a caustic, a card is perforated by a hole of the size of the ulcer, which it is wished to produce; the card is moistened, and adapted to the place to be cauterized. A small bit of potassium is to be then placed in the perforation, and some drops of water let fall on the metal. The combustion is performed with the rapidity of lightning, and the pain lasts only during the oxydation of the potassium. When completed, the burned place is to be dressed with lint and simple cerate.

CASE OF

MALIGNANT CHOLERA,

TREATED AT FIRST ON DR. STEVENS' PLAN,

Temporary Improvement following Saline Injections, and, subsequently, Spirituous Injection.—Excitement.—Death.

To the Editor of The Lancet.

Sir,—Having observed in one of your contemporaries a query, whether the injection of spirits and water into the veins had been tried in the epidemic cholera, I am induced to forward to you for publication, the particulars of a case in which that plan was resorted to by Mr. Bennett and myself, after a return of collapse, previously relieved by saline injection. The result was unsuccessful, though not unfavourable to further trial, since the same effect was produced as that described to have taken place when the experiment was performed on healthy animals, namely "*sudden* production of the same state of excitement which is observed to come on *slowly*, when taken into the stomach."* I am, Sir,

Your humble servant,
W. J. Little.

15, Billiter-street, July 3, 1832.

CASE.

At half-past 11 a.m., June 22, 1832, visited, for the first time, Mrs. Bowles, ætat. 35, complaining of uneasiness at the epigastrium, thirst, and sickness. She stated that she had had diarrhœa upwards of a week; but it was not until half-past three that morning that she was seized with faintness, cramps, and vomiting; the latter of which continued, the diarrhœa also has been considerable, and there was then a slight discharge. The mind appeared clear; the trunk was warm, but the extremities were cold, of a dusky hue, although not much shrunken. The tongue and breath below the natural temperature. Mr. Luff informed me, that the pulse had been imperceptible nearly two hours, and that she had been

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* Although irrelevant to the present subject, it is not quite unworthy of remark, that the increase of the number of cholera cases in London about the third week in June was coincident with a similar increase in Paris (as will be seen by the subjoined table), and with the irruption of the disease in Brussels and other towns of Belgium.

Official Sanitary Bulletin. Deaths from Cholera in Paris.

June 16	8	June 17	2.
11	21	18	35
12	18	19	42
13	14	20	38
14	17	21	38
16	21		

W. J. L.

submitted since four a. m. to the saline treatment. The injection of Dr. Latta's solution at temp. 112° F. was commenced; the pulse returned; she said she no longer saw double; that her head was clearer, but the supervention of pain in the region of the heart led to the discontinuance of the operation, forty ounces having been introduced into the vein. A severe rigor and insensibility quickly succeeded, and after the application of sinapisms, and the administration of stimuli, we left her to all appearances moribund.

At half-past one p. m., we found that the rigor had ceased; pulse still perceptible, quick, and very weak; the skin a little warmer; slight diarrhœa and vomiting continuing. A copious enema of gruel, with salt, was administered and kept up by means of a plug. Brandy and solution of saline powders for drink. From this time to half-past two she complained of intense oppression and pain at the chest, and was very restless; the pulse was not quite extinct above the elbow; sixty respirations in a minute. Light delirium when aroused from her insensibility; was more cold and blue than ever. Thirty ounces of the saline mixture, with two drachms of alcohol, were injected. During the process she at first muttered prayers to herself, then observing that we were watching her, she began to be rather violent, asking us if we did not think her crazy; said she was sure we did; she was no more crazy than we were. Her eyes wandered; in fact, her tone and gesture were those of an intoxicated person; her pulse returned, quick, strong, and full; and after a quarter of an hour she was perfectly rational, expressing great relief, which, unfortunately, was only temporary; intense collapse, with insensibility, returned within an hour. She died at four p. m.

PROPOSAL TO TRANSFUSE BLOOD AFTER SALINE INJECTIONS IN CHOLERA.

To the Editor of THE LANCET.

SIR,—Permit me to recommend to the gentlemen who are trying the effects of the saline injection in the pestilential epidemic which at present prevails, to *follow up* their injections of the veins with transfusion of vital blood.

From what I have seen of the effects of the injections, I am convinced we have established one most important point, viz., that the mere stimulus of the mechanical dilatation will abruptly rescue the patient from the grasp of death. We, however, observe in the majority of cases, that this interference is as transitory as it was peremptory. If then transfusion of blood follow the saline injections, I think we may reasonably expect that the consolidating stimulus of the vital fluid will render the durability of the mechanical stimulus more permanently beneficial. I remain, &c.

WILLIAM JOHN THOMAS, M.R.C.S.
Liverpool, 2d July, 1832.

COLDBATH-FIELDS PRISON.

PAPERS RELATIVE TO THE EXISTENCE OF THE MALIGNANT CHOLERA IN THE ABOVE PRISON.

Communicated by Sir DAVID BARRY, *M.D.*

HAVING lately seen various, and contradictory, published reports, tending to alarm the public mind as to the occurrence of some cholera cases in Coldbath-Fields Prison, and having recently inspected that establishment on two successive days, viz. the 27th and 28th inst., with reference to that disease, I feel myself called upon to lay before the public all the official documents and facts which have come to my knowledge on that subject.

On the night of the 25th of June ult., in a casual conversation with Dr. William Stevens at the College of Physicians, I learned, with no small astonishment, that he had seen upwards of forty cases of cholera in Coldbath-Fields Prison within the preceding 24 hours. Struck with this formidable announcement, I requested permission to see these cases with the Doctor next morning, but could not obtain an appointment with him earlier than for the 27th.

On that day I proceeded to the prison rather in a private than official capacity, accompanied by Dr. O'Shaughnessy, whose "Report on the Chemical Pathology of Cholera" entitles him to such high consideration in everything connected with what has been lately denominated the saline treatment of that disease.

Dr. Stevens conducted us round all the wards appropriate to cholera patients. On leaving the prison, at about half past two o'clock p. m., I observed to him, in presence of Dr. O'Shaughnessy, that I had seen no case of cholera in the prison that day—meaning, as Dr. Stevens appeared to allow at the time, that I had seen none actually labouring under the characteristic symptoms of the disease.

On the morning of the 28th, the following letter (No. 1) was addressed to the governor of the prison, and immediately on receipt of the governor's answer (No. 2), I received the orders of the Central Board to proceed forthwith to examine into, and report officially upon the sanitary state of the prison with reference to cholera, agreeably to the instructions contained in Sir William Pym's letter to me (No. 3, subjoined).

It will be perceived by my letter (No. 4), and by the notes taken in the prison (No. 5), which I transmitted enclosed, that in this inspection I was accompanied by Mr. Maling, deputy inspector general of hospitals, and by Staff Surgeon Dr. Macann, who have also signed the notes (No. 5).

The nominal return marked A reached the Central Board after the inspection just mentioned had been completed. The return marked B was received on the 30th ult., and as no return has been since received from Coldbath Fields Prison, it is evident from this circumstance, connected with the letter from the Privy Council to the governor (marked No. 6), that no new case has occurred in that establishment since the 29th ult.

In transmitting these documents for publication, I beg it to be understood that I am actuated by no wish to impede, and indeed I have no motives to oppose, the full and fair development of the merits of any medicine, but more especially of culinary salt in cholera, having myself given a favourable report of its use in that disease so long ago as the 30th July last year.* No one will rejoice more sincerely than I shall, at the discovery of a really efficient remedy for that dreadful disease.

With regard to the number of the cholera cases which occurred in Coldbath-Fields Prison, from the 2nd to the 26th April this year, or during what has been called the first irruption of the disease into that establishment, I find that twenty-four cases only, and seven deaths, were reported to the Central Board. Not having had an opportunity of seeing any of those cases myself, I shall abstain from making any further remark upon them than this, that the prison, during the period referred to, was repeatedly visited by Dr. Macann, the medical superintendent of the district, and that he has officially reported to the Central Board, that he had every reason to believe that no greater number of cases of cholera than 24 had occurred in the prison at that time. But this gentleman may, of course, be himself referred to on this subject if necessary.

D. Barry.

Central Board of Health,
July 12, 1832.

No. 1.

"Council Office, Whitehall,
June 28th, 1832.

"Sir,—A paragraph having appeared in *The Globe* of last night, stating that the cholera has raged with great violence in Coldbath-Fields Prison, and that upwards of one hundred had been attacked within the last twelve days; I am directed by the Lords of his Majesty's Privy Council to request, that should there be any foundation for the above report, you will cause to be made out by the medical attendant, with as little delay as possible, a nominal list of all persons attacked by the disease within the prison, during the 26th, 27th. and 28th inst., agreeably to the enclosed form.

"(Signed) W. L. Bathurst.

"To the Governor of Coldbath-Fields Prison."

No. 2.

"Coldbath-Fields, June 28, 1832.

"Sir,—I have the honour to acknowledge the receipt of your letter of this date, and in reply beg to acquaint you, for the information of the Lords of his Majesty's Most Honourable Privy Council, that, unfortunately, much of the statement from *The Globe* newspaper is correct. The medical attendant of the prison is not now in the way. In the course of a short time he will return, when your instructions shall be complied with. I have the honour to be, &c.,

"(Signed) G. L. Chesterton,
Governor.

"To the Hon. W. L. Bathurst, &c.

"P.S. The number of persons now labouring under the disease, in its various degrees, is about seventy."

No. 3.

"Council Office, Whitehall,
June 28th, 1832.

"Sir,—I am directed by the Lords of the Council to deliver to you the enclosed letter, from the Governor of Coldbath-Fields Prison, addressed to the Hon. William Bathurst, in which it is stated that the number of persons now labouring under cholera (in its various degrees) in that prison is about seventy; and to request that you will immediately visit that establishment, taking with you one or more medical gentlemen for the purpose of inquiring into the particulars of the disease said to prevail there, and report upon the same to the Clerk of the Council in waiting. I am, Sir, your obedient humble servant,

"W. Pym.

"To Sir David Barry."

No. 4.

"June 29th, 1832.

"Sir,—Agreeably to the instructions contained in your letter of yesterday, I have the honour to state, that I lost no time in proceeding to the Coldbath-Fields Prison, in company with Deputy Inspector General of Hospitals, John Maling, and Staff-Surgeon Francis Macann, with the view of examining the persons said to be labouring under cholera in that establishment. Enclosed I transmit the notes taken by me on the spot. I return the governor's letter to Mr. Bathurst, and have the honour to be, Sir, your most obedient, humble servant,

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* See "Official Reports on Cholera," page 103.

"D. Barry.

"To Sir William Pym."

No. 5.

"Coldbath-Fields Prison,
June 28th, 1832.

"*Notes.*—Visited the wards appropriated to cholera patients in this establishment at half-past four o'clock, accompanied by Deputy Inspector General of Hospitals Maling, and Staff-Surgeon Macann, conducted by the governor of the prison, and two visiting magistrates.

"Saw all the wards in which persons said to be labouring under cholera were treated, and examined individually all those said to be on the sick list, then present.

"*1st Ward visited.*—Nine patients. One man who had been four days under the saline treatment for premonitory symptoms had been attacked this morning, after having been discharged from hospital. A genuine case. Attempts were making, by a young man of colour, to introduce the tube for saline injection into one of the veins at the bend of the arm, under the direction of Mr. Wakefield. Tube could not be introduced, as I learned afterwards. Fluttering pulse, livid and sunk countenance. This case will most probably prove fatal. Another man in this ward, looking thin, pale, depressed, hollow eyes, but good pulse, is under saline treatment. When I saw him about half an hour afterwards, his tongue was cold, with a weak slow pulse. Ward small for the number of beds; close, hot, and oppressive, with a very large fire.

"*2d Ward visited.*—Eighteen persons said to be on the sick list in this ward. Two only present, boys, apparently well. This ward consists of two rooms; the inner a narrow slip. The sixteen not present were said to be out walking.

"*3d Ward called* 'No. 5.'—Six patients on the *Cholera Hospital Book.* Five present. One man complains of constipation of the bowels. One boy has had pain in his side and head, now better; no vomiting or purging. No appearance of cholera in the others at present.

"*4th Ward visited.*—Eleven patients. All present. One boy with slow pulse, and depression of look and spirits; may have an attack in the course of the night. All the others looking well, with no appearance of disease of any kind. Informed by Mr. Wakefield, the surgeon of the establishment, that the diet of the cholera patients consists of arrow-root, tapioca, beef-tea, coffee, and Seltzer-water for drink, *ad libitum*, a wine-glassful at a time.

Convalescent Ward.—Fourteen patients. All looking well.

"*Female Ward, No.* 1.—Nine patients—all looking well. One young woman, apparently *simulating cholera*; warm skin, good pulse and tongue. Said to be a very troublesome perverse character.

"*Female Ward, No.* 2.—Nine patients. One now in mild fever. Said to have been a severe case of cholera. One young woman with bad toothach.

"The two men in No. 1, already mentioned, are the only cases which I saw with the appearance of cholera. Yet the governor assured me repeatedly, that he had shown me *all* the persons considered by the medical gentlemen as labouring under any stage of the disease, and referred to in his letter of this day to Mr. Bathurst.

"The utmost cleanliness, regularity, and discipline, appear to prevail in every part of the prison, as far as I was able to judge; and the visiting magistrates, who went round the wards with us, seemed to be actuated by the most humane feelings, and to devote much time and attention to the health and comfort of the prisoners.

"No new case admitted this day.

(Signed) "D. Barry.
"John Maling.
"F. Macann."

It is almost needless to observe, that no part of the preceding notes or statements is meant to refer to any time or circumstances connected with the patients, anterior or posterior to the moments at which they were seen by Dr. O'Shaughnessy, Mr. Maling, Dr. Macann, and myself.

D. B.

No. 6.

"Council Office, Whitehall,
June 29th, 1832.

"Sir,—I am directed to acknowledge the receipt this day of the return signed by Mr. Wakefield, of cholera cases in Coldbath-Fields Prison, and to request that you will cause Mr. Wakefield to transmit, for the information of the Lords of his Majesty's Most Hon. Privy Council, a *daily* return of such cases as may occur in the prison, instructing him, at the same time, to include in such daily return, cases of confirmed cholera only.

"(Signed) W. L. Bathurst.

"To the Governor of C. B. F. Prison."

"*Nominal Return (A) of Persons attacked with Cholera in Coldbath-Fields Prison, from the 26th to the 29th instant inclusive.*

NAME.	Date of Attack.	Age.	With Diarrhœa, or other Premonitory Symptoms.	Collapse without Pulse.	Remarks.
Jos. Catanach	June 26	25	Confirmed cholera	—	In relating these cases for the information of the Privy Council, I beg leave to observe, that the cholera made its re-appearance in this prison on the 3rd instant, with a degree of malignancy far surpassing the violence of the former attack; and that during the period between its arrival until the present time, upwards of 100 cases have occurred with different degrees of severity, out of which number twelve cases have terminated fatally; but I am happy to add, that the remainder are at this moment in different stages of convalescence, and that the disease appears to be gradually subsiding.
John Moore	ditto	46	Premonitory Symptoms	—	
John S. Gower	ditto	37	ditto	—	
James Sullivan	ditto	18	Confirmed cholera	—	
James Gravener....	ditto	37	Premonitory Symptoms	—	
Thomas Brenner....	ditto	16	ditto	—	
Thomas Green	ditto	19	ditto	—	
Thomas Francis	ditto	17	ditto	—	
Charles Morley	ditto	30	ditto	—	
John Brown	ditto	20	ditto	—	
Georgiana Harris ..	ditto	18	ditto	—	
Ann Morris	ditto	19	ditto	—	
William Ring	June 27	34	ditto	—	
William Thorn	ditto	16	ditto	—	
Godfrey Nokes	ditto	19	Confirmed cholera	—	
Richard Stewart....	ditto	29	Premonitory symptoms	—	
Henry King	ditto	14	ditto	—	
Henry Butcher	ditto	10	ditto	—	
Joseph Jones	ditto	27	Confirmed cholera	—	
Thomas Collins	ditto	24	Premonitory symptoms	—	
John Pratt	ditto	19	Confirmed cholera	—	
William White	ditto	21	Premonitory symptoms	—	
Caroline Thornton ..	ditto	24	ditto	—	
Elizabeth Leach	ditto	19	ditto	—	
Mary Akermann....	ditto	29	ditto	—	
Mary Smith........	ditto	29	ditto	—	
Elizabeth Isaacs	ditto	30	ditto	—	
Mary Bloomfield ..	ditto	22	ditto	—	
Eliza Groves	ditto	26	ditto	—	

HENRY WAKEFIELD, Surgeon.

Return (B).

Ann Smith	night of June 28	21	——	Yes.	Died half past 10 a.m. 29th instant.
Joseph Allen	June 29	32	——	Yes.	Improving.

HENRY WAKEFIELD, Surgeon."

ON THE TREATMENT OF THE

MALIGNANT CHOLERA,

BY SMALL AND FREQUENT DOSES OF CALOMEL.

By JOSEPH AYRE, M.D., *Hull.*

AGREEABLY to the pledge which I gave in my last letter published in THE LANCET, I now proceed to report the result of my practice in cholera. When I first addressed you on the pathology and treatment of this disease, I had had but one case of it under my care, and when I last wrote I had had but three. They were, however, severe cases of it, and they all recovered. Since that time I have had seventy-three additional cases, making the whole number seventy-six, of which sixty have been returned cured, five are under treatment, and four of them out of danger, and ten have died. The whole of these patients were attended at their own homes, and most of them under every possible disadvantage. The treatment pursued was the same in all, and was in strict conformity with the views of the disorder which I have advanced, and with that which I inculcated in THE LANCET as suited to it. The *exclusive object* sought for, has been to restore the secretion of the liver, and the means employed for this purpose have been no less exclusive, and have wholly consisted of calomel and laudanum, given in small quantities and frequently repeated. The dose of calomel was one or two grains, and of laudanum one or two drops, given every five, ten, or fifteen minutes, according to the urgency of the symptoms, and duration of the treatment. The calomel was given alone as soon as seventy or eighty drops of the laudanum had been taken, or the purging and cramps were abated. The calomel was continued uninterruptedly until a substantial abatement of the disease was produced, and the only limit set to its use was in giving it at progressively wider intervals. In several late cases I have trusted to grain doses, with a drop of laudanum given with each, and renewed every five minutes for two or three hours, and then every ten minutes, until a decided impression was made upon the complaint, when they were repeated every twenty minutes, and afterwards every hour, until at length they were discontinued. The average quantity of calomel which I have given has been about eighty grains, and the highest quantity 176 grains. Soreness of the mouth has only occurred in about a dozen cases, and chiefly in those in whom the disease was least severe, or when it was continued after the collapse was wholly removed; for pending the continuance of that stage, no effects upon the mouth are produced by it. At the moment I am writing a patient has just come out from the collapse under this treatment, who has had a suppression of urine during fifty-eight hours, and who has taken nearly 150 grains of calomel in single grain pills, and yet without any soreness of the mouth, or any other inconvenience from it. He is now nearly convalescent. The average duration of the disease in those who recovered under this treatment has not been more than four or five days, and in a very few only were there any consecutive fever. For by the prompt and very liberal use of calomel in *small* and frequently renewed doses,—and it is, I may once more repeat, *indispensable* to their efficacy that they be small,—the secretion of the liver becomes early restored, and that inflammatory reaction in the hepatic circle is prevented, which would otherwise ensue as a consequence of its congested state. Hence, in many instances I have found the most rapid recoveries to occur in patients in whom the disease was most intense. A child (E. Soutter, of Wincombe) five years of age, who was literally "livid, cold, and pulseless" on the Wednesday morning, was walking out on the Saturday, recovered; whilst another child (E. Holdstock, of Machel-street) eleven years of age, whose attack of the disease was far less severe, but whose treatment was at first far too inert, became affected with the consecutive fever as the result of it, and narrowly escaped after a fortnight's illness, and after a plentiful employment of leeching to relieve it. In the former of these cases there was an early and complete renewal of the secretion of the bile, and a consequent remedy to the congestion, and the urgent symptoms which arose out of it,—in the latter the secretion was only very partially renewed, and the result was, an inflammatory state in the hepatic circle, and the consecutive fever as its effect.

There has necessarily been much variation in regard to the duration of the disease prior to my being called to it. In no instance has the disease proved fatal where I have seen it before the collapse has become established and the pulse extinguished. Many have recovered after this has happened, and some even after it has been so several hours. One half of the cases has been in women, and the other half was shared nearly equally by men and children. The most tractable cases have been of children, and the least so of men and the aged of both sexes. When the collapse is complete, and the patient is aged, or a male adult, the stomach in many cases appears insensible to every stimulus, and the calomel has no effect in any way either upon it or upon the complaint. When a favourable impression is produced under these circumstances of extreme collapse, it is generally rather two hours from the commencement of the treatment.

The characteristic marks of the disease that can alone be relied on as such, are the vomiting and purging; but especially the purging of ricey, broth-like, or gruelly motions, with an entire suppression of the urine, if the disease be not arrested. In every case excepting in young children, there have been cramps. In some the cramps were wholly confined to the lower part of the body, which was the case with all the patients that came under my care during several days. The intensity of the cramps furnished no criterion by which to judge of the intensity or danger of the disease. They will abate and be renewed whilst the patient is progressively advancing to recovery. The ceasing of the vomiting and purging of the peculiar fluids, whilst at the same time the weight above the chest is relieved and the pulse keeps up or is restored, are the best indications of the patient's improvement. The renewal of the urine may be delayed for many hours after these signs of improvement, and the case will still terminate well.

In the cases which terminate favourably, the ricey motions become changed into those resembling tar, and these into others less dark, until the natural fæcal condition is restored. In some few cases I have had reason to believe that the motions were tar-like from the beginning, but these were of the milder form of the disease. When the disorder proves fatal, the stools before death are frequently changed from the ricey colour to the appearance of gruel of a reddish hue, and resembling the washing of recent flesh.

In conducting the treatment of this complaint, I have scrupulously avoided the admixture of other medicines with the calomel, that no error might be committed in the conclusions to be deduced from the practice. I have not exhibited a single emetic, or once employed bleeding or leeching, until after the stage of collapse had passed away, and then only on a few occasions. No inconvenience appeared to arise from an unrestrained allowance of cold water, nor any great advantage from much external warmth. I never used the vapour-bath, nor any other means of giving warmth than what the ordinary methods would supply, and indeed so directly have I aimed at restoring the secretion of the liver, and so fully and confidently have I relied on it as the great instrument for removing the whole train of alarming and distressing symptoms, that I have ordinarily neglected to resort to what have been accounted by most as the approved means for affording a temporary relief to them. When the fluid purging was excessive, I have used opiate glysters, and castor oil when the stools became black, and when no further calomel was wanted. I generally have endeavoured to see my patients every two or three hours during the period of danger, and to avoid any delay in beginning the treatment, have provided them at my first visit with a few pills, and the necessary doses of laudanum, until they could be procured from the cholera station.

I shall now detail very briefly the circumstances which appeared to lead to, or influence, the unfavourable termination of the ten fatal cases, that thus a more accurate judgment may be formed of the results of the treatment which I have recommended, and of the obstacles which prevented its greater success. The first case that proved fatal was a poor destitute woman, between fifty and sixty years of age, who had been recently and frequently affected with the lead colic, and who, from living alone, was not discovered to be in the disease until many hours after the commencement of the attack, and when she was livid, cold, and pulseless. She died in two days. The second was the mother of a large and destitute family, who was induced from her own, or other's prejudices, to neglect the taking of her medicines when first ordered. She died in twelve hours. The third case was an infant, who was brought fully out of the state of collapse, and was in a favourable state for recovery, when the mother steadily refused to use a warm-bath and other needful means, and insisted upon its being left to its fate. The fourth was the case of a child twelve years of age, who had been declared by previous medical attendants to be in an utterly hopeless state of collapse, and *abandoned as such*, but who was fully restored from this state. On the fourth day after this collapse, the tongue had become clean and moist, the pulse calm and about 86, sleep natural, skin of a proper temperature and colour, and the secretion of the kidneys restored after seventy-six hours suppression, when an orange and some improper diet were given, and an irritation was set up in the stomach, which was soon communicated to the head, and which carried off the patient in the course of a few days. The fifth case occurred in a most destitute woman with a sick husband and a large family, who refused all assistance during many hours, and was afterwards greatly neglected. The sixth case was a young woman whom I found pulseless, and who for six hours before my visit had been treated profusely with stimulants, and with a most inordinate quantity of opium. The seventh was an old man in whom the disease operated fatally, by bringing into a morbid action some previously existing disease. The eighth was also an old man whom I first saw in the night, and in the most extreme collapse, and who had suffered long and severely from want of every kind. The ninth patient was a very destitute aged woman, who had been in the disease twelve hours, and, living alone, had concealed it. She was subject to fits of slight derangement, and was livid and pulseless, and apparently deranged, when I saw her. The tenth case was a man whom I saw only when dying, and who had been first seen in the night, and prescribed for by a medical apprentice. The poor man was subject to attacks of asthma, and appeared to be dying from an overwhelming congestion of the lungs. He died three hours after my visit.

I will not trespass upon your pages, or upon the time of your readers, by restating the views I entertain of the nature of this disease, or my reasons for the treatment which I recommend for it. Everything I have seen of the disease, and experienced in the treatment of it, serves but to confirm the opinion I had formed of both. The essence of the disease is an interrupted secretion of the bile, and the *remedy is that which restores this secretion.* In the most intense forms of the disease, such a cause, indeed, may seem inadequate to account for its phenomena; but, like every other disease, it has its gradations of severity, and the mildest form of it maintains a resemblance to the severest, proving, as in the instance of the mild and confluent small-pox, their common origin and nature. It has been affirmed that the cases of the disease differ so much as to require a diversified mode of treatment; but the very contrary of this has appeared to me to be the fact, and the only difference in the treatment demanded to be made, is, chiefly in the frequency of the times for repeating the calomel, and the greater or less occasion for the subsequent employment of leeching and the antiphlogistic remedies. In every case there is a repressed secretion of bile, and in every case there is a necessity for calomel to restore it. Emetics and even stimulants may occasionally, and, as it should seem, by accident, restore it, and these last may sometimes even support the patient through the struggle, and thus appear to be the remedy. But their action is indirect, and their success is, in a measure, accidental, and must generally fail, whilst calomel, as possessing the specific property of promoting the secretion of the liver, is at once a medicine that claims not merely to be united with others in the treatment, but to be relied on as a specific in this complaint, and to be employed to the exclusion of all others. Such indeed has been the course I have pursued with it, and such has been the course pursued with it by my intelligent young friend Mr. Sharpe of this town, who has followed my method in every thing, and who having seen and especially attended with me nearly all the cases I have had to treat, assures me that his own experience in the result of the practice with the cases he has had privately to treat, completely corresponds with what he had observed with me, and establishes in his mind the same conviction which is fixed in mine, that calomel given in minute and frequently renewed doses, is endowed with a property of controlling and arresting it, which no other medicine, or combination of medicine, has ever had assigned to it, or will, if I mistake not, be ever found to possess.

Hull, June 28, 1832.

CASE OF

MALIGNANT CHOLERA

SUCCESSFULLY TREATED BY VENOUS INJECTION.

Delivery of a Child on the 3d day.

To the Editor of THE LANCET.

SIR,—After writing to Dr. Abercrombie of Edinburgh, regarding the result of Dr. O'Shaughnessy's remedy (whom I had the pleasure of meeting in Newcastle), I have put it to the test, and have now much satisfaction in stating to you that I have been successful in my first case. It is now fourteen days since the operation, and the woman is perfectly well. I may send you the following outline.

Mary Cunningham, aged 36, and her husband, William Cunningham, both originally from Ireland, were admitted into the cholera hospital in the lowest state of collapse. I saw nothing for them but to try the transfusion; and I endeavoured to inject by means of the apparatus used for injecting subjects, but could not succeed, accordingly I left by the mail-coach for Perth, aroused Dr. H. M'Farlan about one o'clock in the morning, and obtained the loan of an injecting or transfusion apparatus for twenty-four hours. I lost no time in returning; the man was gone, but his wife was still alive. Dr. Bell saw her about half-past eleven p.m., and said she would not live much longer than her husband. He died about one a.m., and Mr. Morris, my assistant, had great doubts all night of her surviving till my return. I made an incision with a lancet through the integuments alongside the median basilic vein, introduced a short probe beneath it, then made a small aperture in the vein itself sufficient for the introduction of the small tube, and injected very slowly ℔iij of the solution, ʒi mur. sodæ, ℈i carb. sodæ, dissolved in ℔iij of filtered rain-water, heated to 110° and afterwards filtered, in the space of half an hour, keeping up the temperature all the while.

She had the following appearance previous to injecting: she lay on her back pulseless, breathing slowly and heavily, eyes sunk, glazed, turned upwards, and the eyelids half closed, and surrounded with the livid areolæ; mouth open; tongue cold and white; face collapsed and dusky; surface of body cold, and covered with the clammy exudation; hands cold, livid, and sodden, as if they had been soaked in water for a length of time; feet cold and livid; voice husky, and scarcely audible; and, in short, as bad a case as I have seen; and out of which I have seen almost none recover. Towards the conclusion of the injecting her pulse returned; eyes lost the glazed appearance; areola less marked, and her whole countenance brightened up; her tongue became warmer, and she moved her jaws; the surface of her body became warmer, as also did her feet, aided by hot sand-bags. She continued in this state for nearly three hours, when the effects began to wear off, and into the same opening I injected other ℔v of the solution. In a very short time after this second injection she became, I may say, convalescent all at once, and with little variation has continued so ever since. During these fourteen days she has had occasional vomitings and purgings. She has had slight comatose symptoms, which were relieved by means of a blister. She had great pain in her chest, and thick, queer, purulent sputa, but all these untoward symptoms have left. I may mention, that in her left eye, which was much inflamed, there is now a small ulceration on the cornea, a little below, not opposite to the pupil. She is now in the convalescent ward, but desired to keep in bed, although she can walk well enough. She eats and drinks well; has natural stools without medicine; passes her urine freely; and sleeps soundly during the night, and is cheerful during the day.

I have as yet not mentioned one very important occurrence, viz., that she was injected on Thursday morning, 21st June; and that on the following Saturday, at three p.m. I delivered her safely and easily of a dead girl (6th month). The membranes and placenta came away at the same moment; the placenta appeared very dry, and contained dark venous blood; the child was very livid,—this, of course, was not in favour of the new remedy; but, notwithstanding, she has done well. She says she was well aware that the child died within, soon after she was attacked.

As we have made it a rule to remove no moribund patients into our hospital but such only as there is some hope of life in, however distant, we refused to admit the subjects of the above case. Indeed the medical gentlemen who visited them first, refused to grant a line of admission, considering them both too far gone; but their landlord insisted they should rather die on the way to the hospital than on his property, and accordingly we were obliged to take them in, as we are often compelled to do. I have repeated this remedy since, but not with like success.

I remain, with much respect and esteem,
David C. Carruthers, M. D.
Dundee, 4th July, 1832,
Cholera Hospital.

BARON HEURTELOUP'S OPERATIONS FOR THE DESTRUCTION OF CALCULI IN THE BLADDER BY PERCUSSION.

CORRESPONDENCE RELATIVE TO THE LETTERS OF MR. COSTELLO, INSERTED AT PAGES 362 AND 430.

From Mr. Edwards *to the Editor of* The Lancet.

Sir,—In justification of a postscrip which I appended to a memoir on the evacuating catheter, lately published in The Lancet by Baron Heurteloup, I beg to enclose for insertion the copy of a letter written to Baron Dupuytren by Dr. Hume and Mr. Brodie, and in which you will find my statements authenticated. I should not have troubled you with this communication, were it not for the publication lately of an extraordinary letter, in which are repeated the calumnies to which the author formerly gave birth. I am, Sir,

Your obedient servant,
D. O. Edwards.

Dr. Hume *and* Mr. Brodie *to* Baron Heurteloup.

"Sir Baron,—Dr. Heurteloup having acquainted us that an unfaithful account of the operation which he performed in our presence on Colonel Rankin has been transmitted to the Institute of France; and that you are appointed especially to inquire into the progress made in England in the treatment of calculous patients, we take the liberty, at his request, of addressing to you a few observations upon the history of this case.

"We certify that no attempt was ever made or contemplated, to extract from the patient's bladder a broken instrument, but that simply the branches of the *instrument à percussion*, which the Baron had then recently begun to employ, remained three lines and a half apart, a circumstance which determined the operator to make an incision into the urethra, in order that the branches should be passed out through the aperture, and forcibly closed and withdrawn, without dilating the urethra excessively. In fact, the branches were brought together, and the instrument was taken out, without exciting the slightest inconvenience. If the lateral section was performed immediately after the disengagement of the instrument, it was not because it was thought *urgent* to do so, but solely because it was considered most advantageous to the patient.

"We add furthermore, that if lithotomy was resorted to on this occasion, it was purely at the suggestion of Baron Heurteloup; and that if this operation had an unfortunate termination, it ought to be attributed in a great measure to the advanced age of the patient, for he survived the operation more than two months, the wound very nearly healed, the urine passed freely and entirely through the urethra, and there was no other cause of death than extreme debility.

"We affirm, finally, that although Baron Heurteloup occupied no more than *one* or *two* minutes in attacking the large stone which the bladder contained, this stone, notwithstanding its toughness, was broken into many fragments when we proceeded to its extraction.*

"We believe we are doing but an act of justice to Baron Heurteloup, in saying that he conducted himself throughout this affair in the most honourable manner, frequently visiting Colonel Rankin in his long illness, and being always well received by him.

"We have the honour to be, Sir Baron,
"Your very humble servants,
(Signed) "J. R. Hume, M.D.
"B. C. Brodie.

"To M. le Baron Dupuytren, D.M. P.,
Chirurgien en Chef de l'Hotel Dieu,
Membre de l'Institut de France, &c. &c."

From Mr. Goodwin *to the Editor of* The Lancet.

Sir,—On the 22nd of June I received the following letter from the Baron Heurteloup, and I rely on your justice in inserting it, as well as my reply to the statements of Mr. Costello.

To George Goodwin, *Esq.*

"My dear Mr. Goodwin,—Might you not have been mistaken in the particulars which you gave me respecting your case? You told me that the surgeon to whom you addressed yourself previously to your consulting me had introduced, *three times*, an instrument into your bladder, and that each time he was occupied from a quarter to half an hour in searching for your stone, which he did not succeed in seizing. Although I mentioned no names in drawing up your case, Mr. Costello, who announces himself as the surgeon to whom you first addressed yourself, has just published in The Lancet of Saturday, June the 23rd, page 363, a series of statements relating to you, which contradict the information given me by yourself and Mr. Hodges your medical attendant. I have not the least doubt, that if the facts, such as you related them to me, were incorrect, you would give them in their right light, and that if, on the contrary, what you stated was correct, you would, as a matter of duty, oppose a contradiction to what so nearly concerns yourself. I hope your health continues good. Believe me, Sir, ever yours truly,
"Baron Heurteloup.
"1, Vere Street, Cavendish Square."

As soon as I had read this, I immediately procured The Lancet alluded to, and found in a letter, addressed by Mr. Costello to the Academy of Sciences of Paris, the following passage:—

"The second patient who left me for M. Heurteloup had paralysis of the bladder, and a stone which held a fixed situation in that organ. This patient had been cut by Mr. Brodie three years before. I made an attémpt with Civiale's instrument; I experienced no difficulty in seizing the stone between two of the branches of the forceps, and detaching from it considerable portions which my confrère must have seen, but I was not able to dislodge the stone from its fixed situation beneath the neck of the bladder, and which appeared to me to be that of the old cicatrix. The attempt was renewed with a similar result; each attempt occupied about *five minutes.* I explained the difficulty to the patient, and proposed to dislodge it by means of Hunter's two-branch forceps, he was however intimidated by officious observations, and he placed himself under the care of M. Heurteloup, whom I had previously proposed to call in consultation. The indication of dislodging the stone, whose fixity in this case formed the sole obstacle to its comminution, was fulfilled by the means which I had previously proposed. The impediment once removed, of course the cure became easy. This patient was restored to health, at which I sincerely rejoice; and far from thinking that I had failed, he came afterwards to thank me for my attentions to him. It was admitted that my confrère and myself were of the same opinion as to the difficulty of the case, and the means of obviating it. So much for the imperfection of the means employed by me."

This case is mine, and as Mr. Costello has made statements not founded in fact, I consider myself bound in justice to the Baron Heurteloup, as well as by my regard for truth, to offer you a positive contradiction of them. I was cut for the stone by Mr. Brodie in May 1829, and in June 1831 I had a return of the same complaint,

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* At that time the instrument of Baron Heurteloup had not assumed its present form. On this account the branches yielded a little to the concussion. In fact the instrument employed upon Colonel Rankin was nearly a first essay, and therefore little more than a model. Since that time a series of 62 have been made, each more complete and perfect than its predecessor; and the weight of the hammer being determined, and the probable resistance of the stone calculated, the whole strength of a man be applied in striking, without any fear of the beak giving way.—D. O. E.

I consulted Mr. Brodie, but having heard that there was a safer mode of cure than the use of the knife, practised by two gentlemen in the profession, both strangers to me, I inquired about it, and was recommended by a friend to Mr. Costello. Upon this circumstance being known to Mr. Brodie and Mr. Morrat, my medical attendants, they both, for reasons best known to themselves, withdrew, and I was left alone, waiting the return of Mr. Costello to town. When he arrived, I thought it right to have the assistance of another professional friend at these operations so important to me, and I called in Mr. Hodges of No. 6, Upper North Place, Gray's Inn Road, who attended at all of them, *three* in number, and not *two* as Mr. Costello says, and who can attest to the truth of my statement, as he kindly visited me every day.

In the first place it is difficult to imagine (as Mr. Costello affirms) that my stone *held a fixed situation in the bladder*, for the Baron Heurteloup, before several medical gentlemen, immediately and with the greatest ease seized and comminuted it; and I have it now in pieces and powder in a box.

In the second place, each of the *three* sittings did not, as Mr. Costello also gives us to understand, in contradiction of what the Baron had said, *occupy five minutes*, but lasted from a *quarter* to *half* an hour, as Mr. Hodges, who was present, can certify as well as myself, who, from the anguish and torture I was enduring, should be inclined to estimate the duration of the sittings at a much higher rate.

In the third place, it is impossible that the Baron should have *seen the considerable portions* of stone which Mr. Costello says his operations caused me to evacuate, for he never, to the knowledge of Mr. Hodges or myself, extracted any from the bladder by his instrument. I never voided any thing like broken pieces of stone after the sittings; and, finally, in point of fact, Mr. Costello never, even with the sound, succeeded in feeling the stone, nor did he ever seize it, as he affirms, between the branches of his instrument; at least we saw no proofs of it, and the manner in which he manœuvred and raked about in my bladder, tends to make us believe that all his attempts to *seize*, not to say *feel*, the stone, were unsuccessful.

In the fourth place, Mr. Costello never proposed, as his letter would lead any one to believe, *to call the Baron Heurteloup in consultation*; but, on the contrary, he said he did not consider himself justified in making any further attempt without the assistance of M. Civiale, and strongly urged me to let him send to Paris for that gentleman, to which I would not consent.

In the fifth place, I did not *go to thank Mr. Costello for his attentions to me*, as he asserts, nor have I ever seen him since. I sent Mr. Hayward, a respectable friend of mine living in North Place, to remunerate him for his attentions to me, and I declined receiving any further visits from him. This step Mr. Hodges fully concurred in. What passed between Mr. Costello and Mr. Hayward, I understood at the time to amount to an acknowledgment on the part of Mr. Costello, of a complete failure.

In the sixth place, Mr. Costello, between the second and third sittings, did propose to dislodge the stone from beneath the neck of the bladder, where he assured me it was situated, with Hunter's forceps, but he came to operate for the third time without that instrument, and the sitting was considerably prolonged. I was then induced to address the Baron Heurteloup, which I did through Mr. Brodie, who was kind enough to introduce him, and not from the *officious observations* of any other person, as Mr. Costello takes the liberty of affirming. I had, indeed, heard from high authority of the Baron's talent and skill, and which I have good reason to know were not exaggerated. He made use of his *new curved instrument and hammer*, and immediately seized, not from *beneath the neck of the bladder*, but from the lateral portion on my right, the stone, which he as soon comminuted, without giving me any pain worth mentioning. I am now perfectly well, and free from anything like a painful sensation.

Such is a true and accurate statement of what I underwent, and such as my medical attendant Mr. Hodges, residing at No 6, Upper North Place, Gray's Inn Road, will, I am sure, feel pleasure in certifying.

I am sorry that Mr. Costello, by deviating so widely from the truth in what concerned me, should have placed me in the painful necessity of bringing to light such gross inaccuracies, and publishing the facts correctly. I considered myself bound in justice to write this letter, and I beg, Sir, that you will give it a speedy insertion.

I am, Sir,
Your obedient humble servant,
George Goodwin.

Chapel Street, Grosvenor Place,
June 25, 1832.

P.S. Since writing the above, I have submitted it to the perusal of Mr. Hodges and Mr. Hayward, and they have given me leave to, say, that they both fully agree in the above statement of facts. G. G.

Twelve o'clock.

From Mr. Briggs *to the Editor of* The Lancet.

Sir,—The letter inserted by Mr. Costello in last week's Lancet being evidently not written in a scientific point of view, it would be quite unnecessary to answer it, did it not contain a numerous collection of incorrect statements, and misrepresented facts. As Baron Heurteloup's pupil, which great advantage has enabled me to become well acquainted with all that relates to his operations, I should justly expose myself to censure if I allowed to pass unnoticed, inaccurate publications tending to depreciate a mode of operating, the advantages of which are becoming daily more and more evident by the success accompanying its application. Allow me, therefore, to offer a few observations in reply to Mr. Costello's letter. I consider it my duty to take this upon myself, although it is not the Baron Heurteloup's wish that any notice should be taken of it.

The cases of several patients to which Mr. Costello alludes in his letter, are as inaccurately, as they are incompletely, related. That of Colonel Rankin was so very incorrectly given, that Mr. Brodie and Dr. Hume, the two gentlemen who were present during the whole of the operation, were kind enough to write a letter to the Academy of Sciences, in which they contradicted the fallacies of Mr. Costello, and by giving to the gentlemen of the Institute accurate information, showed in a true light the motives which induced Mr. Costello thus to speak of an unfortunate, but simple and instructive, surgical case. Mr. Costello's letter, however, although forwarded to the Institute, was not, as he most likely had anticipated, even perused; for the secretary being accustomed and commissioned to read such papers only as are useful to science, soon perceived the tenor of this letter, and it was laid aside. It is due to Mr. Costello here to remark, that he has himself, in a note, rectified, in some degree, the inaccuracy of his assertions touching this case, but not having also transmitted this information to the Academy of Paris, as he should have considered it his duty to do, are we not justified in concluding, that *he knows that the statement is incorrect?*

With respect to Captain W——'s case, to which Mr. Costello alludes, he is equally inexact in his statements, and it would certainly have been more just and more worthy of men of science to have obtained correct information before publishing a case in which the name of another professional gentleman was concerned. Mr. Costello should have gleaned his particulars from Mr. Money and Mr. Green, whom he knew visited, conjointly with the Baron, Captain W——, whilst he was labouring under a disease unconnected with the stone, and which, if not entirely, at least in a great measure, caused his death. The Baron Heurteloup considered it his duty to direct the attention of Mr. Money and Mr. Green to the manner in which Mr. Costello had represented this case, and these gentlemen will, perhaps, think it right to induce Mr. Costello to retract what he has said, and present the case in its true colours.

There is little to say on Mr. Goodwin's case, the second which Mr. Costello mentions; it is one in which the Baron Heurteloup said, that the *perce-pierre* had not been and could not be successfully employed, and also the case in which Mr. Costello introduced *three* different times the straight instrument above mentioned into the bladder, without being able to seize the stone, although each of the three attempts was of very long duration. The Baron was furnished with these particulars by Mr. Goodwin, the patient, and they were corroborated by the testimony of Mr. Hodges, his medical attendant, who saw all that was done.

With respect to Mr. Stockbridge's case, I need make no comments on it; it speaks for itself. A straight instrument could not be introduced into the patient's bladder without violence, which in this operation, above all others should be avoided. The Baron Heurteloup, who, from talents, long experience, and delicate tact, must certainly be as competent as any one to introduce into the bladder either straight or curved instruments, advised the patient not to allow any further attempts to be made with a straight instrument, but to wait until he had constructed a curved one, which at that time engaged his attention. Mr. Stockbridge, however, by some means or other, was deterred from following this wholesome and reasonable advice; in his excusable impatience to be relieved, he was persuaded to consult Mr. Costello, who unhesitatingly proceeded to introduce the *perce-pierre*, a straight instrument, which he employs in all cases. He succeeded in passing it into the bladder, I should think by *force*; for the Baron Heurteloup's acknowledged skill and talent could not have failed in the *usual* application of this instrument. Mr. Costello says, that the instrument went into the bladder easily. It may be so, for it is always *possible*, with a metal instrument, to overcome obstacles presented by soft and yielding organs. But was it prudent to do so on this occasion? It appears not, for death ensued, as Mr. Costello candidly confesses. This case furnishes undeniable proof of the utility of a curved instrument, which, however, Mr. Costello does not seem at all disposed to admit.

With respect to any other cases in which the Baron Heurteloup accuses the *perce-pierre* of being of insufficient and unsuccessful application, Mr. Costello is not the operator alluded to, although I don't know why he attributes to himself all the cases mentioned; but he must certainly be aware, that he is by no means the only surgeon who attempts to operate with the *perce-pierre*.

Allow me, Sir, further to add to these observations:—

1st. It is impossible to admit that Mr. Costello has treated successfully with the *perce-pierre*, since his arrival in England, the number of patients he names; for it is natural to suppose that had he effected these cures, he would have published them in detail, since that alone can justly establish the reputation of a surgeon.

2ndly. Mr. Costello never having seen the curved instrument employed, never having even *seen it* as constructed by the Baron, and, finally, never having received any correct information respecting the manner of making use of it, cannot in justice pass any remarks on it, nor can his opinion respecting it be considered of any more weight than that of any person who should panegyrise or detract from the labours of Mr. Costello, with which few or none happen to be acquainted.

3rdly. It is difficult to conceive how Mr. Costello, in no way distinguished by scientific labours, and comparatively young as to experience, should, in so positive and unrestrained a manner, give his opinion on this operation, and on the means of performing it, whilst other gentlemen, whose talents and long experience have placed them at the head of their profession, continue silent, having too much justice and good sense to condemn or criticise the new system, until they have studied its advantages and defects.

4thly. I would, in conclusion, remark, that a letter, written by a surgeon to any learned body for the sole purpose of preventing a professional brother from receiving the reward of long and arduous labours, supposing every word of the letter to be of the most scrupulous exactness, would be deemed a rather indelicate action, and certainly unworthy of a professional gentleman; but when the facts related in the letter are untrue, such conduct requires an appellation which, out of respect for you, Sir, and your readers, I refrain from applying. The following is a

Statement of Captain W.'s Case, written by himself.

"Fifty-nine years of age. Good health through life, till June 1828, when, after horse exercise, passed bloody urine, which disappeared till October, when horse exercise again produced slight hemorrhage, with pain in the glans penis, making small quantities of urine seven or eight times during the day, and four or five times during the night. Horse exercise discontinued till Oct., 1829, when it again produced profuse hemorrhage for nineteen days and nights. Horse exercise discontinued; great irritation and pain in making water, which at times suddenly stopped. Was sounded the 29th of May, 1830, when calculus was discovered in the bladder. Less irritation, discharging urine four or five times during the day, and generally three during the night, and from *four* to *eight* ounces at a time, *healthy* and *clear*.

"June 25. Mr. Costello passed through my place of residence (Wrexham), when I was strongly recommended to submit to an operation by him, which was granted. Was sounded, and the bladder pronounced free from the slightest organic disease, and in every way prepared. A sitting took place* on the 26th June. The stone seized five times; escaped every time; the *perforator* touched it. The stone was declared to be ten lines. This attempt produced slight hemorrhage for three days, when it was succeeded by fetid and purulent urine, which is now subsiding, but is not even now voided in greater quantities than two ounces, and at times much less; attended with great pain, by which the head is greatly affected. I must remark, that the above attempts produced violent fever for some time, with declining health, *previous to which it was unimpaired.*

"From the period of its being ascertained that Capt. W. had stone, he had fully determined to proceed to London, and apply for relief to Baron Heurteloup; but a friend addressed Capt. W. to say that Mr. Costello was soon to arrive at Liverpool, and at the same time recommended Mr. C. as a careful operator. From these favourable accounts Capt. W. considered it would be a great advantage to him to have the operation performed at his own house, and yielded to so *apparently* favourable an opportunity. Mr. Costello arrived at Wrexham on the 25th of June, and sounded Capt. W. that evening, and declared to him that he never had a patient better prepared, and in other respects more favourable for immediate operation. Judge then of our surprise, when, on the 3rd of July, in answer to a letter from Captain W.'s medical attendant, Mr. Costello states the operation had failed for want of due preparation. Mr. Costello having intimated his intention to return by Wrexham, Captain W. addressed him, expressing his and all his friends' surprise at the contents of his letter, and requesting him not to do so on his (Captain W.'s) account. Captain W. has not therefore heard more from Mr. Costello.

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* Not an exploration.—T. B.

"Capt. W. considered it necessary to make this statement known to Baron Heurteloup as the professional gentleman chosen by Capt. W., from whom, by God's assistance, and the Baron's unremitting care, he yet hopes to experience a cure from so dreadful a complaint.

"THOMAS WATSON.

"London, August 24th, 1830."

Such is the letter which I have in Capt. W.'s own handwriting, and which I feel it my duty to publish, as Mr. Costello has not thought fit to show this case in its right light, especially as far as he himself was concerned. Allow me also to answer the following passage in Mr. Costello's letter relating to Capt. W.

"The first of these I visited in the country, on my way to Edinburgh. After having made an *exploration*, it was settled that he should go to London in order to have lithotrity performed on him. On his arrival in town I had not yet returned. The patient placed himself under the care of my confrère, who operated upon him, and he died."

This passage being evidently written to induce such as read it to believe that the Baron Heurteloup's operation caused the death of the patient, I think it right to combat an insinuation which has so deplorable a motive by a testimony of unexceptionable validity. It is a letter addressed to Mr. Money, in which Mr. J. H. Green explains why he was called by Baron Heurteloup to visit Captain W.

"46, Lincoln's Inn Fields,
July 9, 1832.

"Dear Sir,—In answer to your inquiries respecting the state of the late Capt. W., previous to your attendance, I beg to say that I saw him with Baron Heurteloup after the first and only operation of lithotrity, and that he was at that time labouring under a complaint which had no connexion with the stone or the state of the bladder. I would add that the bladder was then in a favourable condition, and did not appear to have suffered in any degree from the operation. I am, dear Sir, respectfully yours,

"JOSEPH HENRY GREEN."

"— Money, Esq."

From this letter, then, we may conclude that Capt. W.'s death was not caused by the operation of lithotripsy. A few remarks, however, on what Capt. W. underwent, will further prove this.

This patient, after having undergone *an operation of lithotrity* at his own house at Wrexham, which was unsuccessful, came to London to consult Baron Heurteloup. His health was greatly impaired by the attempt of Mr. Costello, who thinks proper to denominate long and painful operations simple *explorations*. The Baron seeing Capt. W. in so unfavourable a condition, at first refused to operate upon him. For nearly *four* months the patient entreated the Baron in the strongest manner to undertake the treatment of his case, and it was only after Capt. W. had called three different times into consultation Messrs. Brodie, Money, Green, or White, that it was decided that on account of the anxious wish of the patient, the Baron could not do otherwise than subject him to his mode of treatment.

The patient was operated upon once with the *trois-branches à virgule*; two calculi were seized and excavated by the *virgule* with rapidity and facility, and in presence of Dr. Granville. No unfavourable symptoms appeared after this operation, but the Baron having ascertained that there existed in the bladder two oval stones of large dimensions, resolved not to proceed with the same instrument, as the patient could not have borne the number of operations which the means he then possessed to break down such calculi rendered necessary.

The Baron was at that time constructing his *percuteur courbe à marteau*, and he told Capt. W. that he would not continue the operation until that instrument was completed, for the size of the stones in his bladder required rapid and efficacious means. The patient waited therefore, and was in as favourable a state as could be wished for; he made water at long intervals, the urine was clear, and the Baron was on the point of proceeding in the comminution of the stones with the *percuteur*, when Capt. W. was seized with an acute pleuritic affection which caused his death. Shortly after the pleuritic symptoms declared themselves, Mr. Green was called in consultation, who found the patient in the state described in his letter. He died three months after M. Heurteloup's operation.

I should have preferred making no use of a paper which I have in my hands, but Mr. Costello, careless of sacrificing truth to the accomplishment of his object, compels me to commit it to print. This paper, which was written and signed by Capt. W. himself, and given to the Baron Heurteloup, contains an account of all that he underwent before he consulted the Baron. This letter will give proofs, too evident, that Mr. Costello did something more than *merely explore* the bladder of the patient.

I am, Sir,
Your most obedient servant,
T. BIGGS.
Vere Street, Cavendish Square, July.

IMPORTANT

MEDICO-LEGAL TRIAL.

MRS. KINNEAR *v.* BORRODAILE AND OTHERS,

On the part of the Rock Insurance Office.

ALLEGED SELF-POISONING BY LAUDANUM—SINGULAR MEDICAL EVIDENCE.

THIS important cause was tried on Friday the 6th July, before Lord Tenterden and a special jury, in the Court of King's Bench, Guildhall, London. The plaintiff, the relict of the late Thomas Kinnear, Esq., banker, of the firm of Kinnear, Logan, and Co., sought to recover 2000*l.* from the defendants, as the amount of a policy of insurance, effected on his life by her deceased husband, and the payment of which amount had been resisted by the Rock-office, on grounds which will be sufficiently explained in the opening address of Sir James Scarlett, who conducted the defence. Messrs. Campbell and Kelly were counsel for Mrs. Kinnear.

Sir JAMES SCARLETT commenced his address by stating, that one issue alone was before the jury in this cause. They had to determine whether the death of the late Mr. Kinnear had been effected by himself, or whether he died by the natural visitation of Providence. The plaintiffs would not have to prove the latter, for in consequence of express stipulation in the policy, the burthen of the proof of suicide lay on the insurance company. The late Mr. Kinnear was a gentleman of great attainments, talents, and respectability; in possession of considerable fortune, and in the management of an extensive concern. There was, however, reason to believe, that recent transactions in the funds had embarrassed his private resources. Mr. Kinnear had a house in Cornwall Terrace, Regent's Park, and he met with his death on the 20th October, 1831. An examination of the body was made, but the medical report was so little conclusive, as to demand the further investigation which had subsequently been resolved on, as well by the defendants in the present case, as by the directors of the National-office, with whom Mr. Kinnear had also effected insurances, and to a still greater amount. Sir J. Scarlett then proceeded to say, that his next object was to show that Mr. Kinnear died by his own hand. In doing so, he laboured under the difficulty of having to derive his testimony from the witnesses of the adverse party. He should, therefore, have to rely much on the strong facts afforded by circumstantial evidence. Mr. Kinnear was a member of the Athenæum Club; he always slept at home, which at the time of his death was under the hands of the workmen, as the premises were in course of repair. Mr. Kinnear and a servant man named Thomson alone slept in the house. Mrs. Kinnear and her family were temporarily resident at Brighton. Mr. K.'s bed-room was on the first floor; on the same floor was the library which, at the time of the events concerned in this trial, contained several unopened packages of furniture. There were also on the same floor two drawing-rooms communicating with the staircase. Below there were four rooms unfurnished. Mr. K. was a temperate man and an early riser; he was accustomed to give directions to his workmen in person at an early hour. It was here proper to state that Mr. K. was in the habit of investing large sums of money in his own name in the foreign funds, and, often, the money employed was the real property of various correspondents. Amongst others, Sir M. Ridley, the eminent banker at Newcastle, had intrusted Mr. K. with money for this purpose, to be invested in the French funds in Mr. K.'s name. This was about the time when the glorious revolution, as it was called, of July 1830, occurred, and, in consequence, Mr. K.'s capital became largely involved. On the 19th of October, Tuesday, he had expected from Paris documents from the house of Rothschild, representing the sums of money employed for the Newcastle banker, but these documents did not arrive. On that day Mr. K. wrote a letter to Mr. Rothschild of London, directing "*that in the event of his absence, the rentes in his*, Mr. K.'s, *name, should be delivered to Sir M. Ridley, whose property they were.*" This letter was not however delivered, but after it was written in the counting-house was placed in the iron safety chest, with other securities of Mr. Rothschild's relative to the 3 per Cents. The letter was thus placed as a testimonial respecting Sir M. Ridley's claims. Now from this circumstance it was manifest that Mr. K. contemplated some event before the return of the *rentes*, which were daily expected, and accordingly his death took place on the following day, the 19th of October. That ordinary absence was meant by Mr. K. was extremely unlikely, in consequence of the indisposition of Mr. Logan, the only other partner in the firm then resident in London. Upon that Tuesday then, the 19th of October, Mr. Kinnear dined at the Athenæum Club, and returned home at a quarter before nine p. m. Thomson, his servant, had however made some arrangement for his dinner in the library adjoining his bed-room. There was a decanter on the table, containing about half a bottle of sherry, and there were also a tumbler and two other glasses. Mr. Kinnear soon retired to his room, and the decanter and glasses were conveyed into the room too, whether by himself or servants the learned counsel could not undertake to prove. There was reason to believe that before retiring he told the servant he was about to take a draught, and it was certain that Mr. K. ordered him not to call him at the usual time in the morning. Thomson then went below stairs, and laid down, but at a little past twelve heard a noise as if of some one walking, and of a window-shutter bolt falling, in the parlour *on the floor below* Mr. K.'s bed-room. Thomson rose, took a light, went to the hall door, found it fast, and on looking round perceived his master on the staircase, undressed, and going up stairs, Mr. K. asked, "why he, Thomson, was coming in so late." Thomson replied, "he had not been out, but that hearing a noise he came up to see if there was no one in the house." Mr. Kinnear said, "Come up-stairs and see." He went up-stairs, all was right in the bed-room and library; his master had got under the bed-clothes, and Thomson spoke to him, but got no answer; he spoke again, and no reply was made. Thomson then closed the door to, and returned down-stairs, but did not go to sleep, and in a little time he again heard Mr. K. go towards the water-closet *on the third floor*. Thomson then again went up stairs. His master shortly came out of the water-closet, and in reply to an offer from Thomson, said, "You may leave the light if you please." He then went into his bed-room, and Thomson heard no more of him for the night. Next day workmen came as usual at 6 a. m. At 7, finding that Mr. K. did not appear as usual, they asked the servant to call him, but he refused. At 10, a workman on the first floor heard a groan, went to the door of Mr. K.'s bed-room, and found it locked inside, applied his eye to the spindle hole,[*] and saw bed-clothes rise and fall as if some one breathed heavily beneath. The servant was advised to open the door forcibly, but he refused. The workman again shortly returned, and saw and heard the same thing. The servant now became frightened; he went to Mr. Trotter, a friend of his master, who came in a short time. A ladder was placed against the bed-room window; Thomson entered, immediately opened the door from within, and admitted Mr. Trotter, when Mr. Kinnear was found breathing his last. A surgeon, Mr. Lovegrove,[†] was hastily sent for, and found Mr. K. warm and moist, but life was gone. The wine had been used, and something remained in the tumbler and wine-glass, which Mr. Lovegrove tasted, and immediately desired the room to be cleared; a dark stain, it should be added, was noticed about the lips, and the pillow was also stained a little. No phial was found in the room, nor any trace of vomiting or other evacuation. Sir James then entered upon a review of the moral and circumstantial occurrences thus narrated, and especially noticed Mr. Kinnear's perambulation of the lower apartments, and of a window-shutter having been opened in the front room: he observed, that the jury would have to weigh the probability of Mr. K.'s having come down-stairs in his agitation *after taking laudanum* to dispose of the phial which contained it. The learned gentleman said, that he understood that laudanum produced the appearances found on the examination of Mr. Kinnear's body, none of which were consistent with the supposition that he had ruptured a blood-vessel in the stomach, as was loosely stated in the medical report. No trace was found of any such rupture; and had such taken place, it could not have caused death without producing vomiting. Besides, the blood-vessels of the head were exactly in the condition that occurs in narcotic poisoning, and in a state precisely contrary to what they would be after the rupture of a blood-vessel in the stomach. The body was also livid, and such was the case after poisoning by opium and its preparations. Had Mr. Kinnear been suddenly taken ill, he would have rung the bell. In short, Sir J. Scarlett was prepared to produce the highest medical testimony in support of these statements, and in opposition to the opinions and conclusions of the individuals who drew up the report on which the coroner's jury returned the verdict of "Died a natural death," which report he believed the medical men themselves afterwards were inclined to modify. The learned counsel, in conclusion, affirmed that it was no part of his instructions or inclinations to throw obloquy on the memory of the deceased. He did not mean to defend or sanction the commission of suicide; but history afforded abundant evidence that great and virtuous and honourable men had, under certain combinations of circumstances, preferred death by their own agency to an odious and dishonourable life.

The following evidence was then adduced:—

Mr. ROBERT LOGAN was a partner of the late Mr. Kinnear. At the time of Mr. Kinnear's death the third partner, Mr. Campbell, was away from England for the benefit of his health; and his Mr. Logan's health was so poor, that a short time before Mr. Kinnear's death it kept him from business; Mr. Kinnear consequently gave daily attention to the extensive concerns of the firm, and his absence would be extremely inconvenient. Besides the partnership, Mr. Kinnear had some private transactions, but to no great extent. He was indebted to the firm about 9000*l.*, but his estate is not now in the least indebted to the firm. Mrs. Kinnear having paid up a balance to the amount of 9000*l.* Mr. Kinnear, the day before his death, told witness that he was summoned to attend next day as a juror at Guildhall, and might consequently be absent from the banking-house. Mr. Logan further gave evidence as to the investment of 10,000*l.* for Sir M. Ridley, and proved the letter to Mr. Rothschild to be the hand-writing of Mr. Kinnear.

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[*] The handles of the door were all removed in consequence of painting, so the workman looked through the spindle-hole, not the key-hole. The door could only be fastened by being locked inside.—*Reporter, Lancet.*

[†] A stranger to the family, but residing close by. —*Reporter, Lancet.*

Cross-examined by Mr. Campbell.—There had been no recent fluctuation to any remarkable extent in the foreign securities in which Mr. Kinnear had money embarked. The monies intrusted by correspondents for foreign investment were at the disposal of Mr. Kinnear, but the real proprietors ran all risks of fluctuation. Witness identified a letter addressed by Mr. Kinnear to Mr. Mackenzie of Edinburgh, bearing date the 19th October, the London post-mark of the same day, and the Edinburgh post-mark of the 22nd October. The letter referred to a pecuniary transaction, and contained also allusions to the delicate health of Mr. Logan, which Mr. Kinnear stated "confined him so to business, that he was unable to be with his family at Brighton as much as he desired; but he added that the house in Cornwall Terrace was in a forward state of preparation, and he hoped that in eight or ten days it would be ready to receive his family again," &c.

Thomas Thomson, late servant to the deceased, was next examined, and deposed to meeting his master on the stairs, and to several other facts mentioned in Sir James Scarlett's address; but in addition to the statements of the learned gentleman, he said that when his master came home at nine p. m., he asked if *Jacques* the smith had come, and expressed himself rather annoyed at being disappointed by him. He further asked if the pictures were safely unpacked, and on finding that the glass of one was broken, he said it was a great ity, and proceeded to show Thomson how such an accident might be avoided in future. *On Wednesday morning, at or about nine a. m., witness heard Mr. Kinnear's bed-room bell ring; he went into the passage, and saw the bell vibrating*; he is quite positive, and certain that the bell could not have been rung but in Mr. Kinnear's room, for no other apartment communicated with that bell. On hearing it, he took up Mr. Kinnear's boots and warm water, and left them at the bed-room door, tapping as was his custom. Mr. Kinnear was a very religious and domestic man; he had a wife and eight children, and witness thought he never saw so fond and attached a husband and father. He was making considerable improvements on the premises, and took great personal interest in the progress of the workmen. He was a kind master, but reserved to the servants. He had been slightly indisposed two days before his death, and instead of his usual breakfast, took only a little bread and milk on Monday and Tuesday mornings, and bathed his feet on Monday night. At nine p. m. on the 19th, and for three quarters of an hour which he spent in going over the house that evening, he witness could perceive no alteration in the manner of the deceased. There was a water-closet on the floor below Mr. Kinnear's bed-room, and above the witness's. This closet Mr. Kinnear always used; but while Mr. Kinnear was out on Tuesday the workmen had painted it, and locked it up without Mr. Kinnear's knowledge. It communicated both with the front and back rooms, and with the staircase. There was a gas-lamp directly opposite the parlour-window, which threw a stream of light directly into the room, so that any one going to the water-closet by night would naturally open the shutter a little. The morning of Mr. Kinnear's death, witness was dusting some papers in the library, and found two bits of coloured paper, such as apothecaries cover phials with. It was not the witness's usual custom to call Mr. Kinnear in the morning, but he had done so on Monday and Tuesday, owing to Mr. Kinnear's having slept longer than usual.

James Bates, a workman, confirmed the statements of the preceding witness as to the looking through the spindle-hole of the door, and seeing Mr. Kinnear breathe. He saw the water and boots at the door as Thomson described. He did not heat any bell rung, but it might have been rung without his hearing it.

Two other witnesses confirmed the statements of Bates; and both agreed that the bell might have been rung without their knowledge. It had been rung on Monday and Tuesday mornings, and they had not heard it.

Mr. Lovegrove, surgeon, of Cornwall-terrace, was next examined by Sir James Scarlett. He deposed to his having been called in haste by Thomson to Mr. Kinnear's house, and to having entered the bed-room with Mr. Trotter. The body was in bed reclining on the right side. The right cheek was on the palm of the right hand, and life was completely extinct. The countenance was pale, trunk warm, but extremities cold. No particular appearance in the lips. There had been a very slight discharge of pale saliva, not of any peculiar appearance. The body was dry. It showed no dark spots, or any other particular appearance. Mr. Lovegrove is a surgeon-apothecary, and has been in business five years. There was a decanter on a table in the bed-room; there were also a tumbler and two wine-glasses, one of the latter of which contained about a table-spoonful of sherry wine. The tumbler appeared to have recently contained water. He sent the workmen out of the room. Mr. Trotter and witness remained in the room a quarter of an hour. He searched for a phial, but found none. He was present also at the examination of the body for the Coroner's Inquest. The head was opened. Blood was found in the stomach in a state of decomposition, such as is likely to occur after death, and mixed with food: there were about three pints of blood. He did not examine the blood-vessels of the stomach. The vessels of the brain were rather fuller than natural. He never saw a case of death from rupture of a blood-vessel in the stomach. He is not accustomed to cases of narcotic poisoning. On the day of the opening of the body the scrotum alone presented any appearance of putrefaction. The other parts of the body were natural, and of a pale colour. *He believes that poisoning by laudanum does* NOT *produce a turgid state of the vessels of the brain.* He does not know whether the rupture of a blood-vessel in the stomach would produce vomiting. *Lunar caustic was drawn across the surface of the stomach, to ascertain whether it contained any mineral poison. This test he deems quite sufficient for the detection of mineral poisons.*

Cross-examined by Mr. Campbell.—He is a total stranger to the Kinnear family. The sherry wine in the glass was quite pure. There was no smell of laudanum in the room. The face was composed; there was no smell of laudanum in the stomach. There had been no vomiting in the room. *The stomach certainly had not been acted on by any mineral substance or acid.* Prussic acid has a powerful smell; but there was no such smell in the room or in the stomach. He smelt Mr. Kinnear's mouth on the Wednesday morning when he arrived, and there was no smell in it of Prussic acid or laudanum, but there was a decided smell of sherry wine. Laudanum has a very strong smell, which would have proceeded from the mouth and nostrils. He believes its smell would have been easily distinguished immediately after death. Laudanum very often produces violent convulsions. Not always, but very often. Vomiting *does not always* take place in poisoning by laudanum: sometimes it does; sometimes not. There was no laudanum in the stomach. In the stomach it would retain its smell a considerable time. Rupture of a blood-vessel in the stomach was the cause of Mr. Kinnear's death; and the witness does not believe that his death was caused by poison of any kind. A rupture of a blood-vessel in the stomach may happen to a man in health. So much blood could not be found, had not a blood-vessel been ruptured. Bursting of a blood-vessel would account for all the symptoms. The other medical gentlemen present at the examination, Drs. Sigmond, Pattison, and others, agreed altogether in these opinions.

Re-examined by Sir James Scarlett.—The blood was putrid. He did not measure its quantity, but guessed it. Medical men, from their habits, can readily form a pretty certain estimation of quantities by sight. Hemorrhage of the stomach would naturally tend to prevent turgescence of the blood-vessels of the brain. The chest was examined, and there were six ounces of fluid on one side and four on the other. If a man takes laudanum as a poison, it would prove fatal in ten or twelve hours. He states this from what he has heard and read. The fluid in the chest was bloody serum. He considered that this was poured out either at the moment of death or shortly after.

In answer to a question from Mr. CAMPBELL, witness stated, that laudanum would retain its smell longer than sherry wine.

Mr. CHARLES ASTON KEY, sworn.—Is one of the surgeons of Guy's Hospital. Has heard the statement of the witnesses. Has seen many cases of hemorrhage into the stomach. In the cases he had seen, vomiting generally takes place. Rupture of the blood-vessels of the stomach would not produce death very quickly, unless great quantities of blood were lost, and in such cases he believes death would be preceded by vomiting. *Gastric hemorrhage is perfectly inconsistent with vascularity of the brain.* As far as his judgment goes, according to the evidence, death certainly was not produced by rupture of a blood-vessel in the stomach; some other cause must have interfered to produce the turgescence of the vessels of the brain and the effusion of bloody serum into the chest. This certainly did not occur at the moment of or after death. Has seen a great many cases of poisoning by laudanum; most of them recovered: two or three were lost. Laudanum rarely produces vomiting; it has a tendency, on the other hand, to check it. It produces death in from eight to sixteen hours. The absence of a smell of laudanum would not be remarkable in poisoning by that drug. A state of lethargy, and sometimes convulsions, precede death by laudanum; sometimes it produces a livid appearance of the body. He is inclined to suspect that Mr. Kinnear's death was occasioned by a narcotic poison.

Cross-examined.—A man with gastric hemorrhage would be able to ring a bell. It is true that a medical man who saw and examined the body might form a better judgment of the cause of death; but if the description given by the witness was correct, a very correct judgment might be formed. Witness gives his opinion on the testimony of Mr. Lovegrove *and on a certificate he has read.* Had witness examined Mr. Kinnear's body, he would have made a more minute inspection, and then his opinion might have been different. Though the heart was not examined, no cause of death could have existed in it which would not have given rise to urgent symptoms during life. He could not conceive it possible that a man could have heart disease without complaining of it. In *most* cases of poisoning by laudanum smell remains. At any rate it would remain as long as the smell of sherry wine. Laudanum was not likely to cause an effusion of blood in the stomach, but he still has seen two or three ounces effused from it. There are cases on record of laudanum being taken into the stomach, and not detected after death. *Had Mr. Kinnear died of narcotic poisoning, he could not have walked across a room to ring a bell in eight or ten hours, or even in one hour, after the poison was taken.* He has never seen blood effused into the stomach from disease of the heart.

Mr. Joseph Henry Green sworn. Is one of the surgeons of St. Thomas's Hospital, and Professor of Surgery in the King's College, London. Agrees with the evidence given by Mr. Key. It is most consistent with his belief that Mr. Kinnear died of a narcotic poison, but the grounds for such belief are not sufficient to warrant a *positive* conclusion; no strong opinion could be given. He thinks the quantity of three pints of blood would produce vomiting. He believes effusion into the chest to be an effect of poisoning by laudanum. *Apoplexy would probably leave greater appearances on the brain.* The deceased would not have been suddenly disabled by gastric hemorrhage.

Cross-examined.—He is not skilled in tests for poisons. *There is often great obscurity as to the causes of sudden death.* The manner of Mr. Kinnear's death contradicted the idea of disease of the heart. Had the witness been engaged on the examination of the body, he would have thought it necessary to make a more minute inspection than was done.

Mr. Alfred Taylor, sworn. Is a lecturer on medical jurisprudence at St. Thomas's. Has been in the habit of applying tests for the discovery of poisons. Death certainly was not produced by the rupture of a blood-vessel in the stomach. Such an occurrence is wholly inconsistent with the appearances described, and with turgescence of the vessels of the brain. *From the circumstances of Mr. Kinnear's death he would infer that some narcotic agent had been the cause of Mr. Kinnear's dissolution. He heard of no symptom or circumstance inconsistent with such an opinion.* He has examined three cases in which death proceeded from gastric hemorrhage. In none was there a rupture of a vessel. The hemorrhage generally depends on effusion through the extremities of the vessel, not from a rupture of its sides.

Cross-examined.—The presence of three pints of blood in the stomach would not lead him to suppose poisoning. He had not known three pints of blood to be in the stomach of persons poisoned by narcotics.

The last witness was Mr. Wettenhall, a stock-broker. He produced the price list of the foreign funds from January to October 1830, inclusive. The greatest fluctuation in October, in the French 5 per cents, was 95 to 93½.

This closed the case for the Insurance Company, and Mr. Campbell then commenced a powerful address to the jury, in which he dwelt forcibly on the absence of all moral reasons for the commission of suicide, and on the strong moral evidence afforded, on the other hand, by the tone of his letter to Mr. Mackenzie, by the anxiety manifested respecting his workmen and his pictures, and by the composure of his manners on the evening preceding his dissolution. The learned gentleman reviewed the evidence relative to Mr. Kinnear's pecuniary transactions, and observed, that the trifling fluctuation of 1½ per cent. could not have embarrassed his affairs in the least degree. He then dwelt emphatically on the utter impossibility of a person labouring under the influence of poison being able to get up and ring a bell two hours before his death, and was proceeding to animadvert on the nature of the medical testimony brought forward by the Rock Office, when he was stopped by

The Jury, through their foreman, who informed Mr. Campbell that it was unnecessary for him to proceed further in the case, and, without further conference, returned a *verdict for the plaintiff.*

THE LANCET.

London, Saturday, July 14, 1832.

The "one cause of misfortune" to Dr. Ryan, to which we adverted at the close of the article in the last Lancet on the subject of the trials, is somewhat peculiar, and certainly this one cause of misfortune deserved not punishment at the instance of Messrs. Ramadge and Long. The Chief Justice, Jury, and Counsel for the Plaintiff, dwelt with much earnestness upon the fact that the alleged libel had not been copied precisely as it was first published.* It was contended that in The Lancet, for example, the statement appeared merely as the letter of a correspondent, and was entitled "Result of upholding Quacks," whilst in Dr. Ryan's Journal it was inserted as an article of news running thus: "Dr. Ramadge was in attendance on a case of typhus; the patient, a young lady, was bled from the arm on a Friday, and *eight dozen* (96) *leeches* applied to the head and neck, &c. &c." The altered article, moreover, was made to bear this title—"*Tweedie versus Ramadge.*" Having here placed before the reader the first sentence of the statement published by the defendant Ryan, let us now throw into juxta-position with it the first sentence of the letter printed in The Lancet:—"Sir,—The following account of a medical fracas shows that your strictures against quacks and their coadjutors are duly appreciated by respectable men. Ten days ago, &c. &c." Observing this strange abandonment of the duty of a mere copyist of news, called, as their attention was, to the alterations made in the statement by the hand of Dr. Ryan himself, the jury—and the judge went along with them in doing so—inferred that the changes had been instigated by malice, by a desire to inflict the greatest possible injury upon the plaintiff, and, in fact, that the letter in The Lancet had been twisted with a view to serve that unhallowed purpose. Dr. Ryan was here utterly defenceless; his counsel had nothing to urge in mitigation or extenuation, and the jury, deciding upon the facts of the case as they were before them, mulct the Doctor in heavy damages. Now, we think, in strict fairness, Dr. Ryan ought to have been excused from any such imputation of malice as was here thrown out against him, and we marvel that the Doctor omitted, or neglected, to point out to his advocate, the only motive which induced him to reconstruct the letter before he gave it a place in his journal. Nothing was more easy of explanation, nothing more easy of justification.

Mr. Serjeant Taddy should have been instructed to speak to this effect:—"Gentlemen, much stress has been laid by my learned friend upon the fact, that the alleged libel has been published by my client in a form somewhat different from that which it assumed in The Lancet. We readily acknowledge the charge; but we deny that we were urged to it by malice against the plaintiff. Yes; we admit that the first sentence was omitted. But why was it omitted? *Not* because my client felt any ill-will towards the plaintiff, but because the passage contained an acknowledgment of the power of The Lancet,—an acknowledgment which my client, as a medical journalist, could not with any degree of prudence have recorded in his own pages. His footing was too insecure to warrant him in making such an admission; hence the alterations in the letter of The Lancet correspondent." In the absence of some such declaration, what could the jury infer, but that Dr. Ryan had been instigated to make the omissions and alterations of type, by some strong and incontrollable feeling of malice entertained against the plaintiff? Appearances, therefore, justified damages to a certain extent. In the absence of proof of the truth of the statements—for where truth is established, malice is not admitted into the account—damages were demanded, although we think not to the extent of *four hundred pounds*. Thus, in supplying these data, we furnish Dr. RYAN with at least *one* ground of argument to be urged in his claim for a new trial—namely, that the award of two, at least, out of the four hundred pounds damages awarded against him—he is called upon to pay Dr. RAMADGE, not as compensation for an injury inflicted upon *his* reputation, but for having unwittingly—though not corruptly—suppressed the passage which contained an acknowledgment of the power of THE LANCET. The dilemma into which the doctor has here fallen is not entirely devoid of dramatic effect; but as it partakes more prominently of the serious than of the comic character, we will not add to the awkwardness of the scene by indulging in pleasantries, which, in the excited feelings of the moment, might be construed into both ill-timed and ill-natured criticisms.

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* Erratum in last leading article; 5th line from commencement, for "*correct*" read "*incorrect.*"

Besides taking into view the striking alterations made in the alleged libel, it must be borne in mind, that the defendant RYAN went into court with his "justification" after he had offered to apologize. Now we fearlessly contend, that no jury could give a willing ear to evidence which went to prove the truth of statements, the falsehood of which had been tacitly admitted by a proposal to acknowledge their inaccuracy. It was not, be it remembered, a mere case of copying an article from another journal; here the pretended copyist had made the matter his own; no longer did the document appear as the letter of a correspondent, but it was made to assume the form of an editorial article from THE LANCET, than which nothing could be more incorrect. The jury, then,—viewing these mysterious metamorphoses, perceiving that the note of a correspondent in THE LANCET had been inserted by the defendant as a portion of an editorial article from that journal,—were forced to believe that Dr. RYAN had stepped beside the path of an upright, candid journalist, and had pursued the plaintiff with a feeling of vengeance rather than followed the line of public duty with a praiseworthy love of justice. Our contemporaries, who have noticed these trials, seem to take it for granted that the alleged libels were the same,—that the pleas were the same,—that the arguments were the same—that the summings-up of the two judges were the same; and that the only discrepancy is to be found in the decisions of the two juries. They hazard no opinion as to which verdict is the improper one, though they conclude that one *must* be improper, because there is such a marked variation in the amount of damages awarded in the two instances. In truth, instead of their having been so many points of resemblance in the two causes, there were scarcely any two that agreed. Certainly the alleged libel in the second case was founded upon the publication for which the first action was instituted. But what were the pleas? In the case of THE LANCET—the general issue; Not guilty. In the case of RYAN, RENSHAW, and RUSH—a *justification* and *general issue.*

Thus the evidence which Mr. WAKLEY contended the *plaintiff* was called upon to put in, was required of the *defendants* in the second cause by *their* plea of justification. But in order to make this point still more clear, let us remind the profession that in the first cause the defendant having pleaded the general issue, the onus of *proof* of the averments in the declaration fell upon the plaintiff. Now how did he (the plaintiff) prove to the jury that "he had always carried on his profession as a physician with skill, care, and integrity,"—that he "had always conducted himself in an upright, honourable, and proper manner," and had "deservedly obtained the good opinion and esteem of the public, and enjoyed great credit and reputation in his profession?" What, we repeat, was his proof? Why, by his own witness stating, in his cross-examination, that "he had told the plaintiff that no man despised his conduct in advocating LONG more than he did," and further, by the counsel for the plaintiff having, with the utmost pertinacity, objected to allow the witness to state what had been his own declared opinion of the plaintiff's professional conduct. Thus circumstanced, allowing that the jury disapproved of the publication of the letter—allowing that they considered it of a libellous tendency—what award could they make other than one farthing damage? Had not the plaintiff failed to prove that he had a reputation to injure? Had not his counsel strenuously objected to the putting of every question which related to his professional conduct? But does it follow that because this jury were scrupulously, religiously just in *their* decision, that the gentlemen composing the jury on the succeeding day were wholly wrong in the verdict of four hundred pounds? Mark the difference in the evidence; reflect upon the variations in the pleas. *Now* a defendant had come into court, upon whose head devolved the responsibility of proving that "the plaintiff's character, as regarded the transaction with LONG, rendered it imperative for all medical men to decline acting with him," and further that "Dr. TWEEDIE cured the patient by *exactly opposite treatment*" to that employed by the plaintiff. Here was a complete changing of sides, and we *must* infer, from the decision of the jury, that the *defendant* in this instance failed to do that which the *plaintiff* had done in the other —, the justification not only being rendered incomplete by the evidence, but the attempt to justify being in itself repugnant to the feelings of justice, after the avowed and unaccepted offer to apologise.

Again, instead of satisfying the jury of the unprofessional attempt to uphold Long, by entering into a medical analysis of the letter which the plaintiff wrote in favour of that quack, some physicians and surgeons were called to prove that they had said they would not meet the plaintiff in consultation. This was good, as far as it went, but the jury should have been satisfied that the grounds upon which they refused to meet the plaintiff were just ones. This should have been made indubitable to the minds of the jury, but how could it be effected without showing what had been the conduct of Long in the terrible cases of Miss CASHIN and Mrs. LLOYD? And following this up it should have been shown what had been the conduct of RAMADGE himself in attempting to bolster up the medical character of such a being. The concatenation should have been made perfect; there should have been no cracked link in the chain; and then the verdict of the jury might have sounded very differently in the ears of the parties most interested, and somewhat differently to our discerning contemporaries of the press. Producing medical gentlemen to prove that they had *said* they would not meet the plaintiff in consultation must have been pernicious in effect, in the absence of a full and searching examination of the grounds on which that decision had been formed. In neglecting to adduce every admissible particular in relation to Long's conduct, the defendant paved the way for the assertions of the plaintiff's counsel respecting "conspiracies" of the medical profession against the plaintiff and Long, and enabled them to make considerable inroad upon the feelings and the judgment of the jury. The evidence of the expulsion from the Medical Society, from the like cause, must have had a prejudicial rather than a beneficial effect, though no *dog* could be disgraced by expulsion from *that* society, the members of which once expelled their best man,—JAMES LAMBERT, because he was bold and honest enough to publish the truth. *We* did not allude, in *our* defence, to Dr. Ramadge's expulsion from amongst a body of men who could so conduct themselves towards a deserving, talented, honourable—most honourable—member of their profession. Had our case required aid of this description we should have been ashamed to have exhibited ourselves in court.

In conclusion, then, it is not quite evident that—although the award of 400*l.* damages may be considered as excessive, from facts which the profession and the public can now fully understand and appreciate—there were circumstances in the case which called for a very opposite decision from the respectable and upright jurymen who were chosen to preside on the occasion.

Here, then, we leave the subject for the present, but should any of our correspondents still feel any difficulty in understanding either the legal or medical points of the case, we shall with pleasure resume the subject for their satisfaction.

We have not room this week to make any remarks on the extraordinary evidence elicited from certain hospital surgeons and lecturers on the medico-legal trial which is reported at page 468 of our present number; we therefore content ourselves just now with merely pointing the attention of our readers to it.

LIBEL—THE LATE TRIALS.

To the Editor of The Lancet.

Sir,—As you thought my letter worthy of notice, I have made bold to write you a second; and I do this in consequence of the blundering of every part of the public press (save that conducted by yourself), which has taken up the subject of the difference of the award of the two juries in the late actions of libel brought by Dr. Ramadge. On perusing your journal for this week, I have been much relieved, and I sincerely congratulate you on the sound and wholesome views there taken in this behalf. Now, indeed, do I see reason to hope; that the public will no longer remain in the darkness hitherto occasioned by the press, one great use of which, I take it, is, to rightly and clearly inform the people on the public topics of the day; but worse than useless are its efforts when they run counter to this object.

I am induced to trouble you further, by going shortly into the reasons of the difference of the verdicts, and the conclusions come to by the portion of the press I have mentioned. The libels were different in words, to say the least—not to mention whether in substantial effect or not. The answers of the defendants to the complaint against them, were widely different. One pleaded the general issue—the other the general issue and a justification. The general issue alone succeeded;—that coupled with the justification failed; and any one who was present in court could not but observe, that the judge, in summing up, said to the jury something to this purport:—that the truth of the facts, taking them literally, word by word, contained in such portion of the libel as was attempted to be justified, ought to have been proved. It is evident to any one that these facts were not so proved—and then came a verdict for 400*l.* I verily believe the facts were substantially proved, but whether the law requires strict verbal and literal proof in such a case, seems to be doubtful, and the lawyers must decide it.

Now then for the gentlemen of the press. Say they—"Libels are the same," but nothing about the two very different answers of the two separate defendants, and what they separately chose to inform their juries. "Verdicts ought to have been the same," they say. Far be it from me to say they ought not; but let facts be stated truly, and then let our institutions be abused to the extent only that facts will warrant.*

An Eye-Witness.

10th June, 1832.

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* It may be worth while to inform our intelligent correspondent, that the additional remarks made in this week's number, on the subject of the trials, were written before this communication was received.—Ed. L.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

MADE BY

MR. BRODIE.

May 31, 1832.

INFLAMMATION.

If a person receives a wound, after a little time the wounded part becomes red, sore, and painful. The same symptoms as these may, however, also arise from other causes, as, for instance, from fever, or from the sudden application of heat, or of hot water to a part previously chilled by cold. Now such a condition as this occurs in all accidental injuries, and in every disease, whether medical or surgical. The doctrine of inflammation may be considered as the elementary part of surgery; but in order to understand it properly, you must be well acquainted with, first, the capillary vessels; secondly, the circulation generally, or that in the capillary vessels more particularly. The aorta arises from the left ventricle, passes down, and sends off numerous branches, which are subdivided into numerous smaller vessels which communicate with the veins, and the blood is thus returned again into the right auricle. But how does the blood in its course pass from the arteries into the veins? Some there are who say that the blood is poured into cells; this fact cannot be demonstrated in the human body, but it may be observed in cold-blooded animals, where the arteries may be seen communicating with the veins by means of the capillary vessels. This may be very plainly noticed by placing the web of the frog's foot under the field of the microscope. These capillary vessels not only convey the blood from the arteries into the veins, but are all very important agents of the human body. The various secretions are formed from them, as the urine, the bile, &c. The blood flowing in the artery is of a scarlet colour, and as long as this scarlet blood flows into the veins, the regular action is kept up; but if the blood becomes dark and loses its scarlet colour, the vital functions are not properly maintained and supported; the muscles lose their power of contraction, and the sensibility of the body becomes enervated, the black blood acting as a poison to the whole system. These chemical changes in the blood take place in the capillary vessels. The capillaries are not always of the same diameter, but are often dilated, as, for instance, when the neck and cheeks are suffused with blood, denominated blushing. In inflammation and in venous congestion, they likewise become dilated. When a part is inflamed it becomes red, but this is not a positive proof of dilatation, but only one of the phenomena of inflammation. The part becomes painful, red, and tender, which latter feeling may be caused by the increased afflux of blood to the part. This particular form of sensibility is termed pain, which is greater or less, according to the degree of inflammation, or whether the inflammation be situated over a loose part of the body, where in general there will be less pain, or vice versa, as, for instance, in inflammation of the finger, where the parts underneath are not yielding; there the part inflamed is hotter, on account of the greater rush of blood to the part. The internal part is not hotter than natural; it is only on the outer surface of the skin that we can perceive any difference. In this way, therefore, we have explained to us the phenomena of inflammation,—the pain, the heat, and the swelling; but these symptoms do not always occur together. Various opinions have arisen with regard to the theory of inflammation, or of the circulation in an inflamed part; but the more probable one is, that the capillary vessels are in a state of debility, rather than of increased action. We are at present, however, quite ignorant of the proximate cause of inflammation.

The *causes* of inflammation are either such as produce chemical or mechanical injury to the part,—the application of fire or caustic to the part, or nervous influence, or many other causes which we cannot distinguish. The local inflammation in a part may subside without producing any changes; the hardness may go away, and the pain and swelling subside; but sometimes other phenomena will occur before the affected part is restored to its usual state of health. The serum of the blood is poured out in the tunica vaginalis; in such a case where does the fluid come from but from the capillary vessels? In hydrocele, if it be not punctured, the fluid remains pent up for a long time, until it at last disappears; it is absorbed, and taken back again into the general circulation, and this is one way in which inflammation terminates; or the inflamed surfaces may be glued together by the effusion of coagulable lymph between them. Blood-vessels may form in the substance of this coagulable lymph, and it may thus become organised, which will take place in twenty-four hours, and sometimes in a much shorter period. Sometimes the coagulable lymph becomes opaque and altered in appearance, according to the part from which the lymph is thrown out. In a fractured bone it becomes a gristly substance, and is afterwards converted into bone. This immediate union of surfaces answers a good purpose to the animal system; but there are also some great disadvantages occurring from it, as in inflammation of the pericardium, where the surfaces become united together, and the heart is not allowed sufficient room to act and play, and dropsy follows.

It is only in the lesser degrees of inflammation that serum is effused, for when the inflammation is great, coagulable lymph is thrown out, which resembles serum, and may be very nearly coagulated by heat. When the inflammation runs high, the part becomes swollen and elastic, and there is then a fluctuation felt, which can only be distinguished by placing two fingers upon the tumour; at last it ulcerates, and the pus is thus discharged, and it then receives the name of an abscess; but when pus is found upon a raw surface, it receives the name of an ulcer. Sometimes the pus is dispersed in numerous cells of the cellular membrane, these in time break down, and the pus forms one large abscess. The inflammation may thus, therefore, terminate by suppuration.

Pus is yellow and opaque. Sometimes the white globules in it are very thick, and sometimes, from the large proportion of serum in it, its consistence is very thin. You see that pus is formed by secretion like the bile.

Abscesses are formed by suppuration, and by the removal of solids by ulceration. At that part where the inflammation is greatest, suppuration and ulceration are present; next to that, and surrounding the part, lymph and serum are present; further on, serum is effused into the cellular texture, and the limb is œdematous. Sometimes the inflammation ends in suppuration, and pus goes on forming from one cell in the cellular membrane to another. When an abscess forms, the ulceration first begins in the substance of the body, but does not equally extend in every direction; it extends most towards the surface of the body, as there it meets with the least resistance. Sometimes the ulceration begins on the surface of the skin or the mucous membrane; but then it is an ulcer and not an abscess. An abscess generally continues to discharge its contents for a longer or shorter period. Sometimes it shows no disposition to heal, which may be occasioned by the presence of a foreign body, such as a ball or a piece of cloth; or from there not being a free egress for the matter, or from the parts in which the abscess is situated being in motion; or from a weakened state of the constitution: all these may tend to prevent the healing of an abscess, but generally speaking it will heal very readily. It may be also the case with ulcers, that they will not heal. Ulcers and abscesses are filled up by new granulations of flesh on the surface, formed by coagulable lymph becoming vascular. When granulations are healthy, they are small and pointed and vascular; they grow slowly, and when once formed, are permanent; at other times, when they are unhealthy, they are large and loose, pale, grow quick, and have but little vascularity. Granulations are covered by a thin delicate membrane, secreting pus. When an ulcer is filled up, it cicatrizes, the membrane becomes thicker and more opaque, and thus, by degrees, new skin is formed; the new cuticle is secreted from that surface, protecting the new cutis. On the surface of the new cutis there are no hairs or nervous papillæ. At first the cicatrix is more vascular than the surrounding parts. Ulceration and suppuration are not always conjoined together. Inflammation has various characters in different diseases; that which I have described is termed phlegmonous inflammation; it is different in small-pox, measles, &c. In some cases it proceeds more rapidly than in others. When acute it is quick in its progress; when chronic or indolent, on the other hand, it is slow; the latter (chronic) differs from the former, as it may terminate in effusion of lymph, serum, or pus, or in the effusion of a watery fluid containing little coagulable matter. When inflammation is very great, and the symptoms are very high, the whole constitution becomes affected, and various diseases, such as fevers, etc. are produced.

Treatment. — The exciting cause producing the inflammation should be removed, and this axiom should be remembered in the treatment of every disease. The animal system generally has the power to right itself. Bloodletting is employed in order to remove the inflammation. When limited, and the constitution is not much disturbed, local bloodletting near the surface of the part by leeches or cupping will generally suffice. If the inflammation be, however, more violent, bloodletting from the arm must be employed, but this will vary; less blood should be taken from old persons than from young. When the inflammation is great, more blood should be taken than in cases where the symptoms do not run so high. Sometimes, when the formation of an abscess is to be feared, as in inflammation of the chest or brain, you should bleed more largely. If the inflammation occur in the leg or arm, it is not of so much importance. In some cases you will have to bleed more than in others. When the blood flows fully and free, its substance being of an azure colour, and, upon cooling, the solid part becoming firm and contracted, pressing out a great deal of serum, the bottom of the coagulum being of a dingy-red colour and weighing most, it is then called *buffy*. When the surface of the coagulum is hollowed out, the blood is said to be *cupped*—this is observed when the inflammation has been violent. If the patient has been bled often, the coagulum is covered with a thin buff, and is of a loose texture, and not hollowed out. The appearance of the coagulum will differ in different portions of the blood taken at the same time. This variation in the appearance of the blood differs according to the heart's action, and not from any chemical changes. Inflammation may be treated by purgatives, where the intestines are loaded. There is a mucous and watery fluid in the intestines, from the blood, and purging carries this off. Diaphoretic and diuretic medicines are also useful in inflammation. Tartarised antimony as a diaphoretic is useful. Evaporating lotions also are useful—they cause the small cuticular vessels to contract. Warm fomentations and poultices are sometimes useful, as inflammation is frequently combined with tension, which keeps it up, and warm applications and poultices relieve this.

In chronic inflammation, it is sometimes of service to keep up counter irritation by exciting inflammation in the neighbourhood of the part; this may be done by rubbing the parts with some stimulating liniment; these should always be of such strength as to excite pain. Blisters will likewise induce counter-irritation, by raising a slight vesicle on the surface, taking off the skin, and dressing the raw surface with savine cerate. You may also rub up ʒij of *antim. tart.*, with ℥j of *lard*, and rub it on the surface of the part affected, for a few days, night and morning, until pimples are produced; these will, after a time, gradually decrease and die away, and they may be again produced if necessary. Sometime the external use of antimony produces constitutional irritation, fever, etc., followed by a second eruption on the skin, which however soon dies away. Issues, setons, caustic potash, and the actual cautery, will all produce counter-irritation.

LIGATURE OF THE THYROID ARTERY, FOR ENLARGEMENT OF THE THYROID GLAND.

We alluded in a former number to a case of enlargement of the thyroid gland in the hospital under the care of Mr. Brodie. The man as we stated had been repeatedly bled and leeched, and every means of art had been used to stop the progress of enlargement in the tumour but without effect, and Mr. Brodie therefore resolved to tie the thyroid artery of that side, and see what chance that would give to the patient. See Lancet, page 27.

June 21st. On this day the operation was performed. The man was seated in a chair with his head inclined to the left side (the tumour being situated on the right side). Mr. Brodie began by pinching up the skin over the upper and inner surface of the tumour which he then divided, thus exposing the muscles, which, with the cellular fascia surrounding them, were either divided or held aside by a blunt hook until the artery was skilfully and beautifully exposed. A double ligature was passed round it and tied, a piece of lint was placed between the edges of the wound, which were brought together by straps of adhesive plaster, and the man was sent to bed. The exposure of the thyroid artery was done with great skill by Mr. Brodie. The man bore the operation remarkably well; there was but little bleeding, four or five small arteries being secured as soon as they were divided; about twenty-seven minutes elapsed from the first incision until the artery was tied. After the patient had left the operating theatre, Mr. Brodie thus addressed the pupils:—

"This man has had enlargement of the thyroid gland for some time past. I have tried the usual remedies for these diseases since he has been in the hospital, he has had leeches applied to the tumour, and before he came in he had used the different preparations of iodine, burnt sponge, &c., but all to no purpose. I have consulted with those who have been in the countries where goitre-neck is more common than it is with us, and who have therefore had greater opportunities of seeing the disease than I have had, and their opinion fully coincides with mine, that iodine is of little or no use in these cases except it be in the very early stages of the disease, and that when the disease is more advanced iodine seldom or never diminishes them in size, or even stops their future enlargement. Well, then, as no other remedy was left for this poor man, I determined upon tying the thyroid artery of the side where the tumour was situated. Sir William Blizard was the first who proposed this operation in cases of this kind. A very skilful German surgeon of the name of Walter tied the thyroid artery in a case of goitre-neck, and the tumour greatly diminished, and he tied the artery of the opposite side, and the tumour diminished still further. Mr. Coates of Salisbury also tied the thyroid artery with great advantage, and I am informed that Mr. Earle has also performed the operation, and great benefit has resulted to the patient from it; I have performed this operation once before upon a patient in this hospital; in that case the tumour was not so large as this man's, and the thyroid artery was situated very superficially, being apparently pushed upward and forward by the projection of the tumour. I tied it, but I do not remember that the tumour was very much diminished in size from it. I nevertheless, however, thought it best to try what the experiment of an operation would do in this case. You see that the operation is not a trifling one—in this case the artery lay deeper and further from the surface than in the one to which I have just attended; should the man bear this operation pretty well, the iodine and burnt sponge may be tried again, and perhaps with greater benefit; and if the tumour should diminish in size I may perhaps tie the artery of the other side, but without there be great benefit arising to the patient from this operation, it will be hardly worth while for him to undergo another operation, and only for the sake of experiment."

SUCCESSFUL EMPLOYMENT OF

MERCURIAL OINTMENT IN ERYSIPELAS.

To the Editor of THE LANCET.

SIR,—The pages of your useful publication first suggested to me the plan of treating erysipelas with mercurial applications, but as I have not heard of the hint having been pursued, I will, with your permission, add a *morceau* of evidence in its favour. Perhaps the following cases may be worth inserting.

CASE I.

Samuel Green, ætat. 48, applied to me, with a severe attack of phlegmonous erysipelas of the anterior part of the right leg, extending from the lower end of the tibia and fibula nearly to the knee joint; limb very much swelled, and pain excessively pungent; constitutional symptoms not very severe. The usual antiphlogistic remedies were resorted to, and warm fomentations were directed, as a local application. The two following days I saw him regularly, and found the disease not only on the increase, but the centre of the part primarily affected much darker than the surrounding surface, and, from the severe pain, which the patient described as being of a throbbing nature, and the great tension, I expected speedy and unhealthy suppuration. Pulse 100, and rather full; tongue white and loaded; bowels freely open; rigors, alternating with much febrile excitement, frequently observed. I now wished a copious venesection, but this being objected to by the patient, I determined to try the application of the *ung. hyd. fort.*, spread upon linen, and repeated three times a day, at the same time continuing the antiphlogistic treatment. From this time, the cure proceeded rapidly; the febrile and other unfavourable symptoms were removed, and in a day or two only a stiffness of the limb remained.

Perhaps your readers may attribute the cure to the antiphlogistic remedies, as the *tart. emet.*, &c., which were employed; and indeed these must not be lightly passed over. But I will adduce another case, in which the only *modus medendi* was the strong mercurial ointment. It may appear strange that I should have trusted to that alone, and I confess it was only an experiment, which I was induced to try from the marked good effects which supervened its use in the aforementioned instance.

CASE II.

John Goulding, ætat. 30, has acute erysipelatous inflammation of the convexity of the right foot. The usual febrile and other symptoms attended it. Has himself used cold applications without advantage. I ordered him to apply the *ung. hyd. fort.*, as in the case of Green, and prescribed no internal remedies. Here also I found the most complete success. Even on the next day but little of the inflammation remained, and in a day or two he was enabled to follow his usual occupation.

We must in this case give the ointment its full meed of praise, and if these remarks be the means of causing a more extensive trial of its effects, which I anticipate will be found most advantageous, it will afford much gratification to your constant reader and well-wisher, A.

Derby, June 10, 1832.

TO CORRESPONDENTS.

Many thanks to *An Old Practitioner* for his very friendly admonition. In a very short period he shall be in possession of a more useful, and hence a more agreeable, reward for it than this acknowledgment.

We should willingly have complied with the request of *Dr. Latta*, but the sheet containing his letter had been long at press when his note for its withdrawal was received.

Erratum.—The words "venous injection" affixed to a note from Dr. Green, in our last number, on the detection of air in the heart of a cholera patient, were not meant to apply to Dr. Green's case, in which injection into the veins had not been employed.

THE LANCET.

Vol. II.] LONDON, SATURDAY, JULY 21. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXVII.

THE CEREBELLUM — PONS VAROLII — MEDULLA OBLONGATA, AND INTRODUCTORY OBSERVATIONS ON THE NERVES.

I have stated in a former lecture that there is only an apparent difference in the relative situation of the cerebrum and cerebellum in the biped and the quadruped; that in both the cerebellum is the portion nearest the foramen magnum, and under which the crura cerebri must pass in order to arrive at the medulla oblongata and spinal chord. I have also spoken of the relative size of the cerebrum and cerebellum in different animals; in the biped, the cerebellum being comparatively small, one-ninth part only of the bulk of the cerebrum —in the ox the same proportion being observed — the cerebellum increasing in size in the dog, the horse, and the sheep, until in the last animal it becomes a full fifth part of the weight of the brain. We will now see what light a consideration of the structure and functions of the cerebellum will throw on the matter.

Description of the Cerebellum.—The cerebellum is apparently and partially divided into three lobes; two of which alone are visible in the human being, while the third or central one is covered by the lateral ones; but as the whole cerebellum increases in bulk in the quadruped, the central lobe does so in a peculiar manner, and is apparently broad and prominent. Its anterior and posterior extremities (*the vermiform processes*) are bulkier and more elongated. The vermiform processes of the central lobe are quite separated from the lateral lobes; and both of them curl up like the extremities of a great worm under the base of the cerebellum; the separation extends a little way beyond the processes, and more particularly from the posterior process. Before, however, we arrive at the centre of the cerebellum from the posterior process, and ere we have scarcely left the anterior one, the division into lobes is lost, and we have one connected body with deep sulci running in various directions, but mostly transversely. The convolutions, less marked in the brain of the quadruped than in that of the human subject, are far more prominent, and the sulci deeper in the cerebellum of our patients than in that of man. Instead of the fissures not being more than a line in depth, some of the lobuli may be separated almost to the centre of the cerebellum. The division between the cineritious and medullary portions is more distinctly marked than in the cerebrum; the cineritious is here also the cortical or outer layer, lining either the external or internal surface of the lobuli; while the medullary substance occupies the central part: here also more evidently than in the cerebrum the cortical is in detached portions, and the medullary connected together, and tending towards the centre through the whole of the cerebellum; so that when we make a vertical incision through either of the lobes, we have a beautiful resemblance of a tree (the *arbor vitæ*), the trunk and the branches composed of medulla—the foliage consisting of cineritious matter.

The Crura Cerebelli.—The trunk from each side of the cerebellum takes a direction anteriorly and downwards, and reaches or rather forms the base of the *tuber annulare* or *pons varolii.* These trunks are the *crura cerebelli*, conveying or combining the influence or power of the cerebellum. These crura prolonged, and widening as they come more into sight, and meeting on the centre of the base of the brain, constitute the tuber or pons. The crura cerebelli are meeting each other transversely, and forming a kind of arch or bridge, and the crura cerebri are passing under the arch. What admixture of substance or influence takes place, I will presently inquire; but, first of all, I must speak of the function of this little brain.

Function of the Cerebrum and Cerebellum.—I have endeavoured to trace the connexion between the medullary and cineritious portions of the brain, and the prevalence of the animal or intellectual principle. My conclusions, advanced with some degree of hesitation, were founded on the seeming connexion between the quantity of cineritious matter in the common sensorium, and the degree of intelligence which the animal possessed. In proportion as the medullary substance prevailed, the quadruped stood low on the scale of intelligence; and so far as we could ascertain it, proceeding upwards from the ox and the horse to the dog, in proportion as the cineritious matter increased, the reasoning faculty was developed. Other arguments drawn from experiments on living animals I purposely reserved, until we were enabled to compare these two divisions of the brain together.

The Experiments of M. Fleurens on the Brain.—Effects of removal of Parts of the Brain.—M. Fleurens has instituted a connected course of experiments on the functions of the brain, the result of which is to a certain extent satisfactory. He laid bare a portion of the brain;—he began by pricking the hemispheres—there was no muscular contraction—no symptom of pain. He cut off a portion of one of the hemispheres in an horizontal direction; still no contraction—no pain. The animal seemed to be unconscious of the affair. He cut yet deeper and deeper; the animal evinced no sign of suffering. At length he reached the medullary part—he removed the corpora striata, and the optic thalami, and yet there was no pain. This, Gentlemen, will not, perhaps, appear incredible, if you recollect that although, according to our opinion, the hemispheres of the brain are the organs to which impressions on the sensitive nerves are conveyed, and where they are received, and we are rendered conscious of certain sensations, it is only *through the medium of the nerves* that this is effected, and there are no nerves in the portions which we have pricked or removed. Cut off in any part nervous communication and you destroy sensation; and so here there is no feeling of pain, for there is no nerve to give notice of injury. He then ventured deeper—he pricked the tubercula quadrigemina, and crying, and trembling,and convulsions, ensued; and these increased as he penetrated deeper still into the medulla oblongata;—the senses of sight and hearing were destroyed, and stupor stole over the animal, and he was incapable of voluntary motion.

Effects of Pressure.—Pressure was made on the hemispheres laterally. No effect was produced, the animal was altogether unconscious of it. Pressure was then made vertically, and insensibility speedily ensued—not, however, total insensibility, for there remained a faint dull sensation, and the animal, when violently excited, could be roused to the performance of certain actions—the quadruped could walk if pushed along, and the bird could fly when thrown into the air. Professor Mayo says, that "if a section be carried through the striated portion of the corpus striatum, the animal will spring forwards and continue to advance in a straight line, till it meets an obstacle, when it still preserves the attitude of one advancing."

Experiments in the Cerebellum.—M. Fleurens next pricked the cerebellum,—there was neither muscular contraction nor pain; he cut off portions of it, but the animal did not appear to be aware of the injury. When, however, a deeper incision was made, when the experimenter cut far into the trunk of the arbor vitæ, there was strangely irregular action of some portion of the frame. One of the pedicles of the cerebellum (crura cerebelli) was divided by M. Majendie; the animal commenced a rapid rotatory motion on its axis, which it continued until prevented by some mechanical obstacle. A deep section of the cerebellum of a pigeon was made, and the bird began to walk and fly backwards. Some of these experiments were curiously diversified by MM. Fleurens and Majendie; a deep section was made in the median plane through the cerebellum of a rabbit; the eyes rolled in an extraordinary manner, and the animal swung from one side to the other as if unable to balance himself. A vertical section was made on one side, and the animal rolled over and over incessantly; the right eye was directed downwards and forwards, and the left eye upwards and backwards. A similar section being made on the other side, the animal stopped, and the eyes resumed their natural direction. M. Majendie divided the crus cerebelli in a rabbit, and for eight days (far, far too long) that this animal survived the injury, it continued to revolve upon its long axis, unless stopped by coming in contact with an obstacle, and when stopped, it ate upon its back with its mouth upwards.

Functions of the Cerebrum and Cerebellum.—The conclusion from these experiments can scarcely be avoided, that the cerebrum is concerned with both sensation and volition—with the first exclusively, and with the second to a great degree: and that the cerebellum is likewise concerned with volition,—that it has the governing and direction of the motion, if not the issuing of the mandate. Dr. Bostock has well expressed this, when he says that "the experiments" (of M. Fleurens principally) "will prove no more than that the cerebellum is the centre of the sympathetic, or associated actions of the nerves that are concerned in voluntary motion."

Different Opinions respecting the Functions of the Cerebellum.—I have often had occasion to regret, that with so many subjects, and so many animals to treat of in one course of lectures, I am compelled, with seeming carelessness and want of candour, to pass over in silence the opinions of some of the best physiologists and pathologists on several points of theory and practice; otherwise it would have been interesting to have inquired into the opinion of Willis, who imagined that "the cerebrum was the organ of the perceptions from the external senses and voluntary motion, and the cerebellum of the involuntary and vital functions; or that of Dr. Wilson Philip, who denies that it has any relation to voluntary or involuntary motion, but does not assign to it any function; that Majendie agrees with Fleurens, that it is the regulator or balance of animal motion; while Gall and others strangely maintain, that the development of the cerebellum bears a relation to the generative faculty.

The Decussation of the Brain.—In describing the cerebrum, I spoke of the medullary matter of the brain as all tending towards one common centre; and likewise of certain commissures or bands, or prolongations of medullary matter, extending from one hemisphere to another, and connecting them together. This may serve to account for the fact often observed both in the human being and the brute, that if a serious injury is inflicted on one side of the cerebrum or cerebellum, the effect is referable to the opposite side of the body. This may be partly accounted for mechanically. When a violent blow is received on the cranium, the extravasation of blood, or the injury done to the brain, and sometimes the absolute fracture of the bone, is on the other side. The pulpy substance of the brain offers to the blow, or to the part of the cranium that receives the blow, a yielding resistance that will neutralise a very considerable force; but in proportion to the impetus which is not thus neutralised, the brain is driven against the unyielding bone on the other side, and is injured. Leaving this, however, out of the question, I believe it to be a fact, that an injury of the cerebrum or cerebellum, or even of the tubercles, is often followed by a loss of function on the opposite side of the body. It is not so with the medulla oblongata or the spinal chord.

The Pons Varolii.—Now, then, we can once more approach the pons varolii. It is the prolongation of the crura cerebelli stretching over the crura cerebri, and I beg you to observe, considering the view which I have just taken of the function of the cerebellum, that is at the base of the brain, and corresponds with the inferior surface of the medulla oblongata and spinal chord. I have already hinted, that the pons varolii is in a direct ratio with the corpus callosum, or superior medullary commissure, and in an inverse ratio with the corpora quadrigemina and the spinal chord. This is the great commissure of the cerebellum, and therefore would probably be proportionate to the other commissures. I have spoken of the corpus callosum in man being large, in order to connect together the medullary matter of the fully developed hemispheres. Compare the bulk of the pons, lessened proportionately in the dog, yet more in the horse, and still more diminished in the ox. I showed the absence of a corpus callosum as the roof of the lateral ventricles in the hare and rabbit, as a type of the rodentia. Observe the little pons varolii in these animals.

The mingling of Medullary matter under the Pons Varolii.—What takes place when the crura cerebri pass under the crura cerebelli, according to the simile of Varolius, like a river under a bridge? Anatomists have talked of the transverse fibres of the cerebellum, and the longitudinal ones of the cerebrum, and of the latter being traced low down through the loci nigri, even to the corpora striata; but whether because the pons varolii is smaller in our patients, or I have not used sufficient diligence, I confess that I have found only faint and uncertain traces of these things; but I have found, and this vertical section of the pons will sufficiently exhibit it, the base of the arch of the bridge composed almost entirely of the medullary matter of the cerebellum; that as it ascends the side of the arch, it is gradually *mingled with and lost in* the darker-coloured matter of the crura cerebri, until when I arrive at the crown of the arch, there is only a thin pellicle of medullary matter remaining. Then at the crown of the arch I have observed two medullary longitudinal bands or tracks of white substance, evidently the continuation of the corpora pyramidalia of the medulla oblongata, and of the central columns of the inferior surface of the spinal chord. I have cut transversely and vertically across the crura cerebri, a little posteriorly to the origins of the third pair of nerves, and the band or track was still visible. I have made a similar section a little more anteriorly, and beyond the origins of these nerves, and all trace of it had vanished. I have returned to the pons varolii, and I have observed a central medullary band occupying the sulcus on the crown of the arch,—an evident commissure,—an evident union of the medullary matter of the two pedicles of the cerebellum; and, looking attentively below this, I have traced a line of separation between the two sides of the bridge, or rather between the crura cerebri which run under the bridge, and I have begun to perceive the central columns of the superior surface. I can fairly make out the rudiments of four of the columns of which I shall presently find the spinal chord to be composed, and some notions of the structure and functions of this part of the brain begin to present themselves, to which however I must not yield too hasty belief. I attach importance, however, to the gradual mingling of the matter of the cerebellum with that of the cerebrum, and this mingling of substance taking place in the direction of the inferior surface of the brain, and also the prolongation of the medullary, I do not yet call it the motor track along the centre of the same surface.

The Medulla Oblongata.—This portion of the brain, of a mingled oblong and quadrilateral figure, I know to be most important, for it will not bear the slightest injury; pressure upon it causes immediate stupor, and if it be cut asunder, consciousness is in a moment at an end. It contains likewise (and we here first of all meet with it) the mingled substance, and probably the mingled influence, of the cerebrum and the cerebellum; and we shall presently see that it first brings to view the origin and source of a new description of nervous influence,—the organic. There are few parts of the brain in which there is apparently so essential a difference between the biped and the quadruped as the medulla oblongata. The small, yet prominent bold surface of the first, forms an evident contrast with the wider, flatter, tamer appearance of the latter. We observe in both the median line or fissure prolonged from the spinal chord, and continued over the pons varolii. It contains the basilar artery.

The Corpora Pyramidalia.—On each side there are two longitudinal eminences—*corpora pyramidalia*, bold and protuberant in the human being, and also in carnivorous animals, as you will observe in this brain of the dog—broader and flatter in the ruminantia and the solipeda. These two eminences are the continuation of the medullary longitudinal bands or tracks of which I have spoken, and the commencement of the central columns of the inferior surface of the chord. The fissure between them is deep; in fact it penetrates through the medulla: it divides it into two parts, which are held together more by exceedingly fine cellular, than by medullary substance.

The Nerves proceeding from the Pyramidalia.—Running transversely across the medulla oblongata, and close to the posterior edge of the pons, and between it and a transverse medullary ridge, that has not yet been noticed by our anatomists, although sufficiently evident, is another sulcus of little depth. The medullary ridge running parallel with the pons, is one of the origins of the portio dura of the seventh pair of nerves, and which springing from the motor column of the spinal chord as well as the lateral column, is, as we shall presently see, a nerve of common motion, as well as an organic one. Springing from this sulcus we also find the sixth pair of nerves, or the abductor; and more posteriorly, and apparently from the sulcus between the central and lateral columns, but actually from the central column arise the linguales.

The Corpora Olivaria in the Human Being.—These eminences are very distinct in the human brain. They are found in the sulcus between the corpora pyramidalia and the lateral column. They are rounded and prominent anteriorly, and gradually sink to the level of the medulla oblongata posteriorly. It is difficult to reconcile the accounts which anatomists give of their structure. Some say that they derive their name from a mixture of yellow cineritious matter which they contain. Others describe an oval white substance surrounded by layers, or folds of greyish matter; but all agree that they are very singular bodies; that they here come first into sight, unconnected with, and not to be traced into anything above, although they may be traced below into the lateral columns, or, rather, they subside, and are lost in them. Behind the corpus olivare is a convex strip or column of medullary matter, which forms the lateral column, and that can be traced the whole of the way down the spine, between the inferior and posterior roots of the spinal nerves, and from which also, in the medulla oblongata, are given out certain nerves devoted to organic life—the spinal portion of the organic nerves.

The Corpora Olivaria in Quadrupeds.—In the ruminantia and the solipeda there is only a faint trace of the corpus olivare to be seen. There is no sulcus dividing it from the lateral column. There is a very slight rounding of the lateral column—in fact, the corpus olivare and the corpus restiforme together, and not to be separated from each other, form the head of the lateral column; and when we cut into the supposed situation of the olivare, we find none of the striæ or arborescent ramifications of grey matter which occur in the human subject. We have simply the two columns, the lateral being abundantly larger than the central. From this portion of the medulla oblongata, there springs another root of the portio dura, and the glosso-pharyngeus, and the pneumogastric—all spinal organic nerves. I shall have much to say of them in another lecture.

The Spinal Chord.—The medulla oblongata now rapidly narrows and terminates gradually in the spinal chord. Some have spoken of a particular change of structure indicating where the one ends and the other begins. I have never been able to satisfy myself with regard to this; I should say that the medulla oblongata is the beginning of the spinal marrow, and that when it passes the foramen magnum, it changes its name. For the consideration of the spinal chord, however, we are not yet prepared; we must a little retrace our steps, and inquire into the structure and functions of the cerebral nerves.

THE CEREBRAL NERVES.

The cerebral or animal nerves are soft white chords proceeding from the medullary portion of the brain to various parts of the head and body, and by means of which impressions from external objects are conveyed to the brain, and sensations are produced there, or the volitions of the mind are conveyed to certain organs, and voluntary motion is effected. I will not enter into the question, whether they are extensions of the substance of the brain, or mere organs of communication between the brain and the various parts of the frame. There are difficulties on both sides. The nerves of pure sensation seem to rise from, or to be prolongations of, the substance of the brain—the nerves of common sensation appear to spring bodily from the matter of the brain—the nerves of motion would seem more to be spread upon the surface of the brain, and to be easily torn off. Whatever be the fact, I must, to render myself intelligible, adopt the popular language, and consider the nerves as emanations from the brain.

The Nerves are Symmetrical.—We cannot regard the base of the brain for a moment, without observing how regularly they arise by pairs from each side of the median line, which, commencing in the brain, pervades the whole frame. There are two complete systems of nerves. It is a kind and wise provision that the senses necessary to the comfort, if not the existence of the animal, should not be destroyed by slight and partial injury.

Comparative Size. — These nerves are, without exception, more developed in the quadruped than in the human being. Compare the nerve of smelling, of sight, or of hearing, in the two. This acuteness of sense was necessary for the existence of the quadruped, and to qualify to be our useful servant. We have that which he seems comparatively to want, the most valuable of all the senses, that of the touch, and, what is yet more important, we have increased power of intellect.

The Neurilema.—Every nerve, as it proceeds from the brain or spinal chord, is covered by a theca or sheath. It is difficult to describe the nature of this neurilema. It most resembles, or, proceeding from the spinal chord, is continuous with, the spinal pia mater. It is denser than the parietes of the vein, or even the artery. In the operation of neurotomy there is sometimes no little difficulty in dividing the theca. The theca is necessary to give support to the pulpy matter of the nerve, and that it may lie in contact with and be compressed by the neighbouring parts without giving pain. When the nerve has reached its point of destination, or is divided into its minutest ramifications, the theca gradually becomes so thin, that the medullary substance is sensible of the presence and agency of these objects to which it was destined to respond—until then it is securely defended.

Substance of the Nerve.—The substance of the nerve resembles much, or seems to be identical with, that of the brain. If, in our anatomical preparations, there seems to be a difference; if we easily preserve the nerves while the medullary matter rapidly putrifies, we should perhaps recollect, that it is the theca and not the substance of the nerve which is thus incorruptible. The nerves being deprived of their theca, or the soft nerves being taken as the subjects of experiments, the same chemical agents act in precisely the same way on the pulpy matter of the brain and the nerves; yet, as the different parts of the brain seem to differ much in structure, the nerves present as striking differences. The proportion and density of the neurilema constitute perhaps the chief difference. There is not only the external theca, but every nerve, even the most pulpy, consists of numerous filaments, or distinct portions of nervous matter, each surrounded by its peculiar sheath. A thousand—ten thousand of these little threads may be bound together to constitute not only a compound, but a simple nerve. The manner in which they are thus bound, may constitute a very great difference in the appearance and function of the nerve. In the olfactory nerve the threads may be large, and the thecæ of gossamer structure. In the optic nerve the filaments have a tubular shape, with a little canal or hollow in the centre. In the pneumogastric nerve there appears to be a single chord only, but it is divided by numerous furrows. In some branches of the ganglial, the filament is surrounded by a net-work or plexus of smaller ones. In the fifth pair many of the filaments are not only large, but the theca is unusually dense, and conjoined with these are numerous smaller filaments.

Blood-vessels of the Nerves.—Every nerve is attended by its corresponding artery usually derived from the nearest arterial trunk. It is mostly seen upon the theca, forming upon it the most complicated plexus of vessels. The capillaries penetrating into the medullary substance, usually carry only a colourless fluid; but they are capable of enlargement, depending on the frequently important pathological distinction, whether it is the neurilema or the nervous pulp that is labouring under inflammatory action. The veins are as readily traced; but, as in the brain, they do not usually follow the course of the arteries, nor even penetrate through the neurilema at the same point. Absorbents must necessarily accompany the nerves in their course, but no dissection or injection, however minute, has yet demonstrated them.

I must not say more until we actually examine the structure, and trace the course, and determine the function of the cerebral nerves, but these somewhat unconnected, and possibly not very interesting, observations, were necessary to prepare us for that examination which will be attempted in the next lecture.

CLINICAL LECTURE

BY

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VARIETIES IN EPILEPSY.

I HAVE an opportunity, Gentlemen, to-day, of submitting to your notice several instances of the varieties in which epilepsy occasionally appears.

You know that epilepsy is usually understood to be a sudden loss of sense, with general convulsions of the body, lasting for a longer or a shorter time, but the convulsions ceasing before the insensibility. Occasionally however we have a variety, and some cases are so singular, that a person not accustomed to see them, would not consider that they were epilepsy.

Partial Epilepsy.

The first variety is, as to the extent of the convulsions. There was an instance here, a week ago, of a boy named William Pitt, fourteen years of age, in whom the affection of the muscles was confined to those of the head and face. He had been ill ten years, and no cause could be assigned for his disorder. His fits consisted in an occasional complete loss of sense. At the same moment his head was drawn back, his eyes turned upwards and also rolled, I believe, outward, and his upper eyelids quivered, went up and down. Sometimes he had thirty such attacks in the course of the day, if he were standing up, but never, I believe, when he lay down. He continued for a minute in the state I have just described. I had precisely such a case a few years ago in this hospital in a boy about the same age. In the other boy the insensibility was so great, that his father on one occasion fired a pistol within a foot of his head, and the boy did not hear it. This was an instance of partial epilepsy. When the convulsions are confined to a certain part only of the body, then it is termed *partial* epilepsy. Occasionally we see them confined to one side, sometimes even to one arm. The fit is perfect so far as it goes—that is to say, there is complete loss of sensibility and considerable muscular agitation, but the latter is confined to one part, and therefore it is called *partial* epilepsy.

There was an instance a short time ago in the hospital of epilepsy, which was represented by the man as likewise partial. John price, ætat. 27, had been ill also ten years. He said his convulsions were confined to the left side, but it turned out that they affected both. They always *began* on the left side, and after continuing there for a time, extended to the right, and so over the whole body. I have sometimes seen the convulsions confined always to one side of the body; and in those instances which I recollect, I think it was generally the left side that was affected; and I may mention that Sir Gilbert Blane, from a calculation which he made respecting hemiplegic cases at this hospital, found that in the majority of instances it was the left side that suffered. However, in this man Price, the convulsions all began in the left side of the body, the left arm and the left leg, and then, after continuing there for a longer or a shorter time, they extended to the right. That is another variety of this disease.

Imperfect Epilepsy.

If any of the symptoms however be wanting in epilepsy, or are not fully formed, then it is called *imperfect* epilepsy—partial referring to the extent of the convulsions, and imperfect referring to the full development, or imperfect development or absence either of the convulsions or the insensibility. If, for instance, instead of what is called convulsions, a person only has a tremor, a shake, then it would be imperfect epilepsy; but there is an instance in the house at this moment, of a man who had imperfect epilepsy in another respect—that is to say, he had insensibility only, not convulsions. Occasionally you will see persons after the decline of life, from fifty onwards, subject to fits of falling down, being insensible for a minute or two, and then recovering, getting up, and being as if nothing had happened. These cases are unaccompanied with convulsions, and are frequently called *apoplexy*, but it is not by any means an apoplectic state. There is no danger in the fit itself, no paralysis is left behind, and persons will have a repetition of these fits for a great number of years without their health particularly suffering perhaps, but at any rate they continue to live; and some persons have these fits only at very distant intervals. They are considered to be fits of imperfect epilepsy, and there is good reason to consider them so, because, after a time, some of these persons have also convulsions, have at last the regular form of epilepsy. In such a case as this, the individual becomes in a moment insensible, falls down, but there is no stertorous breathing, and not that particular blueness of the face that there is in apoplexy, and in a few minutes, perhaps in a moment, perhaps after a longer time, he recovers. Patients will come to of themselves, stir about, and know nothing of what has happened. This is an instance of imperfect epilepsy, where one half the disease is wanting—namely, the convulsions.

Now there is a striking instance of this in William's Ward at this moment, in a man named John Edwards. He has passed the middle period of life, and it seems that he was seized the Tuesday before his admission, having been admitted on the Thursday, with a loss of sense and a loss of motion. He fell down, but shortly recovered from it, and within an hour he had two attacks of the same description, which were followed by severe headach. This I could not help considering as an instance of the affection.

He was treated antiphlogistically, and no paralysis has occurred,—no apoplexy; but the case is not one of simple epilepsy, it is an instance of the combination of different nervous diseases. What we call nervous diseases in general, are merely certain states within, and will produce epilepsy in one degree, and may produce apoplexy in another, and according to the degree of the internal affection and its situation, so you have one disease or the other, and accordingly, no doubt, also, in some measure to the quality of the affection. But this individual affords an instance of the union of a degree of insanity with it. After he had been in the house some time, he ceased to have sudden attacks of this kind, but grew drowsy. During the day he instantly fell fast asleep, and then in the night he lay wide awake and became delirious. Now it is rather a curious circumstance that one has been able to alter the arrangement of this. By giving him a large dose of opium, that is to say, by giving him muriate of morphia every night (for I had no fear of it, after a time, because his case was really not apoplectic, but epileptic), he was made to sleep the whole night, instead of being wide awake, talking incoherently and disturbing the ward. In the day time he is now almost always awake; he is a little inclined to be drowsy, but has much improved. It is certainly rather singular that I have been able to effect so much, as to alter the period of drowsiness; for I was afraid, that in spite of any narcotic that could be given, I should not be able to make him sleep at night—sleeping, as he was accustomed to do, the whole of the day. But such is the case, and the man, although he is not so mad or delirious as he was, is, nevertheless, still rather delirious; he talks nonsense when awake, but not so much nonsense as he did, and what he does say, is not said with so much excitement as formerly. This was an instance in the first case, I have no doubt, of imperfect epilepsy, and it is now an instance of the combination of insanity with epilepsy. The same disease of the brain which produced the one, may, by an increase in intensity, produce various other symptoms. When he was so lethargic, sleeping during the day, I had considerable fear at first, although I knew the case was epilepsy, yet as epilepsy is only a symptom of diseased state within, I had a fear that apoplexy might supervene, and therefore his head at first was kept cool, his mouth was made sore, and means were taken to prevent congestion. Under this treatment, followed by the strong narcotic at night, the man appears to be doing well, yet there may be such a disposition to organic disease, that I may not be able perfectly to recover him.*

Respecting the preparation of opium which he took, I mentioned that it was the muriate of morphia. This has been strongly recommended by Dr. Christison of Edinburgh, as being more uniform than any other form, and attended by less unpleasant effects. It constipates as other forms do, but in general my experience has perfectly agreed with that of Dr. Christison, Dr. Gregory, and some others. It produces simply the narcotic effect, causes the patient to go to sleep, and although it confines the bowels, it has little further unpleasant result. Indeed I only recollect one instance in which I thought it occasioned the slightest inconvenience. The day after it was taken, one patient complained of a little giddiness, or some unpleasant feeling of the head, but this was inconsiderable, and I am inclined to think it will be found one of the best forms that can be given. In general, I am sure that mere opium and laudanum answer all the purposes of practice, and I do not think it necessary to have recourse to refined preparations. I also find that opium and laudanum are quite as uniform as the acetate of morphia, but I do not know that they would be so if the latter were prepared carefully and honestly. The acetate of morphia is frequently not prepared with proper care, and frequently it is intentionally not of the quality that it ought to be. Now this muriate of morphia may be subjected to the same inconvenience as the acetate, if there be carelessness in its preparation on the one hand, or a dishonourable attempt to adulterate it on the other, but in itself it must be, like the pure acetate, uniform (being cheap, there is little temptation to adulteration, and I am inclined to believe that it will be found to answer admirably in every case in which opium is given, and that it is superior in those particular cases where, from some idiosyncracy or other, opium produces some inconvenience. It is nearly four times as strong as good opium, and, therefore, a quarter-of-a-grain dose of it is equal to one grain of opium, or, made into a solution, in the proportion of one grain to 100 minims of water, it is of equal strength with laudanum. It constipated this man as it is found to constipate other people, and therefore it was necessary to give him croton oil to counteract its effect. I understand that it is nearly as cheap as opium itself, so that the solution of it may be exhibited at nearly the same expense as so much tincture of opium. It keeps well in the solid form, and thus may be given in pills. I am satisfied, that whoever will use the muriate of morphine, will never find it necessary to have recourse to any other of the new forms of opium, whether scientific or occult preparations. It is excellent in its properties, cheap, uniform, and capable of being given without the patient's knowledge.

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* This man, under a very generous diet, and the narcotic at night, lost his wandering of intellect, and completely recovered.—Rep.

RHEUMATISM.

There were two cases, which may be worth your attention, presented of rheumatism—rheumatism of a different character; the one acute active rheumatism, the other of rheumatism not increased by heat, not of an active character.

CASE I.

Active Rheumatism.—The first instance was that of a man, ætat. 37, who had been ill three months, and though he had been ill for that time, it was still active inflammatory rheumatism. It affected the hands, arms, shoulders, and knee. The hands and the wrists were swollen, and so was the knee, and not only swollen, but hot and rather red. It was just such a case of active rheumatism as if the patient had only been ill a few days. He had sweated at first at night, and you will generally find this in acute rheumatism; it produces perspiration.

Treatment.—In this case we exhibited colchicum, which always suits cases of rheumatism when there is great heat, or heat increases the pain. He was put on low diet, and took half a drachm of vinum colchici three times a day, and cold water was applied to the parts. There is no fear of applying cold water to the parts in rheumatism, if they be in a state of morbid heat; but if not, it would be absurd. If you allow the water to go to other parts, you may increase the rheumatism, but if you apply it to those in a state of high temperature, so as to reduce the temperature to a natural degree, then you can do no harm, but, on the contrary, will do good.

A gentleman lately told me, that when he was examined he was asked what method he should adopt in a case of acute active rheumatism, and he mentioned the application of cold water to the parts when hotter than they should be. The answer excited some astonishment, and he was questioned for two hours, though not required to give up the point,—the old gentleman who examined him never having heard of such a strange practice before, any more, I may remark, than another examiner, of the use of mercury in acute inflammation, possibly of the liver. The facts which the candidate mentioned, however, though every day witnessed in this hospital, were quite new to him, and hardly proved sufficient to cool his wrath occasioned by the recommendation of such a practice. He said that he had seen it often employed in this hospital with very good effect, that he never saw harm done by it, and should adopt it in practice. Another gentleman, however, who had heard how many questions had been put to the preceding one in consequence of his having mentioned the application of cold water, was afraid to state that he had seen such a practice; he merely said that he had heard of it, but did not venture to stand up for it. I am sure that in cases of acute rheumatism, you will see that the application of cold water is perfectly free from danger, and does a great deal of good. Whenever you find the parts hotter than they should be, and the patient finds it agreeable, you may apply cold water in rheumatism as well as in other complaints. There is no peculiar danger arising from the circumstance of the rheumatism having been caused by cold. Common continued fever is produced continually by persons getting wet through, and yet there is no fear in using cold water, but, on the contrary, great advantage arises from it whenever the heat is steadily beyond the natural temperature. It does not follow, because cold and wet have produced a complaint, that cold and wet may not be useful in getting rid of it. This man on one occasion had eight leeches applied to his knee, eight leeches to his leg, and eight to his shoulder, and after the bleeding, cold water was applied to the parts on which the leech-bites had been inflicted, and he was very soon perfectly well. He was admitted on the 16th of February, and went out on the 27th March, having been well for some time, but harm might have been done had he gone out before.

CASE II.

Atonic Rheumatism.—But in the same ward there was another patient, to whom the application of cold in all probability would have been injurious. It was a case of rheumatism of the right lower extremity. Edward Lawrence, ætat. 31, had been ill two months and complained at his admission of pain of the lower extremity, commencing about the ischium, and behind the great trochanter, and extending down the leg on both sides. I should have stated, that both sides were affected, but the right was much worse than the other. He felt easiest in bed, and he felt better when he was warm.

Treatment.—As the parts were not hotter than they should be, and he was better when he was warm, I conceived that acupuncture would be the remedy for the complaint of the hip and thigh, and, therefore, acupuncture was had recourse to. Two needles were ordered to be placed, one behind the great trochanter, and one lower down on the outside of the thigh every day, and allowed to remain two hours. He was admitted on the 8th March, and the report on the 15th was, that he had been relieved since the use of the needles. When they were in, he experienced a feeling of heat in the part to which they were applied. After two or three more applications the man was perfectly well so far as the parts were concerned in which the needles were applied.

You will learn from this case the description of rheumatism in which the employment of acupuncture is useful, viz., rheumatism not attended, but relieved, by heat. In the next place you will learn, that the needles may be allowed to remain in for two hours, with perfect safety, every day. But you will also learn another interesting and important thing respecting the effects of acupuncture, viz., that they are confined to the particular and immediate neighbourhood in which they are applied. This man had pain down his knee and down his legs; the needles had been applied to the hips and thighs, and they were perfectly cured presently, but the pain remained as bad as ever in the knee and leg. Of course we could not apply them to the knee; at least I should not think of doing so. It is necessary to say, that I would not apply them to the knee, because some persons have written that they may be applied anywhere. They have insisted, from experiments made upon brutes, that they may be introduced anywhere. They say they have passed them into the abdomen of animals; but I should not think of introducing them into the knee. We might, if the pain remained as severe as ever in the knee and down the leg, adopt the same method as was practised in this case. As this was a case for stimuli we applied a blister over the knee, which perfectly cured the pain there, and the patient went out quite well.

This was a very satisfactory case of the utility of acupuncture. In this man the benefit could not arise from the mind: for if the mind had been affected, and the cure had been the result of mere imagination, the leg and the knee ought to have been cured as well as the hip and thigh, but they remained as bad as before the application of acupuncture. Neither was there a possibility of deception in the case, because the cure in the parts occurred at different times. The acupuncture had no effect upon the one and a great effect upon the other. That is just what I have always observed; I have never employed acupuncture without finding that the effects were local in this way. In active rheumatism one cannot see the pain, but you may see the swelling, and feel the heat. In passive rheumatism you have often merely pain; but if this man had been an impostor he would not have said that the thigh and hip were well in the way they were; neither would he have been cured of the rheumatism of the lower part as soon as the blister and rational treatment were adopted. It was altogether a very satisfactory case of the efficacy of acupuncture in this particular kind of rheumatism; but if you apply it where the parts are swelled and hot, it will do no good. It is an excellent remedy, but, like every other, it requires to have the cases selected in which it is applied, otherwise it will fail. If a number of cases of rheumatism come to the hospital, and you treat them all at random, just as mad people have all purged on a particular day, of course no good can be expected; but if you select your cases in this way, you will find the results as satisfactory as they have been in this case.

PURPURA HÆMORRHAGICA.

The hospital is so thin of patients, that I have no more cases to bring before you. There was, however, a case admitted on Thursday, which I will just describe to you. It is of a very interesting disease, one the nature of which is not well understood, and one which we do not see every day. It is an instance of what is called *purpura*, and is well worth your seeing. The patient is ætat. 30, and her body and arms have small red spots; some of a tolerably bright red, and some of a dark red. On pressing upon them, you cannot cause the disappearance of the redness. Down the legs these spots are so numerous, that they are in large patches, presenting the appearance that you have in an individual who has claret marks, stains from original conformation. She states that the spots first appeared about two months ago, and that they appeared first upon the legs, where they are now so numerous as to amount to patches. The ancles are rather swollen. Since last October the catamenia have been absent, and she complains of cough and oppression of the chest. She says that she has been subject to severe headach, but that she had not had it since October. For the last two months her gums have bled, and her breath has been offensive. All this is exactly the disease called *purpura*; and as her gums bleed and she spits up a quantity of blood, it may be called purpura *hæmorrhagica*, but the hæmorrhagic part of the disease is very trifling.

This is a disease arranged by Rayer, not with inflammatory diseases, but with mere congestions of blood, because he conceives that the blood is effused in the spots in which we see it. However, there can be no doubt that the disease is of an inflammatory nature in many instances. In many cases, if you take blood from the arm you find it both buffed and cupped, and great relief is given to the patient. In this affection there is frequently great congestion within the head, so that I once had a patient die here of apoplexy under the affection. There had been great congestion within the head, and a clot of blood was actually found upon the brain. Effusion took place here as in other parts of the body. The lungs are often found excessively congested, and so sometimes is the heart. The liver is sometimes very much enlarged through mere congestion. You know this organ will sometimes acquire a large size from a mere accumulation of blood. Frequently within the stomach and intestines there are red spots and patches like those on the skin, and the accumulation in the mucous membranes is sometimes so great that blood is vomited, blood is spit up, and blood appears in the stools.

In the midst of all these patches and spots, and this effusion of blood, it is not unusual to see the patient look bloodless. I have frequently seen persons in this disease look waxy, as if they were in a state of anæmia, which probably arises from the blood being so much out of circulation, being congested so considerably in the liver and lungs, and in many cases from so much blood being lost into the alimentary canal. This woman looks rather pale. I do not understand the disease at all; I have no idea of that state of the system that causes the blood to be poured forth in this manner, and I do not know that it has been explained. Sometimes the blood is quite firm, but in other cases it is quite soft, will not coagulate. The system is also often in opposite conditions; sometimes there is an inflammatory state, and sometimes there is one of extreme debility; so at least it is said, but in the greater number of instances I have seen, the state has been inflammatory, and the debility has taken place only at last. Whether the blood is much changed in its chemical qualities I do not know. But it would be worth while to have the blood of such patients carefully examined.

Treatment.—As this woman was feeling, or had felt for the last two months, heavy and sleepy, and her pulse was pretty firm, I ventured to take away some blood, but as she looked chlorotic, this prevented me taking away a great deal. I wished to see its state, and eight ounces were abstracted; the blood was not buffy, but firm. I have found colchicum one of the best medicines, and I therefore ordered this patient half a drachm of vinum colchici three times a day. The case is exceedingly worthy of your attention, from its being an instance of a very curious disease, and one you will not see in the hospital every day, though, in the course of a year, one always has a case of it.

Some have imagined the disease to be scurvy, and it may be allied to it in some points, but I cannot believe that it is the same as scurvy; for in scurvy many parts of the body are hard, and the thighs particularly, but that is not the case here. This woman's thighs are soft and flabby. In scurvy you in general see far more ecchymosis in the thighs than anywhere else, and the gums not only bleed, but are spongy and swell to a great amount, but not so in this disease. In scurvy the face is bloated; in this disease it is not bloated, though if much blood be lost, more or less anasarca will take place. Scurvy, too, arises usually from the want of proper food; it does not arise from salt food particularly, but usually from the want of a change of meat and fresh vegetables. But with respect to this disease, I really do not know what produces it; I have not been able to trace it to any of these causes. In no one of the cases that I have seen had there been a deficiency of food, or had the patient been particularly exposed to wet and cold. They resemble each other so far as there are petechiæ, vibices, ecchymoses, effusion of blood under the skin, effusion of blood upon the skin, effusion of blood frequently from the mucous passages. But that is all. In this disease, if a person have had a fractured bone, the callus does not soften, and if an ulcer which formerly existed have healed, it does not break out again.

There is only this resemblance between the two diseases, that there are petechiæ, vibices, ecchymoses, and we might as well I think call hoopingcough and measles and common bronchitis the same diseases, because in those two, as well as in simple bronchitis, there is inflammation of a certain mucous membrane. In measles there is often inflammation of the mucous membrane of the air-passages, and so again in hooping-cough, but the two affections are very different from each other. Because two diseases resemble each other in one particular point, it does not follow that the two diseases are the same. Some have imagined that it is land scurvy, but I cannot see any ground for such a supposition. I hope you will look at the case, for it is one of an interesting nature; and I also hope that it will be cured, but now and then people die of it. I mentioned that I lost a patient myself through the accidental circumstance of a clot of blood being effused into the brain.*

APPARATUS FOR VENOUS INJECTIONS.—RISK OF INJECTING AIR.

To the Editor of THE LANCET.

SIR,—The treatment of cholera by venous injection being at this time the subject of very extensive experiment, I am desirous of suggesting through your journal, that which I think the profession generally are not sufficiently aware of, viz., the extreme tenacity with which bubbles of air *adhere* to the interior of the tubes and other parts of the apparatus employed. Many persons with whom I have conversed imagine, that immediately the liquid appears at the point of the vein tube, the whole of the air has been dislodged from the apparatus, and that they may then safely insert the tube into the vein. My daily experience in fitting up and proving the perfectness of the various modifications of the instruments that we have manufactured according to the suggestions of different practitioners, prove to me that a considerable quantity of liquid must be passed through the *most perfect* apparatus, before the operator ought to satisfy himself as to the safety of introducing the tube into the vein, and for this purpose the point of the tube should be held under water, and the issue of the liquid carefully watched to detect the appearance of air bubbles. It will, *at first*, be found, after two or three strokes of the piston, unaccompanied by air, but the next stroke may exhibit abundance of air bubbles, and prove the necessity of persevering in this preparation of the apparatus until no more air can be detected, which I have generally found to require the passage of about a pint of liquid through the tube.

As a proof that my experiments have not been made on an imperfect instrument, I ought to state, that my observations hold good even when the whole of the apparatus is submersed in water.

Sir, your obedient servant,
J. H. MAW.
55, Aldermanbury, July 13, 1832.

DUBLIN COLLEGE OF SURGEONS.

ILLIBERAL BY-LAW AGAINST PHYSICIANS.

To the Editor of THE LANCET.

SIR,—There never was a period in the annals of the Royal College of Surgeons in Ireland so interesting to the medical journalist as the present; indeed the events of the day are not likely to be forgotten for ages to come.

You may perhaps have already learned, from the Dublin newspapers, that there will be an election for a "Professor of the Theory and Practice of Physic," held at the College of Surgeons on the 28th instant. The reason why such an election is to take place, remains for me to explain. The vacancy in question has been caused by the resignation of a young physician of promise, Dr. Marsh, who did not deem it worth *his* while to hold it any longer. The situation has hitherto been always filled by a physician, but surgeons were, according to the by-law, equally eligible. Since Dr. Marsh sent in his resignation, the College, in a spirit of illiberality I believe peculiar to itself, has altered the by-law, so as to render *none* but surgeons eligible—yes, none but surgeons—to teach the "Theory and Practice of Physic!!!" Yes, Sir, such is positively the case, and the *new* by-law is so worded, that it amounts to the exclusion of all surgeons, save those of the Dublin College! There is a piece of barefaced corporate monopoly for you! Aye, and that too in an age of reform and liberality of feeling! Why make a law to exclude physicians, if surgeons be equally capable of teaching the "Theory and Practice of Physic?" Surely, inasmuch as the electors are all surgeons, *they* would not, without some *very cogent* reason, elect the physician in preference to the surgeon. From what has been done by the College, it is obvious that nothing but a conscientious belief in their own incompetency, or a barefaced love of monopoly, could induce its members to erect so disgraceful a barrier against fair, open, and honourable competition. Let it pass. Such conduct cannot serve *any* public body, much less a "*liberal* profession." The question which now presents itself is—"Who will be the successful candidate?" The professorship in question never brought more than a hundred a year, and many seasons not even half that sum. What surgeon of standing or character, possessing one particle of manly pride, would demean himself by becoming a candidate for so paltry a situation, more especially when its principal inducement—"honourable competition," has been lowered to give him a chance of the chair? Such, Sir, is the fact, and I have been informed that the principal actors in this disreputable affair are already going about offering bribes for a candidate *supposed* to be capable of filling the chair of dignity.

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* This patient very speedily recovered under nothing but the steady exhibition of colchicum.—REP.

In my next I will give you a full and faithful account of the candidates (should there be any), together with such professional news as shall appear to me interesting to the Irish reader of The Lancet.

I am, Sir,
Your most obedient servant,
Observator.

Dublin, July 1832.

COLDBATH-FIELDS PRISON.

FURTHER COMMUNICATION FROM SIR DAVID BARRY ON THE REPORTED CASES OF CHOLERA.

June 27, 1832, visited the Coldbath-fields prison this day, at two o'clock, accompanied by Dr. O'Shaughnessy. Learned from the Governor in his office, that the population of the prison at this moment is about eleven hundred of both sexes, including twenty-one children, that the daily discharges and admissions are about thirty-five on the average; the whole composed of malefactors, vagrants, and paupers; that the daily allowance of food to an adult is (since the introduction of cholera) 1$\frac{1}{4}$lbs. of bread, 1 pint of gruel, 1 pint of oxhead soup, 6 ounces of boiled meat, four times a week, free of bone, and weighed after having been boiled. No soup on meat days.

That the deaths during the spring, summer, and autumn months last year, were only two; during the whole of last year, fifteen. That since the 3rd June, there had been twelve deaths, all from cholera. That the present state of cholera cases in the prison is as reported to him, the Governor,

Men...... 55 cases,
Women.... 15 ditto.

That he had not lately seen them; he could be of no service, and was recommended not to go into the cholera wards.*

Walked with the Governor, Dr. Stevens, and Dr. O'Shaughnessy, round the gardens and other open spaces within the walls, and recommended the prisoner's rooms to be thinned by encamping a part of the inmates in the gardens.

Dr. Stevens, with his two assistants, then conducted Dr. O'Shaughnessy and myself round his cholera wards, in which there might have been thirty or forty patients, but not one in the collapsed stage, though thirteen cholera cases are stated to have been admitted this morning.

There certainly was not a single case which from any symptoms witnessed by me I could point out as a case of cholera. I afterwards made the same observation to Dr. Stevens in the presence of the Governor and Dr. O'Shaughnessy. I repeatedly begged Dr. Stevens to show a recent case, a case of confirmed cholera—a case of collapse. He replied it was unlucky I had come that day,† that there was no case of collapse then in the house, and remarked that by his treatment, he prevents their ever coming to the collapsed stage. Yet twelve have died since the 3rd, two of whom died the day before yesterday; and he (Dr. Stevens) allows that several became collapsed after having been admitted into the hospital of the prison, consequently while under the saline treatment.

One boy admitted this morning, in bed, has had neither vomiting nor purging, and presents a steady pulse.

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* The Governor accompanied me round the sick wards on the 28th, but not on the 27th June.

† I had come, by appointment, with the Dr. himself, made on the 25th at the College of Physicians—D. B.

Two persons looked pale, sallow, and with sunken eyes, as if they had suffered some severe evacuations, but the pulse in both is good. Seltzer water is given as common drink.

Recommended grated openings to be constructed in the walls of the small yards and rooms on a level with the floors. The air in these places being more or less stagnant to a height of eight feet at least.

Dr. Stevens, during our visit to the prison, and afterwards on the way to the Regent's Park, repeatedly asserted, that two-thirds of the patients which he showed us, *would* have fallen into collapse, and *would* have most certainly died if subjected to the ordinary plans of treatment by calomel, opium, stimulants, &c. In short, that the present usual mode of treating this disease is absolutely poisonous. When asked if he had ever seen even one case of blue collapsed cholera saved, that had been treated by the twenty-grain doses of culinary salt, and by nothing else, he candidly admitted that he never had, for that he recommended, in addition to the salt, large mustard poultices, hot saline and opiate enemata, and hot salt and water baths, friction, &c.

The substance of the above notes was written in the prison, or immediately after returning from it, and in their present state were read by Dr. O'Shaughnessy, previously to the publication of Stevens' work on the blood.

D. Barry, M.D.

I have read the above notes, which I saw several days since, and have to state, that they are accurate in every respect.

(Signed) W. B. O'Shaughnessy, M.D.
16th July, 1832.

TWO CASES OF

STONE IN THE BLADDER,

SUCCESSFULLY TREATED ACCORDING TO THE NEW METHOD

BY PERCUSSION.

By Baron Heurteloup, *D. M. P.*

To the Editor of The Lancet.

Sir,—I have the honour of forwarding to you for publication in your valuable journal two new cases, which afford fresh proofs of the advantages possessed by the newly-invented instrument (*the percuteur courbe à marteau*) not only for relieving calculous patients, but for doing so with rapidity. One of the patients, before I operated upon him with the *percuteur*, was subjected, without success, to the application of the *perce-pierre*, as several other patients have been, whose cases are already given in The Lancet. You must, I am sure, be well aware, that such cases are too precious and rich in scientific conclusions for me to omit directing attention to them. I have for a very long time observed the defects and inefficiency of the *perce-pierre*, and have attempted to prove, that to seize a stone in the bladder in order to reduce it to powder, by making *little holes* in it with an instrument, held only by the unsteady hand of an assistant, was a method as dangerous as it was incompetent. Having devised various means with a view to improve the operation, is it not natural and right that I should exemplify the utility and advantages of these means, by proving that they have enabled me to relieve patients who had been unsuccessfully operated upon by other methods? In giving these proofs, it is merely necessary to state, that the instrument resorted to failed, and quite useless to specify by whom it was employed. This is what induces me to pass over in silence the name of the surgeon to whom Mr. Jones (the patient who forms the subject of one of the following cases) first addressed himself, as I refrained from mentioning the names of the other surgeons who operated, without success, on other patients whose cases have been given. I adopt this plan in order to avoid controversies, which are in no way serviceable to science. All that I wish known to the profession is, that by the employment of the *percuteur*, patients have obtained rapid and effectual relief, where the *perce-pierre* has proved unsuccessful. And this all will allow, that I may and ought to publish, although I am grieved to think that in doing so I may interfere with certain pretensions. Those who oppose and condemn my labours, and repudiate my ideas on *lithotripsy*, have one effectual means of maintaining their opinions, and giving them weight, which is to *succeed* with the system they advocate, and which they think requires no improvement in relieving all the patients who come under their hands. Were that the case, I should consider myself surpassed in fair fight; and I can further assure them, that I never would employ in my defence, arms which all upright and honourable men condemn, but should direct all my efforts to recover myself by seeking for the means of self-improvement. I am, Sir,

Your obedient servant,
Baron Heurteloup, M.D.
1, Vere-street, Cavendish-square.
July, 1832.

CASE I.

About thirteen years ago, Mr. B***, 59 years of age, under sheriff at Ipswich, a man of good constitution, observed from time to time some red sand deposited at the bottom of his urine, and occasionally felt slight pain in the left kidney; he continued for the first five months to pass fine powder, but after this time, the grains, round and smooth, gradually became larger, and at last resembled mustard-seed. In 1820 he came to London, and consulted the celebrated Mr. Cline, who prescribed carbonate of soda; the patient took this medicine at different intervals for four years, but this did not prevent the formation of gravel, which, on the contrary, increased in size. In 1825 the calculi voided resembled small peas. Mr. B*** then consulted Dr. Williams of Ipswich, who recommended him to take magnesia, but it had not the desired effect; the patient still continued to pass a great number of calculi; sometimes, when they were small, thirty or forty at once; at other times three or four larger ones. These continual evacuations of stone were unattended with pain, and the patient's health was not at all affected until 1826, when he had an acute attack of nephritis, accompanied with extreme difficulty to make water, and occasional copious discharges of blood. This attack was treated with opiates and antiphlogistic means, and on the second day Mr. B*** experienced an unusually violent desire to make water, and voided a calculus much larger than any of the others; this afforded him great relief, but it was of short duration; the former pains soon returned, and became complicated with other symptoms, clearly indicating the existence of a stone in the bladder. He again consulted his medical advisers, and various remedies were tried, but with little or no success. No sooner did the symptoms assume all the characteristic features of stone in the bladder, *than the patient ceased entirely to void gravel.*

Mr. B*** seeing at last that he could neither ride on horseback, nor even walk, for ten minutes, without suffering excruciating pain, and discharging considerable quantities of blood, followed Dr. Williams's advice, and came to London to be sounded. It was also his intention to seek for information respecting the operation of lithotripsy, which he resolved to undergo, having the greatest dread of lithotomy, and having decidedly refused ever to submit to its performance. He consulted Mr. Brodie, who having felt the stone in his bladder, did me the honour of recommending him to me.

With the *recto-curvilinear catheter* I discovered that there was a good deal of sensibility in the urinary organs, a moderately capacious urethra; but small at the external orifice and very contractile, a rather large and regular bladder, but a contracted *bas-fond*; the stone was about the size of a large walnut, flattened, smooth, and moveable.

In *five* applications of the *percuteur courbe à marteau*, of from two to three minutes each, the stone was reduced to powder and pieces sufficiently small to be voided with facility by the urethra.

Mr. Brodie and Sir David Barry witnessed one of the operations, and Mr. B***'s brother-in-law was present at all but one.

Reflections.

Now that the mode of action of the *percuteur courbe* is becoming generally known, this case does not present any very interesting feature; it is a proof, however, additional to the numerous cases already published, in which the *percuteur* was employed, of a cure (although the stone was of considerable size) being effected rapidly, and with little pain or danger. This result was obtained without subjecting the patient to the long *sittings* which are unavoidable when the *perce-pierre* is used, in cases of stones of sufficient magnitude to require much *boring* before they give way. With the *percuteur* the applications are always of short duration, the bladder is not much fatigued or irritated, and the operations may, when necessary, be continued, safely, at short intervals.

The thing most worthy of remark in this case is, that as soon as a calculus remained confined in the bladder, the evacuation of gravel, to which the patient had for so many years been subject, *immediately and completely ceased.* This circumstance often occurs; and it is well to notice and remember it.

It is interesting to inquire how we are to explain, that a patient in the habit of passing gravel (which is formed generally in the kidneys) should in almost every case cease to do so as soon as a calculus grows too large to pass out by the urethra. When the patient's urine has previously deposited powder only, we can conceive that this deposition takes place on the foreign body contained in the bladder, and is not sufficiently abundant to supply a sediment at the bottom of a vessel; but the explanation is not so easy when the patient voided small calculi before; and yet the fact is often met with; and Mr. B***'s case is an example in point.

Does a stone in the *bladder* modify the secerning action of the *kidneys*, and does this modification, supposing it to exist, tend to prevent the formation of calcareous matter?

CASE II.

Mr. Jones, ætat. 59, of South Street, Manchester Square, of rather a robust constitution, has, during the last twelve years, been in the habit of voiding calculi, varying from the size of a pea to that of a small horse-bean, at intervals of about two years. These evacuations continued till October, 1830, and were sometimes accompanied by the symptoms indicative of the formation of gravel in the kidneys, and its passage along the ureters. In October, however, 1830, the patient, after having for some time before experienced a considerable increase of pain, voided a calculus much larger than any of the others; he hoped that this would afford him relief, but he was painfully disappointed, for the former symptoms not only continued quite as severe, but became complicated with others of a more serious nature, presenting all the marked features of stone in the bladder.

Mr. Jones took the prudent part of immediately consulting Mr. Chesterman, his medical attendant, who prescribed soothing medicines, and in November commenced dilating the urethra by the passage of bougies, which facilitated the escape of a calculus of the size of a marrowfat-pea. Still the symptoms remained unabated. In February, 1831, the patient was sounded, and a calculus was distinctly felt. Mr. Davis, a medical gentleman, and a friend of the patient, recommended him to have recourse to *lithotrity* for the removal of this stone, and he introduced him for this purpose to a practitioner who "undertakes this branch of surgery." This surgeon performed the operation with the *perce-pierre*, but unfortunately could not succeed in seizing the stone, notwithstanding many long and painful manœuvres. This fruitless attempt greatly deteriorated the state of the patient; the bladder became extremely irritable; the urine was voided much more frequently, and became catarrhal and purulent, and the pain was more acute and lasting. These unfavourable symptoms continued with little abatement for about eight months, during a part of which the patient was confined to his bed. In order to obtain relief from so grievous a state, Mr. Jones had recourse to a quack lithontriptic medicine, known as *constitutional water*, of which he drank a great quantity without benefit.

In the month of February, 1832, Mr. Jones again consulted Mr. Chesterman, who recommenced introducing bougies, and dilated the urethra to some extent, but this time without effect; the symptoms continued of the most intense kind, and the patient found no easy moments, except from the introduction of opium suppositories.

Finally, about eight or nine months after the first attempt made with the *perce-pierre*, Mr. Jones submitted a second time to the application of this instrument by the same surgeon. The result of this attempt was not, however, more favourable than that of the former one; and it was after the patient had for nearly a year been labouring under all the serious derangements of health caused by these unsuccessful operations, that he, according to the advice of Mr. Chesterman, consulted me about two months after the last one, and desired to place himself under my care.

By a *methodical catheterism* I discovered a rather large urethra, and an exceedingly contractile and very sensitive bladder, presenting so little room in its *bas-fond* as only to allow the instrument about half an inch play from before backwards; the stone was situated laterally, and appeared wedged in; it appeared of a rounded shape, and about as large as a good-sized walnut.

I consented to perform the operation with the *percuteur courbe*, although the extreme sensibility of the bladder, and the little space to be found at its *bas-fond*, gave me some doubts as to the possibility of effecting a rapid cure. My doubts however soon disappeared, for in the first application of the *percuteur*, this stone, which with the *perce-pierre* could not be seized, was immediately taken and comminuted; in two other applications at short intervals the remaining portions were pulverised in the same manner.

In the course of the treatment, several considerable fragments become so firmly lodged in the urethra, that it was necessary to extract them by the employment of suitable means, to which I alluded in a former case.

Mr. Chesterman, the patient's medical attendant, assisted at the three applications of the *percuteur courbe*, and Mr. Jones is now perfectly well, and free from the slightest sensation that can remind him of his ancient complaint.

Reflections.

This case is simple, but it is instructive; for it shows the comparative goodness of two different instruments; we see what serious consequences followed the unsuccessful application of the *perce-pierre*, and how rapidly the patient was relieved with the *percuteur*. Although I think it possible with the *perce-pierre*, in skilful and practised hands, to relieve a patient who has in his bladder a stone as large as a walnut, I do not think that the cure can be, by a great deal, as rapid, and above all as *safe*, as I have observed it to be with the *percuteur*. In the present case, however, I very much doubt whether the employment of the *perce-pierre could have been followed with success*, although the stone was not very large; for it was firmly retained in the lateral portion of the bladder, where it can readily be seized by the *percuteur*, but cannot be seized by the *perce-pierre*, which can only safely grasp a stone that is placed in the axis of the instrument, directly opposite the neck of the bladder. It is because those who have been engaged in the study of *lithotripsy* have not sufficiently studied the influence which the situation, size, and shape of calculi, have on this operation, that these important points have been overlooked and considered too lightly, and that evident deficiencies in this art have not been discovered and remedied; it is, unfortunately, for the same reason also, that some operators obstinately persist in treating patients by means which they know must so frequently cause the most fatal consequences. I regret to say that in Mr. Jones' case, notwithstanding the deplorable result which followed the two first applications of the *perce-pierre*, the surgeon who operated would have renewed the imprudent attempt. If we reflect a moment, we shall see the dangerous consequences of such perseverance. I make this reflection, in order to prevent, if possible, an operation, safe and valuable in itself, from being exposed to reproaches of which it is undeserving. I will here make another important remark, which will still more clearly show how inapplicable the *perce-pierre* was. On account of the short distance from the neck to the posterior part of the bladder, there was not sufficient room to expand the branches of this instrument; for, by means of the *percuteur*, which from its form and action gives a very correct estimate of this space, I ascertained that there was not more than an inch; and in order to grasp a stone of the size of a large walnut with the *perce-pierre*, the branches must be projected two or three inches from the tube which contains them; it is evident, therefore, that the hooks of the branches must have been in immediate contact with the coats of the bladder, which could not have been unattended with danger.

Finally, I terminate these reflections by remarking, that when the fragments became entangled in the urethra, there was a copious discharge, which ceased entirely when they were extracted with the instruments of which mention has already been made in General M.'s case. I have sometimes, but rarely, seen this discharge supervene in similar cases.

"REASON OF THE VERDICT AGAINST RYAN AND CO."

[As Dr. Ryan seems to think that the publication of the following letter will be of service to him, we most readily give it a place in the pages of this Journal. The debateable points in it, however, are almost too prominent to require any elucidation in the shape of comment; but it is just possible, and no more, that we may bestow a few words on it in the next Lancet.—Ed. L.]

To the Editor of The Lancet.

Sir,—It was my determination not to notice your remarks on the late libel cases, so long as your statement of the evidence in the trial of Ramadge *v.* Ryan was correct; but I observe that you have inadvertently, no doubt, for you were not present, made many misrepresentations of it, which I cannot in duty to myself allow to pass unnoticed.

You state in your last Number, that the Chief Justice, Jury, and Counsel for the plaintiff, dwelt with much earnestness upon the fact, that the alleged libel had not been copied precisely as it was first published, that Dr. Ryan was here utterly defenceless, his counsel had nothing to urge in mitigation or extenuation, and that Mr. Serjeant Taddy should have been instructed to point out the difference after the manner you suggest. This is the substance of p. 473 of the last Number of The Lancet.

In reply I must state, without meaning or intending you any disrespect, that the above is a gross misrepresentation of what happened in Court. The Chief Justice told the jury, that the libel copied from The Lancet was the only part of the article in the *Medical and Surgical Journal* for their consideration, and that the defendant's comment was of minor importance. It is not true that I was defenceless, and that my counsel had nothing to urge in extenuation, &c., for Mr. Serjeant Taddy gave ten times a better explanation than you suggest, and showed that the difference of the copy was in my favour as less libellous and offensive to the plaintiff, as I had expunged your title, "Result of upholding Quacks—the tea-spoonful doses of calomel, the name and residence of the patient, &c." Now all this appears in the authenticated report of the trial published in the *Medical and Surgical Journal*, July 7th, a week before you made the above statement; but it is not a pleasant thing to quote a rival publication, and it is also wise in you to endeavour to make it appear, that our cases were different, and that the verdicts should be different. But this special pleading is too shallow, it will never do; "it would," as was well observed by *The Times* (June 28th ult.), "be a gross libel upon the administration of the law in this country, that the decisions" (in our cases) "are to be explained by reference to the different modes of conducting the two trials. We fear that the measure of justice which varies with the power of the advocate, is not likely to be very satisfactory either to the parties concerned or to the public in general." It is manifest to every rational man in the kingdom, that if the plaintiff's reputation was only worth a farthing on the Monday, it could not be worth 400*l.* on the Tuesday, and the reverse. All the special pleading and logic of which you are master, will not enable you to controvert this fact.

Again, you condemn my offering to apologise, and my subsequent justification. Now I ask you in reply, is there an editor in the kingdom, except yourself, who would refuse to insert an apology, if an individual complained of having been misrepresented in his journal? Do we not daily observe *The Times, Herald, Courier, Globe,* &c., follow this course? But if the complainant should say, No, I shall bring an action against you,—would not any such defendant plead a justification if he could? The fact was, that every plea in my justification was proved but one. The article alleged that 96 leeches were applied, Mrs. Reynolds proved that 140 was the number. It was alleged Dr. Tweedie refused to consult with Dr. Ramadge, &c. Dr. T. proved the fact. It was alleged that Dr. Tweedie reversed the treatment pursued by Dr. Ramadge; this was proved by Dr. T., who would not consent to more leeching, and ordered beef-tea, morphia, &c. It was alleged that the lady improved under this treatment—this was proved in evidence, but she was not recovering in August as stated in THE LANCET, nor recovered in September, as stated in the *Medical and Surgical Journal.* This was the only part of my justification that failed; but I ask any practical physician or surgeon, who could suppose that if the lady was recovering in August, she had not recovered in September, more especially as the first statement was left uncontradicted by Dr. Ramadge? Neither was it the first time that doctors differed, and that a patient recovered under an opposite mode of treatment. The lady was dead when your journal stated she was recovering, and when mine said she had recovered, and yet you had a farthing damages awarded against you, and 1 400*l.* You may flatter the juries as you please, but the verdicts were awarded by them, and not by the judges or counsel. Had an equity judge decided the amount of damages, the verdicts would have been totally different. I might dilate upon trial by jury in civil cases, but prudence compels me to decline doing so at present; though a time will come when I may prove beyond the possibility of doubt, that men get into the jury-box whom justice, common sense, and public opinion, declare unfit for the situation. In saying this, I do not refer to the jury who sat on my case, but to juries in general, who frequently are men of contracted minds, and totally unable to distinguish between assertion and evidence, between the turbulent declamation and false assertion of lawyers, and between the quantum of injury done one party, and the amount of reparation, if any, to which he is entitled.

I am, Sir, your obedient servant,
M. RYAN, M.D.
Editor of the London Medical and Surgical Journal.

CASE OF

MELÆNA,

By WILLIAM JACKSON, *Esq. M.R.C.S., and Lecturer on Anatomy, Sheffield.*

MRS. G., the subject of this case, is a married woman, æt. 30, of spare habit and delicate constitution, but had hitherto enjoyed a moderately good state of health. On the 26th of March Mrs. G. was seized somewhat suddenly, after having been engaged in rather severe exertion, with pain at her stomach, followed by vomiting to the extent of a quart of a fluid resembling coffee-grounds, mixed with coagulated blood. Alarmed at the unusual appearance of the ejected fluid, medical assistance was promptly procured, when the following symptoms were presented:—Oppression and pain at the scrobiculus cordis; fulness and tenderness throughout the whole abdomen; tongue slightly furred and brownish; pulse 110, neither full nor strong; constipation of some days standing, although at this period it was reported that two or three ineffectual evacuations had just occurred. The following means were adopted:—

> *Pil. hydr. subm.* gr. v; *statim sumend. et postea;*
> (*Hora* 4) *Capt. ol. ricini* ℥j.

27. The vomiting had returned during the night, and the contents of the stomach exhibited the appearance of soot and blood intermixed with water, the blood appearing in coagulated shreds. It was not clear whether the medicine had been rejected or not. There had been only one evacuation per anum, which presented the same appearance as the fluid just described. Pulse 120 and small; tongue moist, and covered with a thin and brownish coating; a sense of great debility; urine in small quantity, and of dark colour; thirsty; rigors and hot fits; much anxiety; palpitation of heart and pulsation of the arteries.

℞ *Hydr. subm.* gr. xii, *in pilulas* xij,
fing. quorum capt. q. hora.
Rep. ol. ricini ; mane sumend.

28. The symptoms, generally, aggravated. The gastric and alvine evacuations presented the same pitchy appearance, intermixed largely with coagulated blood. The quantity being greater than before. There was now considerable spasmodic pain in the abdomen complained of. A fourth part of the following mixture was directed every four hours till the pain should be relieved :—

Acid. nitr. ʒss ;
Tr. opii ʒj ;
Aquæ m. p. ℥j ;
Aquæ ℥iij. *M.*

After which the pills to be resumed.

29. Evacuations from the stomach and bowels copious, and of the same appearance as before. A more alarming state of debility now presented itself, a state which reminded the practitioner of experience of one who had recently sustained a great loss of blood. There was anxiety, palpitation of the heart, throbbing of the temple, great sense of faintness upon attempting to rise, with a sharp pulse at 125. The pills and oil were persisted in, and a dose of the mixture occasionally.

30. The evacuations as before ; faintness urgent ; palpitation distressing, so as to visibly agitate the whole frame ; frequent flushings ; urine scanty ; thirst great.

℞ *Infus. ros.* ℥vj ;
Magnes. sulph. ʒvj. *M. sumat. pars*
4ta, tertia quaque hora vespere.
Habt. ol. ricini ℥j ;
Et repetantur pil. calomel.

In the night the following mixture was directed to be given (a quarter) every fourth hour.

Ol. terebinthinæ ℥ss ;
Mucil. acaciæ ℥j ;
Syr. simplicis ℥ss ;
Aque linnam ℥iij. *M. ft. mist.*

31. The oily mixture had been ejected by vomiting, but had still been persisted in. The general sense of debility and faintness was unabated. The evacuations, which were more copious per anum, were of a somewhat more natural appearance, and contained several large, globular, and hardened masses, of a dark-green colour, intermixed with the black-coloured fluid previously described. Palpitations distressing ; pulse 125, and small ; countenance pallid and anxious. The patient now complained in a decided manner of a sense of bearing down at the anus, and had made several ineffectual attempts to evacuate, which circumstance, together with the fact of the hardened fæcal substance just passed, pointed out an indication (if not a cause of this disease) which ought to guide the future practice.

℞ *Hydr. subm.* gr. v. *statim s. necnon ;*
Ol. ricini ℥ss. *3tia q. q. hora ;*
℞ *Ol. terebinth.* ʒvi ;
Ol. ricini ℥ij ;
Aquæ tepidæ ℔ j. ɱ *ft. mist. pro enemata, quamprimum injiciendum.*

April 1. The medicine had been rejected only once, and an hour after the injection copious fluid evacuations, and of a ligher colour than before, were passed. The vomiting had been less frequent than before, and the fluid of a more natural colour. The pulse was reduced, being 110, and more full ; palpitation still continues ; tongue moist, but coated with a brownish furvetigo, especially on raising the head ; syncope. In the evening the injections were repeated.

2. Nausea and vomiting have subsided ; the evacuations per anum copious, and more natural in colour ; still feverish ; tongue improved ; pulse 100 and quick ; palpitation, and sense of general throbbing, especially on raising up the head, most intolerable.

Free ventilation was directed ; nutritious broths and gruels allowed, and perfect composure enjoined. The action of the intestines was kept up by small doses of *sulphate of magnesia* in the infusion of roses, and a draught containing 20 drops of *liq. op. sedativus* given at bed-time.

It would occupy too much of the space of this journal to detail the treatment of the case. I shall therefore merely state, that aperients were steadily persisted in for two days longer, together with the administration of opium, which had the effect of tranquillizing the irritability of the circulating system, as in other cases connected with profuse hemorrhage. The quinine in the infusion of roses was afterwards administered and persisted in for a few weeks, when the strength of my patient became perfectly restored.

Remarks.

This disease is generally considered to be the result of some organic affection, or of a constitution breaking up, and therefore the prognosis is usually unfavourable. In the case just detailed, no organ could be said to be unhealthy, nor was there any assignable cause, excepting that of constipation of the intestines, favoured by a constitution characterized by a laxity and delicacy of fibre. The effects resulting from the free evacuation of the bowels were highly interesting. Here was the axiom verified—*Causa sublata, tollitur effectus.* In No. 445 of THE LANCET, Dr. G. Jackson gives a case of melæna, treated successfully by the oil of turpentine, in which case mercury was also largely exhibited until the mouth was affected, thus showing some doubt as to the share which either medicine might have had in producing the favourable result.

In the case of my patient, I may justly ascribe the success to the purgative medicines, for although some of the consequences of the disease remained after the clearing of the intestinal canal, yet the genuine character of the disease (the pitchy and bloody dejections) had disappeared. The palpitation and throbbing were symptoms which continued most distressing throughout the period of convalescence.

It is probable that a greatly distended state of the colon might obstruct the return of the venous blood through the superior mesenteric vein, or the trunk of the vena porta, and the blood so detained would become effused upon the inner surface of the intestinal canal, where the branches of that vein originated. Turpentine has been successfully employed in this, and diseases resembling it in their nature, and appears to derive its utility from its stimulating effects upon the terminations of the vessels supposed to be the seat of the discharge. Its efficacy in external hemorrhage is well known.

Sheffield, June 3, 1832.

EXTRACTION OF A CALCULUS

FROM THE FEMALE BLADDER,

AFTER USING WEISS'S DILATOR.

To the Editor of The Lancet.

Sir,—The rare occurrence of cases of calculus in the female bladder will, I trust, be a sufficient excuse for my requesting you to insert in your valuable Journal the annexed case as early as convenient.

I am, Sir, your humble servant,
S. Brougham, M.R.C.S.
Falmouth, Cornwall, July 6, 1832.

Elizabeth Williams, ætat. 19, of spare habit and chlorotic temperament, consulted Dr. Boase in August, 1831, having suffered severely for some time from dysuria, accompanied by distressing symptoms of irritable bladder. She experienced considerable relief from the exhibition of ura ursi and alkaline remedies, but after a truce of a few weeks her complaint returned with increased severity; and on again applying to Dr. Boase, he expressed his conviction that there was a calculus in the bladder. She was accordingly transferred to my care. On introducing a sound, I found the diagnosis perfectly correct. At this time her sufferings were so much aggravated as to be almost insupportable. Although the stone appeared to be of considerable size, both Dr. Boase and myself thought it would be advisable to attempt its removal by means of Weiss's dilator.

The operation was commenced on Monday, Oct. 10, by myself, Dr. Boase being obliged to leave Falmouth for a few days on a professional visit.

The patient being placed in the usual position for lithotomy, I introduced the dilator at 10 a.m., and continued gradually and at intervals to expand the blades until they were separated rather more than one inch, which occupied about four hours and a half. The dilator was then withdrawn, and on introducing my finger it came immediately in contact with the stone.

The patient was now so much exhausted, that I thought it prudent to defer the extraction till the next day. The dilator being again introduced and extended, she was removed to her bed, and, after taking an opiate, soon fell into a quiet sleep. I shall copy the remainder from my case-book.

Oct. 10. Nine p.m. Tranquil; does not complain of much pain. Dilator extended.

11. Eight a.m. Passed rather a restless night; very reluctant to have the dilator further extended. The finger can now be readily introduced into the bladder between the blades of the instrument, but the urethra is still not sufficiently dilated to allow the passage of so large a body as the stone appears to be. *At* 10 *a.m.* I was called to a very urgent case at some distance, and during my absence a friend of mine (Mr. F. Ward, who was then here) superintended the patient, in whose surgical skill I have the highest confidence, and for whose valuable assistance on this and on other occasions I feel deeply obliged.

At 5 *p.m.* we met and found our patient in better spirits, and anxious to have the operation completed. I should have said that during the day the dilatation was kept up. On removing the dilator the urethra was found sufficiently dilated to admit the passage of two fingers. A pair of middle-size lithotomy forceps were then introduced, and on attempting to seize the stone, the bladder contracted to such a degree as to expel every drop of urine, and firmly to embrace the forceps. After waiting a short time the forceps were expanded, and the stone was firmly grasped. It was now evident, from the great extension of the blades, that the stone was of an unusually large size, but by a steady and continued motion, it was brought half way down the urethra, when unfortunately a portion of its outer lamina gave way, and the forceps slipped. In endeavouring to grasp the stone again, it returned into the bladder. This organ again became so spasmodically contracted, that the finger could not be passed between its lining membrane and the stone without great difficulty. At this time we were compelled to combat the extreme exhaustion of our patient with forty minims of tinct. opii, and frequent exhibition of wine. A larger pair of forceps was then introduced, the stone firmly grasped, and, by the assistance of pressure with two fingers in the vagina, was at length extracted. The bladder was then carefully examined, and the patient removed to bed. Very little blood was lost, and as far as a careful examination could ascertain, the urethra was not ruptured or lacerated, except slightly in its lining membrane. A full dose of opium was administered, and quietness strictly enjoined.

October 12. Nine a.m. Slept several hours during the night; face flushed; skin hot; pulse sharp, 120; does not complain of pain; no tenderness of abdomen; bowels relieved once; urine slightly tinged with blood; fomentations to pubic region.

Ol. ricini, ʒvi *statim.*

13. Has passed a good night; bowels open twice; pulse 100, more natural. In the evening the pulse rose to 120, was sharp; restless; complains of pain in the pubic region on pressure.

Applic. hirudines xxx, *et fomenta*;
Habeat ol. ricini, ʒvi *statim.*

Has passed at one time four ounces of urine.

14. Has passed a good night; pulse 100, soft; skin moist.

16. Going on very well in every other respect, except the urine passing involuntarily. From this time her health gradually improved, though, for a considerable period, I had to contend with extreme debility. I regret there has remained permanent incontinence of urine, a result which I fear must always attend the extraction of so large a calculus (entire) by means of Weiss's dilator, until that instrument is so much improved as to bear with more equal pressure on all sides of the urethra.

The facilities now offered by lithotrity are so great, that should another case, similar to the above ever present itself to me, I should certainly have recourse to that mode of operation, having previously dilated the urethra sufficiently for the passage of a moderate-size calculus.

It is to be remarked that this patient retained perfect command over her bladder when the urethra was dilated to the extent of more than an inch, which would allow ample space for the operation of breaking up the calculus.

The calculus has not been subjected to chemical analysis, but its external characters indicate it to belong to the lithic acid species. Its weight, when extracted, was three ounces, five drachms, ten grains. It measured in its largest circumference seven inches, and six in its smallest; was of a roundish figure, smooth, and slightly flattened.

THE LANCET.

London, Saturday, July 21st, 1832.

The trial of Kinnear *v.* the Rock Office, which we inserted last week, will afford a fund of amusement, and some little instruction, to our juvenile readers in town and country. O how the youngsters will shake their sides, as they peruse Mr. Lovegrove's discovery of "the detection of mineral poisons made easy for the express use of King's counsel, hospital surgeons, and bantling lecturers on medical jurisprudence!" How they will stare as they pursue the sagacious inferences of Charles Aston Key, *senior* surgeon to Guy's, from premises, which we shall presently show would lead any one else but an hospital surgeon and a lecturer to diametrically opposite conclusions. And then the more cautious but equally sensible insinuations of Professor Joseph Henry Green! And last, the solemn ex-cathedra *opinions* of Mr. Alfred Taylor, the learned but somewhat beardless lecturer on *medical jurisprudence* (!) at St. Thomas's Hospital.

Let us briefly look at the nature of the case. A gentleman of unexceptionable moral character, and of liberal fortune, goes to bed in his usual health at 10 p.m. on the 19th of October, rises and visits the water-closet at midnight, rings his bedroom bell at 9 the next morning, and in three hours after is found at his last gasp. His body is incompletely examined by several medical men. They find three pints of blood in his stomach, but cannot trace the source of its effusion. They find some ounces of serum tinged with blood in each cavity of the chest, and they find the blood-vessels of the brain *slightly* turgid. They make no examination of the heart, and none of the spinal chord, and they give it as their opinion that the deceased died from "rupture of a blood-vessel in the stomach."

Well, the deceased having insured his life to a large amount, "a little bird" (we have heard he was brought up to the medical art) betakes himself to the councils of the insurance office, whispers to the directors that the examiners of the body were all bunglers, that nothing would be less difficult than to tear their opinions to atoms in a court of justice, and then that a few dexterous insinuations might settle the affair. At all events it was calculated very judiciously, that as the case was an unusual one, there could not be wanting a few *distinguished* medical gentlemen who might take views of it not unfavourable to the interests of the office. Mankind, especially the medical department thereof, as the directors knew, are remarkably prone to causation, or what is vulgarly termed suspicion; and if they could not account for death on the facts elicited by the examination of the body, why nothing would be more natural than that they should fix on "*poison*" as the cause of the fatal event. It is just what always happens in these cases. The PRINCESS CHARLOTTE was "poisoned." General HOCHE was "poisoned." The Crown Prince of Sweden was "poisoned." It is the very easiest way, in fact, to account for every mysterious instance of sudden death; and of course far preferable, in a medical point of view, to a disreputably candid confession of ignorance on the matter.

Mr. LOVEGROVE was the first medical witness. As one of the original medical reporters, he deposed to the pathological facts we have related, and then, hop, step, and jump, he leaps at the conclusion, "that rupture of a blood-vessel was the cause of death." Had he paid a little more attention to the nature of cause and effect, he would have done no more than state, that he could not positively infer the cause of death from the facts before him, but that the presence of so much blood in the stomach led him to *believe* that death was the event of natural disease.

The presence of this blood constituted a *positive* fact indicative of some morbid process, and a fact sufficient to outweigh every negative circumstance which could be brought forward. But Mr. KEY, it seems, thought otherwise. "Gastric hemorrhage was inconsistent," according to his notions, "with turgidity of the vessels of the brain." Accordingly, though the presence of this blood in the stomach was a *positive* fact, which no cross-examination could distort, yet, argues Mr. KEY, there was turgidity of the cerebral vessels, ergo, there was *no* gastric hemorrhage! This difficulty being surmounted, Mr. KEY sees nothing but poisoning by laudanum in the other circumstances of the case. The cerebral vessels were turgid in Mr. KINNEAR, so they are in laudanum poisoning. Mr. KINNEAR died in 12 hours from the commencement of his illness, so do those poisoned by laudanum. To Mr. KEY's mind these two circumstances seem proof "like holy writ." Did it never occur to Mr. KEY that he was sworn to give his opinion, not on the evidence of one witness, but of the entire? Now, THOMSON had sworn that his master's bell rung at 9 a.m., about two hours before his death. THOMSON's testimony was not impeached, and consequently Mr. KEY had no right to disbelieve it. Yet in the teeth of this all-important fact, Mr. KEY infers poisoning by laudanum! Mr. KINNEAR, although dying from laudanum, two hours before his death possessed enough of volition and of locomotive power to walk across his room and ring the bell! It is useless for Mr. KEY to say that he did not believe the testimony of THOMSON. He was bound to believe it, until the witness's character was successfully impugned.

Almost the same remarks apply to the testimony of Mr. GREEN. But we must do him the justice of admitting that he was too cautious to go further than an insinuation,—a guarded, though influential insinuation,—that death was caused by laudanum. Both the gentlemen also forgot that no medico-legal examination can justify any opinion on negative evidence as to the cause of sudden death unless the spinal chord is examined. Our readers will recollect that the body of the late Miss CATHERINE CASHIN was exhumed for this express purpose after a previous scrutiny of the cerebral, thoracic, and abdominal viscera.

Mr. ALFRED TAYLOR, the last witness, the *teacher*, by the way, of forensic medicine at St. Thomas's, agreed in the opinions of Messrs. KEY and GREEN! As a skilful jurist *he* knew that he was bound to take *all* the evidence into consideration. Accordingly he swears "that, from *all the circumstances*, he inferred that a narcotic poison had been the cause of death!" This was a plain, straightforward opinion with a vengeance. There was no evidence of the character of Mr. KINNEAR's symptoms during life. There was no pathological proof of narcotic poisoning, for every booby knows that narcotics effect no perceptible anatomical changes. There was no chemical evidence of a narcotic poison being found. Now these facts are more than sufficient to show, that death by narcotic poisoning could not be inferred from the medical facts of the case, and that a witness would only be entitled to profess his ignorance of the cause of death. But the presence of the blood afforded evidence of the operation of natural disease, and so far favoured the inference that death depended on natural causes. Yet Mr. Taylor, in defiance of the *only fact* elicited by the dissection, broadly concludes, that the deceased died from laudanum,—because, we presume, he was a most moral, domestic, and religious man, attached to his family and in affluent circumstances,—and because his bell rang two hours before his death, under circumstances totally incompatible with suicide by narcotic agents!

We do not deem it necessary to enter into more detailed comment on this case, nor to offer any further suggestions on the cause of the late Mr. KINNEAR's death, but shall merely remind Messrs. Key, Green, and Taylor, that turgidity of the cerebral vessels is *not* incompatible with gastric hemorrhage, or, in fact, with hemorrhage of any other description. For proof we refer them to the experiments of Dr. Kellie, of Leith, who, as is well known, has bled animals to death, and yet found their cerebral vessels distended with blood. Indeed every slaughter-house in this metropolis exhibits, weekly, hundreds of facts of a similar description.

The jury were "stupid" enough to differ so widely in opinion from Messrs. Key and Green, and the young lecturer on medical jurisprudence, that they stopped Mrs. Kinnear's counsel in the middle of his reply, and returned a verdict against the insurance office. Indeed these gentlemen seem to have perceived, what was we fear somewhat manifest to the whole court, that the medical men whose evidence we have briefly discussed, appeared in the witness-box more like interested advocates of the insurance company than impartial commentators on the facts before them.

We are tempted to append to the preceding, a passage from a leading article, which appeared in the *Times* of Monday last: "We never read any reports where medical evidence is given without blushing for the state of medical science in England, and being convinced that this branch of education is defective, not only as regards the inculcation of sound principles, but even in the application of undoubted facts to recognised principles. Certainly we have yet to lament the want of a "well-governed school of medicine." *The Times* is deceived. "Well-governed schools of medicine" are *not* wanting, but the active operation of a *College of Medicine* is wanting, wherein candidates for the diploma shall be submitted to a public examination by competent scrutators, that examination being made to include those branches of the science in which the incapacity of hospital surgeons and lecturers has been so frequently and so painfully exhibited. If *The Times* felt not the force and extent of the evil ere this, it stands excused, on the plea of ignorance, for having withheld its support from the London College of Medicine; but now, being aware—according to its own acknowledgment—of the defective ordeals which practitioners undergo before they are permitted to practise, or appear in courts of justice, our contemporary cannot, without a culpable—nay, a criminal—abandonment of its duty towards the public, neglect to support a college in which a knowledge of the leading principles and practice of the science will be elicited from the candidates in a public examination, before they can be admitted to the fellowship.

At a meeting of the magistrates for Middlesex, held on the 7th of July, the subject of the Coldbath-Fields Prison epidemic was brought under consideration, and some feeble attempts were made by a few venerable twaddlers to sustain the exaggerated cholera reports they had issued from the prison. We cannot, of course, find room for Sir Peter Laurie's panegyric on Dr. Stevens, but there is one statement made by Mr. Rotch to which we deem it necessary to allude particularly. Mr. Rotch asserted, that the visit of the official inspectors on the 28th June, was *superficially* conducted, and that much incredulity was manifested by Sir David Barry, &c. It is very easy to account for this. Sir David Barry's visit was the *second*, made within twenty-six hours, and but one new patient had been admitted in the interval. That the first visit was made with the utmost patience, will, we believe, be evident by the notes respecting it, which we publish in this day's Lancet, and the accuracy of which is attested by the gentleman who accompanied Sir David Barry on his first inspection.

We are most reluctantly compelled by the conduct of the Middlesex magistrates to return to the unpleasant topic of the Coldbath-Field's Prison "epidemic." We say unpleasant, because up to the time of the announcement of a second irruption of cholera in that establishment, and previous to the publication of the various documents respecting that alleged occurrence, we regarded Dr. Stevens in the most favourable manner. We considered him to be an individual whose mind was too highly and honourably constituted to permit him ever to stoop to the humbug and charlatanry connected with these proceedings. If, however, he will be so deaf to the counsels of his friends, and so insensible to professional opinion, as to countenance the mummeries of Sir Peter Laurie and Mr. Rotch, regarding this *saline practice*, why, on his own head must fall the blame, and he must be content to have his conduct criticised in a tone which may probably be far from agreeable to his feelings.

There is now before the medical profession, we regret to say, a body of evidence of the most palpable character, to prove the real state of the *facts* regarding the sanitary condition of the prison from the 3d to the 29th of June, the period comprised in the following statements of Dr. Stevens in his work on the blood, published on the 3d instant.

"In the first irruption of cholera which occurred in the prison of Coldbath-Fields, the disease was confined entirely to the males. It commenced in the beginning of April, and the last case was dismissed on the 30th of the same month. From this period up to the 3d of June, there were no new cases; but on that day it broke out a second time. In this instance it commenced amongst the females, *and soon spread almost all over the whole establishment, and is now at this moment much more virulent, and I am sorry to add more fatal, than it has ever been at any former period.* In the first case that occurred the woman was attacked on the night of the 3d, and died on the 5th. Her sister who attended her was next taken ill, but recovered under the saline treatment.

"Soon after the commencement of this second irruption, I called at the prison, and there were then four cases. These were under the saline treatment, and as they were all doing well, I did not return. On the 21st of June, however, I received a note from Mr. Wakefield, requesting me to meet him at the prison as soon as possible. When I went there, I found about twenty patients with cholera, and out of this number five were actually dying. *There was one obvious cause for this which I do not feel myself at liberty to point out,—suffice it to say, that it originated from either a mistake or neglect on the part of the nurses who administered the medicines.*

"A saline fluid, similar to that which had been used at Leith, was injected in two cases, into the veins; but the one died almost immediately, and the other though he rallied for a time yet he also ultimately died.

"From the commencement of this second irruption there have been in all about 81 cases: many of these have been of the most malignant description. Out of this number there have been 13 deaths, and the other 68 have either recovered or are now apparently nearly out of danger; but new cases are brought into the infirmary almost every hour. They are all of them however now under the most energetic treatment, and I sincerely trust that the mortality of the disease will be arrested in its progress."

Again, in the appendix,—

"We have now had, during the present irruption in the prison at Coldbath-Fields, 105 cases, and 15 deaths; and there are at this moment in that establishment 22 patients who have recently recovered from a state of complete collapse. But though the proofs in favour of the saline treatment were as numerous as the sands on the sea-shore, still there are individuals who will deny its utility—and that too in a manner which can neither do credit to themselves, nor good to humanity."

From the preceding statement it is obvious that Dr. Stevens wishes three conclusions to be formed by the *public*. First, that 105 decided, unqualified, malignant cases of cholera occurred in the prison from the 3d to the 29th of June; secondly, that the 90 recoveries are solely attributable to the saline practice; and thirdly, that of the deaths which did take place, at least one-third were occasioned, not by the inefficacy of saline remedies, but by the criminal neglect of some person or persons—respecting the nature of which alleged neglect Dr. Stevens preserves an unaccountable silence.

Let us next turn to the official documents elicited by the reports on which these statements are founded, and what do we there find? Sir David Barry and Dr. O'Shaughnessy visit the prison on the 27th June. There are 75 cases of cholera on the books as under actual treatment. Several are reported as new cases admitted that morning; yet not a solitary example of *decided* cholera is to be seen in the sick wards, and Dr Stevens himself admits that the new cases are only affected with the *premonitory* symptoms! We have further the curious fact, that the number reported to the governor is greater than the numbers actually in the hospital. Of those in the cholera hospital we also find a young lad affected with no symptoms of cholera, but labouring under a slight thoracic disease. A second visit is made on the 28th, by Sir David Barry, Mr. Maling, Inspector-General of Hospitals, and Staff-Surgeon Dr. Macann, and now but one case of absolute cholera is to be seen.* The signatures of the gentlemen who made the two different visits, openly and plainly attest these circumstances, and bear testimony to the accuracy of Sir David Barry's memoranda; yet in the teeth of these incontrovertible facts, Dr. Stevens up to this hour has published no further explanation of the affair. He thus abides by his first announcement of "105 cases of cholera successfully treated!" &c., &c. He gives us no tables classifying the patients labouring under "premonitory," or "collapse" symptoms, and he thus practically sanctions the barefaced humbug of the magistrates' circular of the 29th of June, and of their subsequent quarterly sitting of the 7th inst. He allows himself to be cited by poor Sir Peter Laurie as a second Jenner, as an exterminator of pestilence, as a missionary of Providence for the preservation of the public health! And all this fanfaronade is occasioned by the successful treatment of cases which, according to Dr. Stevens' own admission, in conversation, to Sir David Barry, only amounted to the premonitory symptoms of the disease. Now let us mark the attempt apparently made to gloss over the deaths which did *happen* to occur. *Five* of these were occasioned, according to Dr. Stevens, *by the neglect of the nurses!* Statements like this require no comment.

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* The case found on the inspection of the 28th, and which proved fatal on the next day, notwithstanding it had been regularly under the saline treatment.—Ed. L.

We have therefore no hesitation in expressing our conviction, 1st. That the Coldbath-Fields Prison epidemic has been most reprehensibly exaggerated by Dr. Stevens and the magistrates; 2dly. That considering the vast number of "premonitory" cases included in the cholera list, the mortality was greater in this prison than it has generally been elsewhere under the most opposite modes of treatment. During the first irruption of cholera in London, for example, the disease broke out in the Marylebone Infirmary, and 39 children were attacked far more severely than the prisoners of Coldbath-Fields. Dr. Hope treated them with venesection, calomel, &c., and only one died! Again, during the first Paris irruption, of a regiment of dragoons stationed in the district of Petit Musc, near the arsenal, 900 men were attacked with symptoms as violent as Dr. Stevens' prisoner-patients, and *of these* TEN died, about one per cent. The surgeon of the regiment, however, was either too honest, or else too foolish, to return the 900 as cases of decided cholera. He made out a table with different columns, showing how many suffered only from "premonitory" symptoms, how many from "decided though mild" cholera, and how many from the truly dangerous and terrific form of the disease. He farther claimed no credit for arresting premonitory symptoms in 870 of the 900 cases, and he was manly enough to deplore his loss of one-third of the remaining thirty. The conduct of this high-minded officer deserves imitation. There are, however, many persons in the world who will laugh at him for his simplicity in neglecting to send out the trumpeters of the regiment to proclaim in the highways, "900 cases of malignant cholera, successfully treated by a new method!"

After what has now transpired on this subject, we shall not press for the further investigation of the merits of the saline practice, so far at least as concerns the medical treatment of the cases in Coldbath-Fields Prison. At the same time it should be recollected, that the *principle* of the saline practice, and its alleged success in that establishment, are *entirely distinct questions.* The "wholesale" principle, which it appears has been followed in the reports from that quarter, however applicable it may be to *trade*, is not quite adapted to medical usage. It is the duty of the medical press to scrutinize these transactions closely, and comment on them boldly and independently. If on every "breeze of epidemic bane" we are to issue such lists of cases as those lately contributed from the Coldbath-Fields Prison, it is almost impossible to say what mischief may not be effected. So long as quarantine restrictions last, our commerce will thus be at the mercy of every thoughtless or designing empiric, and whilst men continue to be imitative animals, the medical art and public health will thus assuredly suffer, through the trial of fallacious specifics puffed into note by the credulity of doting magistrates, or theoretical physicians. To show how practically these remarks apply, we will undertake, at three days' notice, to produce, for Sir Peter Laurie, fifty cases of *plague*, or a hundred cases of hydrophobia; or, if he be hard to please, we will find him in this warm weather 100,000 of "*sweating sickness*," of the most genuine water. And for all these most incurable disorders—let Sir Peter mark us—we will engage to find him a powerful and never-failing specific, *provided* Sir Peter will engage that he will not scrutinize our reports with more jealousy and vigilance than he has done those from that force-bed of pestilence, the Coldbath-Fields Prison.

ROYAL COLLEGE OF PHYSICIANS

25th June, 1832.

This, the last evening conversation for the season, was tolerably well attended, but not so numerously as some of the early meetings. At four o'clock the Harverian oration was delivered by Dr. Tattershall, which may account for a comparatively thin attendance at the concluding meeting. The Bishop of Chichester, Sir John Leach, Sir John Nicholl, Dean Davies, and many literary and scientific men, were present. Dr. Francis Hawkins, the registrar, read a paper from the pen of Dr. David Uwins, "On the present state and prospects of medicine." The author declared it to be his opinion, that medical practice is much less advanced at the present time than medical *polity*, and that the diminution in the rate of mortality, which is universally acknowledged, is in great measure due to moral improvements in society. Many practitioners, he thinks, are now occupied with the partial views of pathology,—a fault which can only be avoided through the influence of an enlarged and liberal education, without which no one can justly pretend to the character of a complete physician. Since an education, strictly professional, begun at a late period, is sure to cramp and contract the mind, and consequently unfit it for just and comprehensive views, it is an inadequate preparation for the fulfilment of the numerous and diversified states which devolve on the medical practitioner.

Throughout the season the meetings have commanded a numerous and respectable auditory, but the subjects treated on these occasions, and the manner of handling them, show in a glaring light the impudence and vanity of a body which arrogates to its members superiority of attainments, and pretends to give a tone of importance to the medical intelligence of this country.

ST. THOMAS'S HOSPITAL.

CASE OF DISEASED HEART.

Joseph Steadman, æt. 25, was admitted into William's ward, under the care of Dr. Elliotson, on the 10th May, 1832. He had been ill, he stated, twelve months. On admittance he was suffering from cephalalgia and gastritis, together with an affection of the heart. Some time since he had an attack of rheumatism, and has occasionally palpitation of the heart. There is a double bellows sound in the cardiac region, all over the sternum, and extensively to its right and left above and below. The sound elicited by percussion is dull to an unusually great extent in the same region. He lies bent on *the left side*,* but can lie on either. The force of the heart's action is greater than natural.

He was treated antiphlogistically, under which the inflammatory symptoms all subsided, and the heart became much more quiet. The bellows sound remained double, though less loud than previously. He continued well, with the exception of the heart symptoms, till the 28th, when he was attacked by erysipelas of the head, face, shoulders, and back, which left him in a debilitated state, requiring the support of quinine and nutritious diet. He continued to improve up to the 23rd of June, when he was attacked by dyspnœa, which recurred frequently, and produced great distress. All the heart symptoms were increased in severity, and a crepitous rattle in the chest gave evidence of the existence of peripneumonia. These could not be removed by treatment, and he died on the 28th.

Diagnosis.—The following was the diagnosis of the disease of the heart given by Dr. Elliotson on his admission. That there existed hypertrophy of the left ventricle, with disease of the sigmoid valves of the aorta, producing an obstruction to the passage of the blood from the left ventricle into the aorta, and giving rise to the first bellows sound. That the disease of the valves was such as to incapacitate them for the performance of their duty, and consequently they allowed the passage of some of the blood back again into the left ventricle, producing the second bellows sound.

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* An exception to a general rule in cases of this description.—Rep.

Autopsy.—It will be seen that this diagnosis was beautifully verified by the post-mortem appearances. The heart was found considerably enlarged on the left side, and adherent at one spot to the pericardium. On opening the left ventricle every part within was seen to be much larger than natural. The wall of the ventricle was not much, if any, thicker than it should have been, but the cavity was much enlarged, rendering it evident that there must have been a great addition of substance. On passing the finger through the aorta from above downwards into the ventricle, the commencement of the aorta was found constricted. The aortic valves were felt to be diseased, and in touch resembled whip-cord. On splitting the aorta open it was very obvious that the disease was such as to prevent the valves doing their duty. They were very much corrugated, thickened, and inverted, rendering them nearly useless, and from the edge of one there was a warty excrescence about three-eighths of an inch in length, hanging down towards the ventricle. The mitral valve was also diseased, but the situation of the disease was such as neither to impede its function, nor give any stethoscopic sign.

⁂ At page 486 will be found a renewal of Dr. Elliotson's admirable Clinical Lectures, which an overwhelming pressure of matter has prevented us from before concluding. We have three or four more, only, on hand, to complete the course, which we shall insert forthwith. In giving precedence to other matter during the last two months, we may observe, that it would be difficult to select any portion of the usual contents of a medical journal, the value and interest attached to which would more readily admit of delay in their publication than the clinical remarks of Dr. Elliotson. Their worth is of the most permanent kind. We refer to the lectures in this place, however, for the purpose of adding to the one on epilepsy in our present number, the following note which was accidentally omitted from its proper place.

There was a very curious variety of epilepsy in Dr. Elliotson's female ward, at the time of the delivery of the lecture, published this week. In this case after the convulsions were over, *but while the unconsciousness still remained,* the woman began to *hum a tune and dance,* pointing her toes and bending her wrists with what the nurses and patients considered, considerable grace. The Doctor remarked on this case, on going through the ward one day, that he knew perfectly that there was no doubt in the matter, and that such instances were to be found in medical records, and had been seen before by himself. This patient is still in the hospital, and continues to sing and dance, unconsciously, in the fit, as before.

WESTMINSTER HOSPITAL.

ANEURISM OF THE DESCENDING AORTA.—DEATH.—POST-MORTEM APPEARANCES.

Patrick Driscol, ætat. 35, a man well set, of middle stature, and of healthy appearance, was admitted under Dr. Bright on the 12th of October last, with symptoms of dyspepsia. He was a seaman, but had been roving on land for the previous six months, and had indulged in every conceivable excess, but especially in drunkenness. He first complained of obstinate constipation, after this a tumefaction occurred in the left iliac region, and which was removed by purgatives, but it occasionally returned accompanied with pain. The appetite fell off, and he became subject to flatulence. When received into the hospital, the countenance indicated derangement of stomach. Tongue broad, flabby, and furred; appetite bad, flatulence; bowels constipated; pulse 78, full, soft, and *regular*; the other emunctories healthful in their function. A stomachic mixture of *senna, gentian, and carbonate of soda,* in due proportion prescribed.

17. The medicine produced little or no good effect. The man complained of nausea, the bowels became bound, and the tongue more deeply furred, and severe pain came on in the lateral regions of the abdomen. *Eight ounces of blood to be drawn from the side by cupping. An injection of turpentine and castor oil to be given.*

19. Pain diminished, bowels relieved by the injection; bladder irritable; frequent micturition; skin dry; pulse 84, full and soft. *A calomel pill to be swallowed at night, and a black dose in the morning.*

24. The symptoms recited continued with slight variations, and were mitigated by the same means as we have described. This morning acute pain was felt under the left breast, increased on each inspiration; breathing accelerated, and attended with a short cough; pulse 120, full and hard; countenance anxious. *Venesection to ten ounces, a blister to the side, and a diaphoretic draught every four hours.*

25. Anxiety of expression diminished; pain somewhat alleviated, but still consi-

derable; cough painful; respiration rapid. The heart beats with great force, and a *grating sound;* pulse 104, full, strong, and *irregular*; tongue pallid and clean; bowels open; other functions duly performed. *Two grains of calomel to be given every four hours.*

29. Pain in the region of the heart lessened. Vertigo on exertion; pulse 108, full and *jerking;* incipient ptyalism; bowels freely open.

Nov. 3. Had a severe fit of syncope whilst on the stool, for which he had a dose of sal volatile administered. Pulse 114, feeble; tongue exsanguous; skin cold; face and general surface pallid; pain in the lower part of abdomen. He moans continually. *To discontinue the calomel; to take ten grains of Dover's powder every night, and to have a blister on the left lumbar region.*

Nov. 5. He has stood erect to-day without fainting; pain still remains in the left iliac fossa; pulse 108, feeble; circulation more active in the capillaries.

8. On attempting to sit on the close-stool, a syncope occurred, which lasted for more than half an hour; the collapse was very considerable; no pulse perceptible on the radial artery during the paroxysm. *A wine-glassful of brandy and the following mixture*:—

Laudanum, a drachm;
Spirit of sweet nitre, an ounce;
Camphor julep, six ounces. Mix; an ounce every four hours.

12. The opium has produced a slight jaundice; the pain continues in the iliac fossa. The patient has the greatest dread of moving; no persuasion can induce him to sit up. He restrains all his natural wants, and crouches at the bottom of his bed; pulse 110; tongue cleaner; *continues the remedies.*

20. Another severe fit of syncope; takes *ether and laudanum.*

25. Feels stronger, but has pain of chest on inspiration; tongue flabby and bloodless; his urine deposits a vermilion-like sediment; pulse 100, full; respiration 22 per minute as he sleeps. The fear of moving still haunts him. *A pill of sulphate of iron and opium every four hours.*

28. Has rallied considerably; is in no pain; countenance improved; seldom adventures to assume an upright posture; cannot lie on right side; respiration 24; on right side murmur very distinct, louder than natural. Percussion gives a hollow sound, and the resonance of the voice is remarkably distinct. On the left side no respiratory murmur is audible; percussion yields the *son mat*, but little resonance of voice; pulse 120, full; appetite improved. *Continues remedies; occasional cathartic given.*

Dec. 6. Continued in *statu quo* since last notation, and without syncope. This morning at two o'clock the fainting fit occurred, which was relieved for a time by an antispasmodic draught. He was never completely roused. At one p.m. the pulse was not perceptible; he was ghastly; the respiration suspicious; deglutition gone. He expired at two p.m.

Examination of the Corpse thirty-six hours after death.

The body not emaciated, but the surface was pale, and the muscles were remarkably exsanguous when divided. On the left side the chest was dilated, and the parietes of the abdomen of a dark colour. On cutting through the costal cartilages of the left side, the pleural bag was wounded, and a quantity of blood flowed out. This was collected in a sponge, and the thorax exposed in the usual way. The left thoracic cavity was completely filled with blood, and the lung quite deprived of air, and pressed against the mediastinum. On the right side all was well, and the lung natural. A small quantity of fluid was found in the pericardium, and the heart was pale, its substance softened, and its parietes thin; the right ventricle being scarcely thicker than a healthy auricle. The heart itself was rather small, contracted, and contained no blood. After removing about three pints of coagulated blood, no solution of continuity could be discovered in the thoracic aorta or its branches. The pleura was thickened, and layers of lymph were deposited on its surface, and were gradually lost in the clotted mass of blood which was incumbent on them.

The first unusual appearance in the abdomen was the course of the colon from its hepatic fold, obliquely downwards to the pelvis, till it was in contact with the bladder. It then mounted as high as the spleen, thus forming a loop, and descended normally on the left side. On turning aside the stomach and intestines, a large dark-coloured tumour was discovered under the peritoneum, extending the whole length of the mesentery. On incising the peritoneum, it was found much strengthened by subimposed layers of cellular membrane. The spleen and adjacent kidney were *ramollies* and discoloured. The tumour being opened, yielded a quart of coagulated blood, and in its centre was discovered a sac sufficiently spacious to contain the hemisphere of an adult brain. This was the aneurismal bag, and was fortified by strata of coagulated lymph. On examining the abdominal aorta, an aneurismal pouch as large as a kidney was discovered on the right side, projecting from near the origin of the cœliac axis. This was found to be nearly an inch thick, and protected by laminæ of old fibrine. On slitting up the aorta, it was discovered to communicate with three large cysts; the one first descried, the one last mentioned, and another rather to the left side and lower. The anterior sides of these cysts were formed by layers of coagulum, impressed on the cellular tissue of the abdomen. On the posterior aspect were the vertebræ of the dorsal and lumbar class in different degrees of absorption, the intervertebral substances being scarcely affected at all. The cyst, the rupture of which was the immediate cause of death, was of immense size. It was bounded above by the whole of the left pleura; passing under the diaphragm it had denuded the ribs, the intercostal muscles, ligamentum arcinatum, and quadratus lumborum. The psoas magnus was flattened, and its substance softened. The sides of the sac were lined with blood in different stages of coagulation. A laceration in the pleura, admitting the escape of blood from the aneurismal cyst into the thoracic chamber, was apparently the immediate cause of the patient's destruction.

The viscera were moderately healthy. The head was not examined.

It is remarkable in this case, that no unnatural pulsation existed over any part of the abdomen. Could this be due to the great thickness of the cysts, and their being extra-vascular? This supposition would appear countenanced in some degree by the occasional deliquium, which was most probably produced by the escape of a quantity of blood into the cellular tissue. This blood remaining some time would become coagulated, and thus remain passive. The area of the aorta was not obstructed, and the circulation was enabled to proceed, as long as the resistance of the coagula was sufficiently strong to afford plugs to the lateral fissures of the artery. When the patient was perpendicular, the weight of the column of blood was too great to be thus confined, and a quantity escaped, as was evinced by the fainting. The principal sac was so enlarged by these successive influxes, that it projected considerably into the chamber proper for the lung, and much straitened the function of that organ.

THROAT CUT IN A FIT OF NERVOUS IRRITATION.—RECOVERY.

George Slough, 43 years old, admitted on the evening of 23rd June, under Mr. Lynn. He was always a healthy man until about two years ago when at Brighton, following his employment of painter and glazier, he fell from a height on his head, and suffered concussion of the brain. He was a long while in the Sussex hospital, and was ultimately discharged cured. He has, however, been ever since subject to occasional attacks of vertigo and epilepsy, and frequent fits of depression of spirits. His life has always been regular, and his diet abstemious. During one of his melancholic paroxysms he drew a razor across his throat. A branch of the superior thyroideal artery was divided, and some hemorrhage took place. When brought to hospital, it was found the man had made a deep incision between the os hyoides and thyroid cartilages. The ligaments and muscles between the two had been cut, and the finger could be passed into the wound, as far as the mucous lining of the larynx, which, when touched, produced a sense of suffocation. Deglutition was performed without difficulty, but the patient spoke with reluctance, on account of the alarm it excited in his mind. The wound was covered with a large coagulum, but this being carefully removed, and the surface washed with warm water, the bleeding ceased. *Three* sutures were made by Mr. Gill, the house-surgeon, and the patient composed himself to sleep.

July 5. The ill consequences of the wound have been prevented by an antiphlogistic and soothing treatment. Pultaceous food has exclusively been allowed to him; his mind is tranquil, and the wound is healing with very little suppuration.

SARCOMATOUS TUMOUR OF THE NECK.

Frederick Benyon, aged 54, a man of middle stature, and tolerably good constitution, who has spent part of his life as a seaman, and has been generally occupied on land as a light porter, enjoyed a fair share of health for the greater part of his life, having never suffered from any disease except a bloody flux whilst in the West Indies. He has been a married man, and never addicted to irregular pleasures, and has, consequently, been perfectly exempt from their usual attendant affections. On the 18th of January last, this man was admitted under Mr. White with a tumour on the left side of the neck and throat, apparently of a mild sarcomatous kind, and extending from the lobules of the ear down to the middle of the clavicle, and from the right sterno-cleido mastoideus muscle across the throat and left side of the neck over the nape, as far as the external edge of the cervical part of the right trapezius muscle, thus including three-fourths of the circumference of the neck. The greater part of the surface of this tumour is an open ulcer covered with florid, large, and flabby granulations, and bedewed with a laudable pus. Its margins are hard, livid, and projecting, and the whole tumour is immoveable, being apparently amalgamated with the substance of the adjoining tissues. It gives no pain or inconvenience, and is not tender to the touch. Since the setting in of the hot weather, the discharge has increased, and amounts now (2nd of July) to about four ounces daily. The patient's general health is not much impaired. He swallows, digests, and sleeps, well. The bowels are regularly open. Pulse 90, strong, and full.

In September, 1831, whilst shaving, he perceived a tumour and stiffness of motion *on the left side of the neck.* This was about the size of a marble; it was not painful. It grew rapidly, and having attained the size of a hen's egg, broke in November, and discharged a quantity of blood and pus. Poultices were applied, according to the order of the surgeon at the Charles Street Dispensary, but a fungous growth sprung out of the ulcer, and could not be restrained by astringents. On attaining the magnitude described, he applied, and was admitted into this hospital.

Since admission, the object of his treatment has been to sustain his health, and produce the absorption of the excrescence. Iodine has been exhibited in gradually-augmented doses, his diet has been full, and lunar-caustic washes and regulated bandaging have been resorted to. It is imagined the size of the tumour has diminished, but there is a difference of opinion on this point. He still continues to eat his allowance, and his spirits do not flag, but his colour is fading, and other harbingers of deteriorated health are every day becoming manifest.

ANEURISM OF POPLITEAL ARTERY—TREATMENT BY COMPRESSION.

Richard Bigland, ætat. 30, a tall well-formed, muscular man, of ostensibly sound constitution, was admitted June 25, 1832, with popliteal aneurism. He is a gardener, and having had pretty constant employment, he has lived well, and has never been given immoderately to drinking or venery. He had the venereal disease but once, and that mildly. His health was uninterrupted, except in one instance, many years ago, when he was attacked with brain fever. About eighteen months since he was troubled with a pain in the ham, extending upwards and into the calf of the leg, whenever he made any unusual exertion. He continued at his work, however, unsuspicious of any harm, and without any very great aggravation of his symptoms. He became aware of a greater *beating* in the affected limb than in the other, and which frequently interrupted his employment, and at last compelled him to desist altogether. About a month ago he applied to Mr. Lynn, in Parliament Street, who discovered an aneurismal tumour in the popliteal space of left leg, and had him immediately brought into the hospital. During the formation of the disease, his health was not at all affected. When admitted, the pulsation over the entire surface of the tumour was distinct; pressure on the common femoral annulled the pulsation, and lessened the size of the tumour. Pressure on the part itself also produced the same effect.

His colour is good. Pulse 80, regular, steady, soft. Action of heart in all respects natural; respiration perfectly healthful; appetite and digestion good; tongue wide and clean; bowels naturally open. He is placed on middle diet. He takes no medicine but salts and senna on occasions.

Mr. Lynn has determined to adopt the Italian plan of pressure on the superficial femoral, and this day the system is commenced. The leg and foot are bandaged up to the knee, a tight roller applied to the thigh, and over that an ordinary tourniquet, with which gradual pressure is to be made. He has had a slight degree of pressure for about six hours, and feels considerable numbness in the inferior part of the limb. The pulsation is however quite perceptible, though less strong in the tumour. The circulation generally is not as yet accelerated.

LONDON HOSPITAL.

CASE OF RECTO-VAGINAL FISTULA.

On Wednesday, 13th June, Mr. Scott operated on B. C., ætat. 30, an Irishwoman, for a fistulous communication between the rectum and vagina, which has been existing for about ten weeks.

This patient, who does not give the most intelligent account of the manner in which the accident occurred, states, that in her first labour, which took place about ten weeks since, during the passage of the child's head in the vagina during a very severe pain, the midwife who attended her, thinking to hasten the passage of the child, passed her hand up the vagina, at which time she felt something give way in the vagina.

On examination a fistulous communication was discovered from the rectum to the vagina, by which a considerable portion of the fæces pass through this fistulous opening into the vagina, thus rendering the woman an object of misery to herself. She applied at this hospital for relief, being willing to submit to an operation, if necessary, and was accordingly admitted on the 24th May, under the care of Mr. Scott, for the purpose of operation.

The bowels having been previously freely evacuated, Mr. Scott commenced by pulling down the vagina by a double hook, then paring off the cartilaginous edges, with a probe-pointed bistoury, afterwards bringing the raw edges together, and retaining them in contact by means of three sutures.

The following night she rested well, and passed her urine three times. The nurse was strictly enjoined, not on any account to allow her to have any solid food, but to confine her to slops, as beef tea, tea and milk, so that the parts might not be disturbed by her motions.

15. Was very unwell during the night; fainted twice, but for which no reason could be assigned. She recovered on the admission of fresh air. In every respect as yesterday.

17. In every respect as on the 15th; has not had any motion since the operation. Was ordered by her dresser a dose of castor oil, which has operated, and a portion of the fæces passed off by the vagina.

18. On being visited by Mr. Scott, he found she had taken the castor oil, and, in a very mild manner, mentioned his regret to the dresser for his inconsiderateness, having endangered the success of the operation by exciting the bowels, which he, of course, wished to be kept in a state of perfect rest.

26. On Mr. Scott making an examination this day, found the operation had in a great measure succeeded, the edges having united throughout nearly the whole extent, there is, however, a small spot which has not united, through which a portion of feculent matter passes when she has a motion, but which, he hopes, will be filled up by granulations.

FRACTURED THIGH.

— Haydon, ætat. 71, was admitted 7th June, with fracture of the right thigh through the trochanter. Considerable difficulty was experienced in the treatment of this case from the restlessness of the patient, who appears to be almost in a state of dotage. The straight position, double inclined plane, a round pillow, were each tried in succession without benefit; there being, with all those means, considerable shortening of the limb; at last the splint invented by Mr. Masset (we believe) was tried, which appears to answer extremely well. The limb is now of precisely the same length as the sound one; lies in a perfectly natural position, and we have reason to hope this man will ultimately leave the hospital with a useful limb.

AMPUTATION.

June 27th. Mr. Luke amputated the thigh of J. W., ætat. 17, for disease of the knee-joint.

This lad states, the disease has existed for about three years, but he is unable to assign any reason for its formation, never having, to his knowledge, received any injury to the knee. About two years since he was a patient in Guy's Hospital, where he remained for six months for this disease, and for which leeches, cupping, and blisters were employed, but without receiving any benefit. He has since been under the care of several practitioners, but who declared they could be of no assistance to him, except by removing the limb, and recommended his getting into this hospital for the purpose of having amputation performed.

At the period of his admission the limb was semiflexed; the joint in a state of perfect anchylosis, without any motion in it, with a patch of ulceration on the inner side. It began to ulcerate about six months since; he has not suffered any pain in the limb for some months past.

Mr. Luke performed the operation by the double flap and thrusting a catlin through close to the bone on the outer part of the thigh, and cutting obliquely out, then dividing the muscles on the inner side obliquely upwards, terminating at the point where he had first thrust in the catlin. Twelve ligatures were applied, the flaps were brought together, and retained by four sutures and straps of adhesive plaster and a roller.

On being placed in bed, twenty-five drops of the tinct. opii were given him.

On visiting him the following morning, we found he had passed a very comfortable night, having had several hours' refreshing sleep, and felt quite easy; spirits good; has no pain in the stump; tongue clean; pulse 84; skin cool; bowels opened yesterday.

29. Slept very well; pulse 78; skin cool; tongue clean; free from pain, feels quite comfortable; bowels have not been opened since the operation.

30. In every respect as yesterday. This morning the bowels were comfortably relieved.

July 3. The stump was dressed to-day, and looks very well. No suppuration has as yet taken place; his bowels are open; tongue clean; sleeps well; pulse quiet. In every respect doing well.

The limb was not examined.

AMPUTATION OF THE LEG AFTER FRACTURE.

(*Concluded from page* 317.)

May 30. T. S. complains of great exhaustion, having passed an extremely restless night; pulse quick and small; skin hot; tongue furred; countenance indicative of anxiety; has no appetite; bowels confined; stump extremely painful, and discharge very considerable.

June 4th. Extremely weak and low; stump very painful and hot, particularly on the inner side; has very little appetite, pulse small and quick, about 130; the discharge still continues considerable; has profuse night sweats; the stump was ordered to be buried in a linseed-meal poultice.

6. Stump less painful than on the 4th; discharge still very considerable, and to-day mixed with blood; his appetite has returned, and he now eats very heartily; pulse about 130, and weak; the night sweats profuse; has a trifling cough; and when he coughs the expectoration is purulent. To continue the poultice.

8. Has very little pain in the stump, which discharges daily a considerable quantity of purulent matter. The night sweats continue profuse, and he complains of a quantity of water continually running from his mouth; pulse still extremely quick. He was ordered to have eight minims of dilute sulphuric acid added to each dose of his mixture, with a view of checking the night sweats, and to take a dose of castor oil immediately, as his bowels have not been relieved these two days. This gave him one copious evacuation. To continue the poultice and the anodyne at night.

The stump has been poulticed since the above report was drawn up. The discharge is materially lessened, and the man's general health much improved; the night sweats are now inconsiderable; pulse increased in strength and reduced in number; bowels open; tongue clean; appetite good. He sits up daily; and we have reason to expect a favourable termination.

THE LATE MR. JOHN WOOD.

(*From a Correspondent.*)

The premature death of this promising surgeon, who fell a victim to cholera on the 9th inst., after a few hours' illness, while it is an overwhelming affliction to his family and friends, must be regarded as a loss to the profession and public. After receiving a good general education, Mr. Wood began to study medicine at St. Bartholomew's, as the articled pupil of his relative, Mr. Lawrence. He then visited the medical schools of France and Germany, and subsequently filled the situation of House-Surgeon for a twelvemonth. From the commencement of his studies he displayed superiority of talent, indefatigable industry, and an ardent desire for knowledge. His greatest pleasure was in the performance of his duties, observation of disease at the bed-side, study of the best writers, and reflection on the materials derived from those sources. These valuable endowments were adorned by a most amiable private character, by a benevolent and humane disposition, honourable principles, and the most exemplary conduct in the relations of life.

As this case affords an awful lesson of the severity and intractable nature which this frightful malady sometimes exhibits, we shall communicate a few particulars derived from an authentic source.

Mr. Wood was regular and temperate in his habits, and accustomed to take regular and strong exercise, particularly in walking. He slept at the house of a relation several miles from London on the 7th, rose early on the 8th, and took a hearty breakfast. Between two and three o'clock he visited, with Mr. Lawrence, the prison of Bridewell, in which the cholera had suddenly broken out, and again attended the patients in the evening. He was called to the prison two or three times in the night, so that he got little or no rest. When he met Mr. Lawrence next morning (the 9th) at Bridewell, before breakfast, he looked pale, and said he had been a little purged, which he attributed to having eaten gooseberry-fool for supper on the 7th. Mr. Lawrence advised him to take a little opium with chalk to check the purging, but he entertained no suspicion of cholera. Mr. Wood went home to breakfast, and took an ounce of castor oil. He continued in conversation with two friends; the purging increased, but he felt so little alarm that he was with difficulty prevailed on to go to bed between eleven and twelve o'clock. One of his friends perceiving a rapid alteration in his countenance, sent for Mr. Lawrence, who saw him about one o'clock. Dr. Tweedie was also requested to visit Mr. Wood soon after. The disease had at that time assumed decided characters of the collapsed stage of cholera, and impressed them with the alarming progress it had made. The measures pursued were ineffectual in producing reaction; the vital powers continued to sink, the countenance and extremities became blue, the voice peculiarly characteristic, the spasms incessant, and he died at ten o'clock in the evening.

CORRESPONDENTS, &c.

Our apologies are due to *Dr. S. Kinnear*, for the non-insertion of his letter, dated 26th June. It was mislaid or it should have appeared. On recovering the article, we observe so many observations which were of interest temporary only to the period at which it was written, that we do not now publish it. His predictions, however, bid fair, we think, to be fulfilled in a very melancholy manner.

THE LANCET.

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CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE

IN THE UNIVERSITY OF LONDON,

Delivered April 2, 1832.

Most of the cases, Gentlemen, that were presented or have died since the last lecture, afford considerable matter of interest, but I must pass over some that are curious on account of one of great interest.

PORRIGO LUPINOSA.

The first case to which I will allude was that of Elizabeth Roach, who was admitted for porrigo lupinosa—that kind of porrigo affecting the head, in which there are numerous granules, so that the appearance is that of lupin seeds, hard granules, dry, and, I believe, containing earth. I understand that a considerable portion of lime has been found in them. In this form of the disease there is generally no inflammation, and stimulants are therefore applicable, and are found very useful. Nothing can be worse than to apply stimulants indiscriminately to affections of the skin, for the skin is occasionally, in the same disease, in a very different condition. Sometimes there is no inflammation; sometimes, if there be no inflammation, it is excessively irritable; and sometimes it is in a languid condition, so that stimulants are absolutely required.

Treatment.—In this case, which occurred in a girl ætat. 16, and had existed twelve months, I ordered the head to be shaved, and what is often very useful in this disease, two grains of calomel, which she took night and morning, until her mouth became tender. The stimulant I applied was ung. picis, and in the particular state of her head, there being an entire freedom from irritation, it was productive of very great benefit for a certain time. All these crusts came off, and the head looked comparatively smooth, but the application at last, having done all the good it could, was beginning to do harm. The head became hot, and an inflammatory state came on. If I had continued tar ointment under these circumstances, I should have made bad worse, there being a copious discharge of pus, and the whole of the head being red and heated. I therefore omitted it altogether, and ordered simple cold water to be applied. The mouth had become sore from the calomel, but that presented no objection to the cold water, provided it was applied only where the parts were excessively hot. This method speedily brought down the inflammatory state, and the patient was so much better that she went out. The case was not cured, and it will require constant attention for many months, and after all perhaps the disease will not be entirely eradicated. It subsided however by stimulating treatment in the first instance, and sedative treatment, if I may so call it, in the second; and it is very possible that stimulating treatment will, after a time, be again necessary.

ECZEMA.

Another case was presented in the same ward, of disease of the head, which would be called *porrigo* by Willan and Bateman; but I think there is no doubt that many diseases of the scalp, called by Willan and Bateman porrigo, are really other diseases. I hardly ventured to say so till I heard that a French physician, Biette, was convinced of the same thing. Many cutaneous diseases affect the head as well as porrigo, which have no contagion in them, and many of them have improperly been called porrigo by Willan and Bateman. This appeared to be a case of eczema of the head.

There was a considerable discharge of thin watery fluid, which concreted into fine minute crusts. I frequently found some on the head, exactly of the appearance of eczema in other parts of the body.

Treatment.—It was necessary here to conduct the treatment on antiphlogistic principles. She had had the disease four years. Leeches were applied every other day behind the ears, and cold water, in addition to which she took five grains of hydrarg. c. creta twice a day. Accidental inflammation took place—a degree of inflammatory fever, and there were signs of inflammation in the head, and inflammation of the stomach and intestines. There was inflammation of the mucous membrane of the intestines, inducing diarrhœa on the one hand, and of the head inducing delirium on the other. This was subdued in the common way by leeches and blisters applied to various parts, and then the head was attended to again, and similar treatment adopted as at first; but at last she became free from all excitement, the scalp fell into a languid state, and the tar ointment was applied with great benefit, so that I was able to present her likewise on Thursday last.

You will find it of the greatest importance to vary the treatment in these local chronic diseases of the skin, as well as in those much more diffused throughout the body, accordingly as there is inflammation or great irritation, or neither—for the applications which would be right in one instance would be wrong in another.

JAUNDICE.

There was a man presented who came in with jaundice, and the case was instructive, as showing that jaundice may be a mere inflammatory disease,—nothing more than an accidental symptom of hepatitis. This man's chief apparent complaint was the yellowness of his skin, the yellowness of the conjunctiva, and the yellowness of the roots of the nails. He said he had been ill three months, but particularly ill only during the last six weeks. Of course the motions were rather white, and the urine the colour of porter, showing a yellow tinge if you bent the vessel down a little so as to see through a thin portion of fluid. But on examining the right hypochondrium, I found considerable tenderness, proving that the case was clearly one of hepatitis, inflammation of the liver, and I regarded it simply as such.

Treatment.—He was treated by cupping the epigastrium to twelve ounces, by giving him ten grains of blue pill twice a day, and putting him on low diet. By this means the tenderness all went from his side, and with it went away the jaundice. You will find continually that jaundice is nothing more than a symptom of hepatitis. You may have hepatitis without jaundice, and I presume it is from the accidental circumstance of the inflammation affecting the parts where the ducts are, that you sometimes have jaundice in hepatitis. But it is worth knowing that jaundice is often an acute or sub-acute affection, and yields to the common treatment of inflammation.

GASTRODYNIA.

There was a girl presented who came in on account of gastrodynia, and as it is a common affection it may be worth while to say a few words on the case. She was twenty-three years of age, and had been ill she said a twelvemonth.

The symptoms were—pain at the pit of the stomach over the epigastrium, coming frequently with great violence, and darting through to the back, and occasionally she discharged a watery fluid into the mouth. It was a case of *gastrodynia* and *pyrosis*; sometimes there was a discharge of fluid, and sometimes none.

Treatment.—One of the best medicines for this affection, when there is no inflammation, is hydrocyanic acid. This sort of case is sometimes very obstinate; it is very common in young females, and sometimes is so obstinate, so violent, that it has been supposed indicative of organic disease, but afterwards they have recovered perfectly. Of course hydrocyanic acid is not a specific for it; there are cases in which it will fail, as other things fail in every disease. She began with two minims three times a day, and it was increased to what is a very considerable dose, and one which you will not often be able to reach. It was gradually increased, from the 6th of February to the 19th, up to eight minims and a half for a dose. This must be considered a very large dose, but it was given with perfect safety, because after we had arrived at seven minims it was increased only half a minim at a time. However, this medicine did not do her good till we had arrived at this large dose, and when we arrived at it, from the long continuance of the pain, there was more or less inflammatory state of the stomach, and it was in vain to think of subduing that by hydrocyanic acid or purgatives. It is necessary under such circumstances to take away blood from the arm or the region of the stomach, and in the latter case leeches are the most eligible mode, the parts being too tender to bear cupping-glasses. Leeches were applied to this patient several times, and we continued the hydrocyanic acid. She had no tenderness when she came in, and when it came on at last there was no inconvenience from the medicine. While however I mention this dose, it is right I should tell you that I have known a patient who would not bear at first more than a fourteenth of a minim for a dose, but who was cured of a complaint with doses not much increased beyond that.

ABSCESS OF THE LABIUM.

A woman came in the course of the week who was represented as very bad,—as in a dangerous state; but on examining her it turned out that there was nothing but an abscess of one of the labia. She said she was a new married young woman, and her complaint had suddenly begun at six o'clock in the morning. There was a quantity of matter, and she said that her bowels were confined. We could not feel fluctuation satisfactorily, but it was evident there was an abscess of one labium; and on being poulticed, it broke, and the case terminated.

HYPERTROPHY OF THE LEFT VENTRICLE AND AURICLE.

There was likewise a woman presented with hypertrophy of the left ventricle. The symptoms were, great irregularity of the pulse, both in force and frequency, difficulty in lying on the left side, palpitation, and when the heart gave the strongest strokes, I may mention, that the stroke was dreadfully violent, that the heart thumped against the side, and raised the stethoscope. There could be no doubt of the left ventricle being hypertrophied. These were her symptoms, together with difficulty of breathing and œdema of the legs. There appeared to be no disease of the openings of the heart, for there was no preternatural sound.

Treatment.—She received the greatest possible relief, though a cure is out of the question, by a combination of ten minims of tinct. digitalis; the same quantity of tincture of squills, one drachm of nitric ether, and half a drachm of acetas potassæ, every eight hours. I have mentioned several times in the course of the winter, that a medicine of this description often effects the greatest relief, although it will not succeed in curing the complaint.

SCIRRHUS UTERI—MALIGNANT CHOLERA.

I now proceed to the case of the woman from whom the parts now before me were taken.

A woman, ætat. 44, married, but who had never had any children, was admitted on the 22d of March, whose catamenia had ceased for five years. She complained of pain in the loins and lower extremities and hips, and general feverishness. She had also discharge from the vagina of a yellow colour and a very offensive odour. This had continued six months. I examined her carefully by the vagina, and found the os uteri sound, nor could I discover disease of the neck of the womb. From the pain in the loins and hips, and this discharge, there was every fear of uterine disease, but I did not feel justified in saying there was disease of the womb; for both the os uteri and neck were sound, so far as I could judge. I would not say there was no disease of the womb, but only stated there was none to be felt. You have no right in any case to say, that a person has not organic disease, for it is astonishing what organic disease may be found after death, when there was no ground for asserting it before death. You may believe it, but you cannot assert it. One has seen a large portion of the brain softened from an abscess without any disturbance of the mental faculties before death, and without paralysis, and such a person has died suddenly. We can hardly say that any of us here are free from organic disease, though I hope we are; yet organic disease may exist to a certain extent, frequently without producing symptoms. This woman had great structural disease of the womb, but unless you can feel disease of the os uteri, or can feel the womb enlarged, either by the vagina or the rectum, you cannot say that such is the case. This woman had likewise had a cough for six months, and was evidently spitting pus; she was likewise deranged, wrong in the head, was frequently incoherent, evidently labouring under a degree of mental alienation.

Now it so happened, that on Friday last, when I was going round, I observed the woman's countenance to be altered. There was the slightest possible suffusion about the eyes, and an expression of the face which I cannot describe. There was a weakness, a tremulousness, and a very slight hoarseness in the voice, which appeared to me exactly like the cholera voice. On feeling her pulse, I found her hands cold, and the pulse exceedingly feeble. These things struck me very much, and I inquired if there had been any vomiting or purging. She declared she had been purged all night, every ten minutes, but the nurse declared there had been no purging at all, and I could not learn whether there had been vomiting or purging. The symptoms were so suspicious, that though I would not pronounce it cholera, yet I believed it was. Just as in all probability there was disease of the womb, though I could not prove it, nor could I think of asserting it, so I would not assert there was cholera, but I had great fear about it, and considered it a matter of prudence to remove her from the ward. She was removed, and died, I understand, at eight o'clock the following morning. It appeared from all I could learn that she had been a dram-drinker; she had the appearance of it when she came in, and her breath had that particular smell which a great number of dram-drinkers have. She said she was very weak at her admission, and half starved; and sometime afterwards she said she had had nothing to eat since she came to the hospital; however, she was allowed a pint of porter a day, and meat diet. The account I have given you from memory, accords exactly with that which appears, I see, in the entry book.

About two hours after her admission into the cholera hospital, she had a fluid stool containing some bile. She did not vomit, nor was she affected with cramps, neither was there blueness of skin. At seven o'clock great difficulty of breathing came on, and she gradually sank and expired at eight in the morning.

Post-mortem Appearances.—On opening the head the dura mater appeared more opaque and thick than usual, and the outer surface of it was in a state of congestion. The arachnoid was very opaque, and had on its surface a flaky deposit. This is the dura mater very much thickened, and there is great congestion of blood on the inner surface. This is not at all to be wondered at, as she was wrong in her head. You very often after insanity discover no disease in the brain itself, but disease of the membranes; not that the disease of the membranes has been the immediate cause of the insanity, for they are not the organ of mind, but the disease in the brain has extended to the membrane and produced visible marks of disease there, though they have not appeared in the brain. The disease of the membranes had disturbed the function of the brain by vicinity. The brain was unusually firm, and litmus paper placed on the surface of a section was reddened. At the base there was considerable effusion, probably one ounce and a half. I mentioned that she was spitting pus when she came in, but she would not allow me to use the stethoscope carefully. She was a restless woman, and being a little cracked, she would not allow me to keep the stethoscope steady upon her for a moment. However, the substance of the lungs was almost completely disorganised, and filled with tubercles, and containing many cavities. The heart was healthy.

The uterus was greatly diseased—affected with cancer. These parts of the womb are in a state of cancerous ulceration. Here is scirrhus and open cancer by its side, very offensive, as you will perceive. You see the fundus of the uterus irregular, having a scirrhous deposit. The walls of the uterus are much thicker than they should be at the fundus; they are a little thicker lower down, but at the fundus you see the great thickness. The fundus, too, is ulcerated; that part of the walls of the uterus which forms the upper part of the cavity of the womb; and what is very rare indeed, here is the os uteri and the cervix, as I mentioned, not at all contracted or thickened. This must be considered a rare specimen of disease of the womb, for, in most cases, disease of this organ, whether it be cancer or malignant ulceration, begins at the neck, and that part, or os uteri, ulcerates, while the fundus may be sound, and, as a general rule, the ulceration proceeds more rapidly at the neck and the os uteri than anywhere else, that part being first affected. I could pass my finger around the os uteri, it was so perfectly smooth, but the discharge coming through it was horrid enough. Here is a fresh cut, here is scirrhus softening, and here it is as hard as cartilage.

The kidney was perfectly healthy, and so were many other parts. I was not present at the examination, but with respect to the abdomen, the stomach contained much mucus, as well as the duodenum, in which also there was some bile. The intestines, from their commencement at the duodenum to their termination, throughout their whole tract, were distended with fluid of a milky-whitish colour, containing white flakes, and having much the appearance of gruel. Now this is exactly the kind of fluid which is found in the stomach and intestines of many persons, in fact, the greater number who die of cholera. There were no fæces, I understand, in the intestines—a lumbricus was found, but no fæces. The glandulæ solitariæ were enlarged throughout the whole length of the intestines, and the fluid contained in the stomach and intestines reddened litmus paper.

Now as to these circumstances, twice last week I was present at the inspection of persons who died of cholera, and the alimentary canal was filled with exactly such fluid as this, and litmus paper, whether it was dipped in the fluid of the stomach or the intestines, was reddened in both cases; but in one case I distinctly noticed that the litmus paper was reddened more at the stomach and upper part of the intestines, than anywhere else. As we went down, the paper was less and less reddened, till at last it was not reddened at all, and on applying turmeric paper where the litmus was not reddened, that was not discoloured, so that the fluid was not alkalescent. I mention this, because it has been observed that the fluid in cases of cholera is alkalescent. It is certain from the case of cholera before us, and the two cases to which I have alluded, and which undoubtedly were cases of cholera, that the contents at the upper part of the intestines were acid, and proceeding to the lower part in one case they were neutral. I recollect, that in trying the stool of another patient who was under the care of Dr. Roots, and who had cholera in the hospital a fortnight ago, the litmus paper was reddened by the stool.

I have no doubt whatever, now that the body has been opened, that the case before us was one of cholera. I had a strong suspicion of it during life, but there was no discharge by which I could ascertain it, but the intestines were filled after death with a white fluid containing flakes, having all the appearance of gruel.

In the case that I saw examined last week, the surface of the stomach and intestines was of a fine pink colour, a bright rose colour, and I should say, clearly showing an inflammatory state. The vena cava inferior and superior was immensely distended, in such a state of distention as I never saw before: whether it was so in this case I do not know. The blood in one case, I forget how it was in other, was grumous, half coagulated in black lumps. The blood in the left side of the heart had coagulated just as usual, the fibrin had separated from the red particles in some parts, so that you had just the white collection of fibrin that you have every day. The remarkable circumstances were, the distention of the left side of the heart and the venæ cavæ, and the blood being in this half-coagulated state, exceedingly grumous.

Remarks.—With respect to this disease, I have really seen enough to convince me, *that it is an affection perfectly new in this country, and that it is the same disease that has prevailed in India.* On these points I am perfectly satisfied. I have been physician to this hospital fourteen years, and I never saw the disease before the last month, never saw anything at all like it. It is quite in vain to say that it is a species of common cholera, because in the slightest forms of this present disease you have, generally, this peculiar white matter. Now in the disease to which the name cholera is given in this country, you have a discharge of bile; the motions are bilious—it is principally a discharge of bile; but in this disease you have no bile discharged, except at the beginning or close of the disease. When the vomiting and purging first occur, the bile already in the duodenum and intestines is discharged, but when the disease is fully formed, no more bile in general goes into the intestines; and the slightest cases of this description are characterised, not by bile, but by these motions. Cases so slight, that they do not cause death, or cause the patient to be ill above a day or two, are attended by these particular motions. If this were only a modification of cholera, slight cases ought to be like ordinary cholera, and we should have bilious stools, but that is not the case. A gentleman present at a dissection, who has been in the way of catching the disease, has merely purging, but the diarrhœa is characterised by white fæces. As to the name cholera, nothing can be more absurd, because it means bile flow, and in this disease there is anything but bile-flow. *Leucorrhœa* would almost be a more appropriate name if it were not already engaged.

You know that the symptoms observed in this disease prevailing now in this country, are diarrhœa and vomiting; sometimes diarrhœa existing for a larger or smaller number of days before any other symptom comes on,—diarrhœa of this white description, watery, not feculent, and containing these white flakes. Sometimes this disease begins all at once, without any previous diarrhœa, and when it does begin, there are generally violent spasms of the calves of the legs and thighs and trunk, and sometimes in various other parts. The nails and ends of the fingers frequently become blue, and sometimes before the body is dead, it is blue from head to foot. Sometimes the fingers also become shrivelled, and the eye sinks into the head, so that there is a dark circle round it. The voice becomes feeble; great prostration of strength is felt; the extremities are cold; the pulse is not to be felt perhaps at all at the extremities, although it may at the wrist, and patients may still experience vomiting and purging. When patients do not die immediately, the symptoms become fainter after a time, and signs of congestion take place in the head, but I have not yet seen that stage: the patient looks as if he were labouring under typhus fever. However cold the body may be, many hours after death it often becomes exceedingly warm. These are the things usually observed. Even the fingers and toes often move after death. You will not find perhaps two cases of cholera alike; some patients are not blue, and there are great variations, but these are the general symptoms, and I have seen most of them myself in this epidemic.

[The learned Professor then read several extracts from a pamphlet entitled *Cholerica Indica*, published by Dr. W. H. Foot, of Edinburgh in 1825, for the purpose of showing the perfect identity between the present epidemic and that which prevailed in India.]

Treatment.—With respect to the treatment, we had better say nothing about it. It is my own private opinion that no good has, on the whole, been done; that if all the patients who have been treated had been simply put into bed, and made as comfortable as possible, we should have had as many recoveries as with all the means that have hitherto been used. I hope that those who attend to cholera patients, will go on making a cautious trial of some new means, or employ the means already in use in some fresh mode; for I cannot but think that the mortality has not been diminished by any measures hitherto adopted. I feel satisfied that many have been over-treated—have had measures used quite indiscriminately and in excess. I may add, that *I have seen enough to convince me also, that the disease is contagious*,—may be produced by emanation from a system affected by the disease; though I do not doubt that it may be also taken without contagion,—that something in the air will produce it, without that something having proceeded from a diseased body. It is a remarkable fact, that Dr. Prout, who has long been weighing the atmosphere twice a day, found it suddenly become very heavy exactly at the time that cholera broke out, as if something were added to the air, and that it long continued so heavy, up to the present time.

I may also mention, that for the last three months, Dr. Prout has observed an extraordinary change in our secretions. He has found the perspiration, which is mostly acid, to be exceedingly so: the saliva, which, except in some cases of dyspepsia, and before death from organic disease is neutral, to be acid; lithic acid and lithote deposits, to disappear, or be greatly diminished in the urine, and an acid more like oxalic to prevail in it, and the morbid deposits of patients, which before were usually compounds of lithic acid, now to be the oxalites. Something evidently exists in the atmosphere at present, which exerts a strange power over, at least, the human body.

I may add, that in the woman whose case I have mentioned, the bladder was found contracted, and contained a thin fluid, with white flakes, like that which was discovered in the alimentary canal.

⁂ Our reporter has requested us to append the following note to this part of the present lecture:—Since the cholera has again grown violent in London, Dr. Elliotson has employed the *saline treatment*, and has a high opinion of it. The cases he has lost were, we understand, of extreme rapidity, and were such as he says that he should henceforth feel justified in treating with far larger and more quickly-repeated doses of the alkaline salts, than those given in ordinary cases.

The very interesting fact may be here added, that when cholera appeared again in St. Thomas's, three or four weeks ago, after its entire absence, during the intervals it broke out in a patient lying ill of some other disease *in the very bed in which the patient whose case is detailed above, lay*, though the bed had been scoured and cleansed in every way. The cholera has now again entirely ceased in the hospital, and, indeed, is hardly, if at all, seen in the Borough. It may here be stated, and the circumstance is a curious and important one, that Dr. Prout discovered the sudden increase of weight in the atmosphere alluded to in Dr. Elliotson's lecture, to occur almost *on the very day* on which cholera broke out in London.

LONDON HOSPITAL.

CLINICAL LECTURE

BY

DR. BILLING,

Delivered Jan. 28, 1832.

HYSTERIC DISEASES.

I HAVE a few cases of hysteric disease to remark upon to-day, and in the first place that of S. S., who has been mentioned already (see LANCET, last volume, page 583), which you may take as the prototype and beau ideal of hysteria, and which is worthy your continued attention, as you will in your practice meet with cases of the kind not unfrequently, though, fortunately for the patients, few so bad—bad as to the sufferings of the patient. She has been in the house since Nov. 18th, and has already had many of the remedies which have been found efficacious in similar cases, with no good effect, at least not permanently; but I shall persevere in the treatment, both as a duty to the patient, and because I wish you to have a thorough acquaintance with the phases of such a malady. We cannot treat her as an out-patient, as her home is in the country, but many of these patients are much better out of the hospital, as having more exercise and air. You have seen that without chiding her and the nurse, we cannot keep her out of bed, though she is the picture of health; most patients in her state have a propensity for lying on the back, and if they remain in bed, the heat produces relaxation and increased irritability of the nerves of the spine and sacrum; they ought to be kept out of bed, and every effort made to break them of bad habits they may have acquired; their fingers should be employed with some kind of light work, or a book, &c. as much as possible; and when it is necessary (which sometimes is the case, on account of bearing-down pains) that they should keep a good deal in a horizontal position, they ought to lie on a firm sofa, or on a thin mattress, or on a board.

When I last mentioned the case of S. S., which was on the 18th December, she was taking *ferri carbonas*, which had the good effect of removing the nervous tremors and headache, but did not prevent the recurrence of the fit, which occurred at its usual period; you had a specimen on the 15th, as she was attacked, just as we went into the ward, with a fit of hysterical epilepsy, violent convulsions, and insensibility, knocking herself about, and trying to bite, the eyes squinting obliquely upwards to the utmost; cold water was applied to the face and epigastrium, smelling salts to the nose,

&c., *secundum artem*; but these have little effect on the duration of the fits; your efforts must be directed to preventing their recurrence. The principal thing during the fit is, to prevent their injuring themselves; and this is difficult, for even the necessary force used in holding them down, makes them feel the next day as if they had been beaten all over. You know that a person who is sensible, will cease to exert the muscles when the resistance gives pain, but in convulsive fits, the patient, not being conscious, exerts the muscles to their utmost power, appearing to have extraordinary strength, and thereby strains them, so that the day following they are all in pain.

After this she had a better excuse than ever for sticking to bed. You had then an opportunity of seeing how little real benefit is derived from bleeding, even in such a plethoric subject as this; and I must repeat to you my reasons for resorting to it, contrary to my usual practice in hysteria. The effect of the pains of the limbs, almost like acute rheumatism, and her other usual sources of irritation, produced heat of skin, increase of pulse, and headache, with permanent flush, and an occasional strabismus, besides which, the usual periodical return of the catamenia had been retarded; under these circumstances it would have been carrying too far the objections to the secondary consequences of bleeding, to have run the risk of the brain being affected; and I must warn you, that the effect of the cerebellum becoming inflamed in such a case would be, the occurrence of nymphomania, the most melancholy visitation with which a modest woman could be afflicted, a disease in which the sensations are all painful, and which no persons who know the nature of it would speak or think lightly of, any more than of hydrophobia. I ordered her *to be bled* from the arm ℥xvj, and to have *a blister* on the nape of the neck, which relieved her head; but having still much pain in the pelvis and lower extremities, I had her *cupped over the sacrum*, at the origin of the nerves of those parts, and afterwards a *blister* applied; her pains were relieved, and the catamenia were restored. You ought to know that amenorrhœa is frequently relieved in plethoric habits by bleeding from the arm, or cupping the sacrum, or leeching the pubes or thighs,—as it is, in delicate habits, by stimulants, aromatics, and opiates; thus I have constantly to remind you and to exemplify, that diseases with the same name require opposite remedies.

As the fits were threatening on the 20th, and she once had been benefited by the *ol. terebinth.*, I ordered it in *enema*, to avoid irritating the kidneys, and she appeared better, but had a fit on the 27th. In the beginning of this month, she became more hysterical than ever, lay in bed constantly, had two or three fits a day, and had complete hysterical loss of voice; she also got that hysterical tenderness of abdomen which simulates peritonitis, but is entirely neuralgic, not inflammatory, and hence so often relievable by carbonate of iron. I took this opportunity of pointing out the diagnosis to those gentlemen who were with me in the ward on the 13th instant. In the first place the abdomen was exquisitely tender; indeed more so than in peritonitis, for in peritonitis the skin is not tender as in hysteria; but there was no confinement of bowels; the tongue was clean and the skin soft; the pulse 132, which, I remarked, was too quick for inflammation; the pulse of an adult is scarcely raised above 120 by inflammation, but these nervous patients have it from 130 to 160, and when I feel this nervous flutter, it relieves my mind from apprehension of danger, with the other absence of symptoms which I have just stated. After this explanation, I pointed out that she was sobbing, and just on the point of going off into a fit, as she had done several times lately whilst we were in the ward, and that I would try if the effect of making her exert herself would not be to keep it off; I told her rather suddenly, that I insisted on her keeping out of bed in the day-time, and that if she could not stand she must lie in her clothes on the bed, but not in it, and that she must exert herself, as it was the only way to get well, and so persuaded her out of the fit for that time; I at the same time desired the gentlemen, to convince them there was no inflammatory disease requiring active treatment, that she should take only *aqua menthæ* ℥iss *ter die*, and to relieve the uneasy sensations in the abdomen, she had a mustard poultice applied in the evening: she has been going on very well with the aqua menthæ ever since. On the 17th I ordered the watering-pot douche to the loins, which strengthens the nerves, and she would now be better, but that it is just the period at which the fits usually recur, and she has had two or three lately; you shall see me persevere, however, and though I do not expect to be able to cure her whilst she remains in the hospital, as it is not justifiable to occupy a bed so long with a chronic case, she is by no means incurable, and may get well more rapidly at home than you would suspect.* You must not give way to the idea that the disease is mental, though mental exertion, you see, influences it, and you must turn that to account in treating these cases; the toothache is not mental, yet if very busy, a person forgets it for the time. Matrimony produces a total change in the thoughts, produces fresh cares and occupations, and hence has a powerful moral effect in this disease, independant of utero-gestation or other considerations.

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* This patient was dismissed in the middle of March, still subject to fits, after having had fœtid, antispasmodic, mercurial, and other remedies; she appeared in perfect health, as when admitted; if any thing rather stouter.—Rep.

AMENORRHEA.

I have now some cases of amenorrhea to allude to; first, those which are simple; secondly, those connected with vicarious hemorrhages.

S. G., ætat. 19. Dec. 8. Complains of pain and giddiness in the head; faintness and debility; pain in the left hypochondrium; uneasy sensation in the throat (globus); feet cold; pulse, tongue, and skin, natural; appetite good; bowels confined four days, and are generally costive; has been ill three weeks, but catamenia absent three months.

> *Pil. hydrarg. Pil. aloes c. myrrha aa.* gr. v. *alt. noct.*
> *Magnesiæ sulph.* ʒi;
> *Liq. antim. tart.* ʒss, *ex*
> *Aquæ* ℥iss *ter die.*

This was a case of simple amenorrhea brought on by the most common cause,—cold or wet feet during the catamenia, and eventually producing the train of hysteric symptoms, with debility, preventing her going through her work as servant; she was not one of those who require to have the occiput shaved. Besides having globus hystericus, she was just beginning to get that white appearance about the lips and mouth which would have terminated in chlorosis. You find that in these cases, sympathy with the uterus brings on a sluggishness in the intestines, which causes costiveness, and the most effectual emmenagogues are those tonic laxatives which renew the activity of the canal,—the blue pill by increasing the biliary secretion, and the old-established Rufus pill, in which the myrrh and aloes are both tonic, the aloes tending to keep up that action in the lower intestines which the blue pill does in the upper region. Sometimes blue pill, or calomel, is sufficient for all, and thus the sympathetic reaction on the uterus becomes emmenagogue. On the following day, finding that though the bowels had not acted there was no inconvenience, and that the abdomen was neither distended, nor tender on pressure, I thought it better to give her only the emmenagogue laxative, and at the same time *ferri carbonatis* ʒj *ter die*, instead of the mixture, as it would much more improve the chlorotic state; and, besides, you will often find the iron a most powerful means of keeping the bowels regular, when their sluggishness is from want of tone; another thing which decided me in not forcing the bowels with purgatives was, that her head was much better, which showed she was not suffering from not having had a motion for five days; and you will often find a patient relieved by the influence of a medicine on the primæ viæ, sometime before it produces a motion; she continued these medicines for a fortnight, and then left the hospital quite well, and able to resume her employment.

M. D., ætat. 20, unmarried. Dec. 2. Complains of palpitation and dyspnœa on walking or going up-stairs; pain in the left leg; debility; she has nausea, and flatulence after eating; tongue furred and pale; bowels confined; amenorrhea; is very pallid, and says she has suffered privations.

> *Mist. ferri comp.* ℥iss *ter die.*

This was another case of simple chlorotic amenorrhea, in which the tonic myrrh and iron mixture, with good diet, kept the bowels regular, and restored the health and strength in three weeks, without any other prescription.

VICARIOUS MENSTRUATION.

There are two cases of vicarious menstruation. It is not a very uncommon thing, when there is amenorrhea, for the patient at the regular periods to bring up blood, either from the stomach or lungs, or have epistaxis; sometimes this hemorrhage takes place from an organ not previously diseased, but suffering from sympathy with the uterus, as in the young woman J. C., who went out sometime ago well, after having had spitting of blood, vicarious, with amenorrhea; she first had hysteric cough, and then hemorrhage from the irritated bronchia; the treatment in these cases being, emmenagogue medicines, combined with dilute sulphuric acid, and other cooling astringents, and quiet when the hemorrhage occurs; but in the case of E. C., who is now in the house, there is more difficulty, as the hemorrhage takes place from a lung previously diseased.

E. C., ætat. 32, unmarried. Dec. 30. Amenorrhea for eight years. She has had cough for two years (from exposure to cold after being called out of bed in the night), during which, at the usual periods of the catamenia, she has either epistaxis, or spits blood; leucorrhea profuse; expectoration now not scanty, but tough muco-pituitous; rhonchus sonorus gravis and sibilans; crepitation and dull respiratory murmur, with bronchial respiration, and dulness on percussion inferiorly on right side; legs œdematous; tongue clean; pulse 74, soft, and regular; bowels confined; urine scanty.

> *Decoct. senegæ* ʒx;
> *Syrupi papaveris albi* ʒij;
> *Zinci sulph.* gr. ss *ter die.*
> *Pil. hydrarg. Pil. aloes cum myrrha; aa.*
> gr. v, *alternis noctibus.*

The explanation of the symptoms is this: This woman has had leucorrhea and amenorrhea for years; she caught cold two years ago, and had, in consequence, peripneumony of the right side low down, in which place hepatisation of the lung exists, but in a very passive chronic state, as her pulse, &c., indicate; she coughed and expectorated from chronic catarrh, for which I prescribed the tonic anodyne and expectorant mixture, and, for the uterine symptoms, the pills; there was a slight crepitation on the edge of the hepatised portion of the lung, but as there was no acute symptom, it did not call for any active depletion. The medicine at first produced nausea, and relieved the cough; her mouth became sore in less than a week, and, as it did so, her pulse became hard. On the 13th, the usual period, she brought up about a teaspoonful of blood; considering the equivocal state of the lung, I had her bled to ℥x, which relieved her much, and she brought up no more blood, only the expectoration was discoloured, of a brownish red. You must observe that much of her dyspnœa, palpitation, and pains, is nervous and hysterical, as well as the globus and low spirits, depending on the old uterine affection, which make me less anxious about her, though the state of the lung must require constant care.

I have ordered her now ʒij *cubebs ter die*, which, besides being indicated for the leucorrhœa, is as good a medicine as she can take for the chronic catarrh.[*]

Some years ago I was consulted by a young lady who had had, at first, vicarious hæmatemesis every month, but the hæmatemesis had become more frequent, accompanied with pain at the pit of the stomach, dyspepsia, constipation, and dropsical symptoms. I prescribed *leeches to the epigastrium, magnesiæ sulph.* ʒj, *with dilute sulph. acid.* ♏xv, *ex. inf. rosæ comp. ter die*, which stopped the hemorrhage, kept the bowels in order, and removed the pain; after this I gave small doses of *ol. terebinth. ter die*, which improved the state of the mucous membrane; and, subsequently, the *emmenagogue pills*, with *carbonate of iron*, restored the catamenia and the health of the patient.

CASES OF PETECHIAL FEVER CONCLUDED.

We must now continue the consideration of the two cases of fever mentioned in the last lecture. On the 23d, S. J. was relieved so far, that she was free from delirium and got sleep in the night, but still had hot skin, headache, pulse 120, and complained of thirst, and of her throat being sore; the bowels were too much purged, and therefore I left off the mixture, and ordered her

> *Sulph. quininæ* gr. i;
> *Acidi sulph. dil.* ♏j, *ex*
> *Aquæ* ℥j *ter die.*

On the 24th she said she felt better, her head nearly clear, and had had some sleep in the night; the skin continued hot, the pulse still harder than natural, and the tongue furred; the bowels continued to be relaxed enough, four or five times in twenty-four hours, and therefore I made no alteration in the medicine, except the addition of gr. j *ipecacuan.* to each dose as an expectorant, as the cough continued; and as there remained some tendency to flushing, and heat in the head, she had *eight leeches to the temples.* The *ipecacuanha* produced some nausea and expectoration; her pulse fell on the 26th to 108, and soft; skin more temperate; the bowels continued relaxed. The report yesterday, the 27th, was, that she felt much better; pulse 96, soft, and full; coughs still, but expectoration easy, mucose; tongue moist to-day; she may almost be pronounced convalescent.[*]

M. H. continued to suffer from pectoral and gastric inflammation, notwithstanding the active and antiphlogistic measures adopted; on the 22d she had *six more leeches* applied to the *sternum*, which removed the pain, but the dyspnœa and cough continued. On the 23d she had less fever, little or no headache, and the skin soft; she was very much purged, but no involuntary motions, and I *omitted the antimonial mixture*, and gave her the *emulsio oleosa* for her cough. On the 23d, nine p. m., as the purging continued, Mr. Williams thought it necessary to give ♏x *tinct. opii post sing. sedes liq.* After the second dose she fell asleep, and on the 24th she felt better in the morning in consequence of having had rest, but in the afternoon became worse, the crepitation continuing, with dyspnœa, and the cough sounding wheezy. She had *decoct. hæmatoxyli* for the bowels, with gr. ij *ipecacuan.* as an expectorant, 6*tis horis*; *eight leeches* and a *blister to the chest*; next day she felt better, and on the 26th also, though she had been delirious in the night, and her breathing was oppressed at times, although the respiratory murmur was better. On the 27th, yesterday, she was much worse again; she had been delirious and restless in the night, getting out of bed; she was at two p.m. incoherent, talking anxiously about members of her family; the respiratory murmur became weaker again on the left side, with crepitation, and respirations more frequent, besides which she now and then gave a very faint suppressed cry, as if something gave her pain, though she was too confused to give any account of what it was, but there could be no doubt from the symptoms that there was a renewal of pleurisy, or rather pleuroperipneumony. Under these circumstances, though she was much reduced, and the pulse was becoming weak, it was necessary to *take some blood* from her, which we did to the amount of ℥xvij, upon which the pulse became rather fuller, and she less restless. I directed her to have the *head shaved*, and ordered her *decocti cinchonæ* ℥j *ter die*. for her skin was temperate, and though the vessels required unloading, the constitution needed support, and she was constantly supplied with *milk*. To-day the report is very favourable, though she can scarcely be said to be out of danger; she was very restless in the early part of the night, but slept afterwards, and is drowsy to-day; not comatose, being collected when awakened. Says she feels hot all over; pulse 108, not weak; tongue moist on the edges; bowels free; urine natural; skin temperate; does not cough frequently as before.

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[*] This woman left the hospital in the middle of March, having been free from cough for some time, but the hysterical symptoms and leucorrhea continued, for which she was directed to attend as an out patient; the *cubebs* had been changed for *mistura ferri comp.* on the 2d of February.—*Rep.*

[*] The convalescence was gradually progressive from that time; the patient subsequently had only some emmenagogue and tonic medicines.—*Rep.*

If she has no return or exacerbation of pleuro-peripneumony she may do very well, as the fever is gone.* I must again impress upon you the necessity for giving nourishment, and not merely contenting yourselves with ordinary medicines in fever, when patients are in a state of insensibility, at which time they are not conscious of thirst, though standing much in need of the support of drink, of which you will be convinced by seeing the greedy thirst of patients just recovering their sensibility in fever; if you do not insist upon the attendants giving supplies of liquid at this stage. they will, perhaps, let the patients sink, from their not merely not asking for, but refusing it, and you must watch your opportunity to get them to take a sufficient quantity at a time, from a quarter to half a pint, as during a period of insensibility it is difficult to get them to swallow; and, besides, you must caution attendants against putting liquid into the mouth of a patient lying down insensible, as I have no doubt in this way medicine or nutriment has occasionally become the cause of death rattle, by getting into the trachea. When attendants tell you patients will not, or cannot, swallow a spoonful, you may, nevertheless, get them to drink a teacup full, by setting them up, and rousing them a little, which though not desirable so far as the fever is concerned, must be done to prevent inanition. Whilst the fever lasts, I never wish my patient to drink anything but spring water or milk, and you will find these most uniformly grateful; water for thirst, milk for nourishment. Most generally the smell of food, particularly meat, is nauseous to fevered patients; and hence I do not order beef-tea or broth; milk is at the same time the simplest and best nourishment for all ages.

DISEASES OF THE HEART.

Both the cases of disease of the heart, which I explained to you a fortnight ago, are improving. T. M. (p. 330) in particular has every prospect of getting permanently well; he continues to take the digitalis and opening medicine; his pulse is now 80, and though his breathing is still easily affected by exercise, he is stronger.*

The other man, J. W., has made some progress, but is still suffering from rheumatic pains, though less; he has been taking gr. v *sulphate of quinine*, with ʒss *vini colchici ter die* since the 20th; he has still a difficulty of lying on the left side; bruit de soufflet, and the other symptoms of the organic disease of his heart, for which we must keep him at rest as long as we can.†

CONCLUDING REMARKS TO THE COURSE.

I have now, Gentlemen, come to the conclusion of my three months of clinical lectures, but although I vacate the chair, I shall continue to give you less formal, though, perhaps, not less useful instruction, both in the wards, and in pathological examinations, when opportunities occur. Indeed part of the clinical lecture must necessarily be a recapitulation of peripatetic observations, but those observations are sometimes not heard. I recommend you carefully to attend to the practice of the other physicians, as well as the constitutional treatment adopted by the surgeons. I can assure you, that you will glean something from the practice of each and every one, and it is only thus that you can learn to be what is of so much consequence, unprejudiced. I have heard teachers recommend pupils not to wander from one professor to another, for that thus their notions would be unsettled, and not regulated by any system. But as no system which is the work of man can be perfect, I advise you to learn from one professor to correct the system of another, and so far as you have time and means, to visit other schools, both in this country and abroad.

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* From this day she continued to get better. On the 31st the cough was "slight and loose," and she quickly recovered her strength. There was no alteration of her medicine after the 27th.—*Rep*.

* He was discharged from the hospital on the 7th of February, "feeling quite well; no dyspnœa or impulsion on ascending the stairs; pulse 84, full, and steady."—*Rep*.

† This patient was permitted to leave the hospital at his own request on the 5th of March as he "felt well; pulse 88, regular, soft, still jarring; no dyspnœa or pain in the chest, but bruit de soufflet still, and impulsion upon exercise."—*Rep*.

You will do well to attend constantly to the treatment of the out-patients, as you can thereby learn the nature of many chronic diseases, especially those of the eye and skin, which are of much consequence though not generally requiring admission into the wards. As far as our time would allow, I have shown and explained to you our new and inestimable mode of diagnosis by auscultation. I feel that to the practical physician it is like the acquisition of a new sense; but this you must practise yourselves, constantly, in order to render perception clear. Let me, however, remind you, that its value is in distinguishing the disease, and that though, according to the proverb, the knowledge of the disease is half the cure, it is but half, for if you keep your ear to the stethoscope for a week, it cannot whisper to you what remedies to resort to; therefore, practise auscultation and morbid anatomy to ascertain the nature of diseases; but withal, watch well the effects of remedies in order to cure them. I have endeavoured to teach you the true value of morbid anatomy,—to show you, that though some diseases are incurable, you may yet prolong life, and that you will do so more certainly, by knowing that the disease can only be palliated, than if, from not being assured of that, you should attempt impossibilities; and this applies more especially to organic alteration of the heart.

There are some men who, through ignorance, from want of being better taught, underrate morbid anatomy; I say through ignorance, for I hope there is not any medical man so devoid of principle as to neglect it if he knew the value of it as much as we do. It is true that there are some diseases connected with a derangement of the nervous system, the immediate cause of which has not yet been satisfactorily detected by dissection; but we shall succeed in time, I believe; and the phrenological mode of unravelling the brain must at length lead to discoveries of the nature of several obscure diseases. It is of the greatest consequence to you, in order to gain confidence and steadiness in the application of remedies, to lose no opportunity of post mortem examinations, and especially in acute diseases, such as fevers and inflammation of the viscera. A knowledge of the true nature of diseases gained by morbid anatomy, will give you confidence to adopt decided practice; knowing that if the disease be within the power of remedies, you will cure, and if the disease be mortal, you will not hasten death a moment; on the contrary, perhaps, gain time for the sufferer to settle his affairs, which is often of the utmost consequence to a family.

In order to instruct you in morbid anatomy, I have not confined myself to the specimens obtained actually from our own clinical patients during the session, but I have also resorted to the museum for exemplification, and have brought forward specimens obtained out of doors, with which I have been favoured by friends, including former pupils, to whom I feel much obliged. Whenever I receive any specimen, or letter communicating medical facts, from a former pupil, it is a source of gratification, as a proof of that good understanding and friendship which ought to exist when each party does his duty; I receive it as a proof of conviction, that what is thus communicated will not be thrown away, and that there is a feeling of agreeable recollection of our school, and that my old pupil wishes to help me in instructing my new ones, that he does not consider me in his debt, or that I had broken faith with him in any way, but that, on the contrary, I had given him value for what he paid according to the custom of the school,—that he has that feeling which makes him wish us prosperity, as I do most cordially to all of you, Gentlemen, in taking leave of this class for the present.

THE EXPERIMENTS
OF
FARADAY, NOBILI, AND ANTINORI,
ON A NEW CLASS OF
ELECTRO-DYNAMIC PHENOMENA.

The late discoveries of our distinguished countryman Dr. Faraday in this important branch of physics, has excited the attention of natural philosophers in every part of Europe, and his experiments have been successfully repeated and modified by many of his contemporary savans. Amongst other exotic inquirers, Signori Nobili and Antinori of Florence, who derived their first knowledge of Dr. Faraday's pursuits from a short notice given by M. Hachette to the Academy of Sciences at Paris, and afterwards published in "Le Temps" journal, are the most distinguished. The researches of Nobili and Antinori were published in the Antologia, No. 131, and were translated into the Philosophical Magazine of June last. This paper has been extracted by Dr. Faraday, and printed, with annotations, for private circulation, but as the reputation of this justly eminent chemist is now a sort of national property, we shall make no apology for devoting a portion of our pages in justification of his claims to these important discoveries. It has been said, and the assertion has been industriously spread by certain *liberal* and *candid* persons, that the two Italian physicians were the originators of the inquiry, but we are happy to observe, that those gentlemen do justice to the merit of our countryman, and frankly declare, that their "particular opinions do not in any way diminish the intrinsic merit of Mr. Faraday's discovery. It is one of the most beautiful of our time, whether it be considered in itself for the largeness of of the vacancy it serves to fill, or for the light it throws over the various theories, and especially that of the magnetism of rotation." The following is the notice which appeared in "Le Temps," and which being translated into several languages, has served as the text of all subsequent papers on magnetic electricity.

"The memoir of Mr. Faraday is divided into four parts. In the first, entitled Induction of Voltaic Electricity, is found the following important fact, that a voltaic current which traverses a metallic wire, produces another current in a neighbouring wire; that the second current is in a direction contrary to the first, and continues but for a moment; that if the producing current is removed, a second current is manifested in the wire submitted to its action, contrary to that which was first formed in it, i. e. in the same direction as the producing current.

"The second part of the memoir treats of electric currents produced by the magnets. On causing helices to approach to magnets, Mr. Faraday has produced electric currents; on removing the spirals, currents in the contrary direction were formed. These currents act powerfully on the galvanometer; pass, though feebly, through brine and other solutions, and in a particular case, Mr. Faraday *has obtained a spark*. Hence it follows, that this philosopher has, by using a magnet only, produced the electric currents contemplated by M. Ampere.

"The third part of the memoir is relative to a particular electric state, which Mr. Faraday calls *electro*-tonic *state*." [Vide LANCET, Nos. 456, 457, 458.]

"The fourth part speaks of the experiment, not less curious than extraordinary, of M. Arago, which consists, as is known, in making a magnetic needle revolve under the influence of a rotatory metallic disc, and *vice versa*. Mr. Faraday considers this phenomenon as intimately connected with that of the magnetic rotation, which he had the good fortune to discover about ten years ago. He has ascertained, that by the rotation of the metallic disc under the influence of a magnet, there may be formed electric currents in the direction of the rays of the disc, in sufficient number to render the disc a new electrical machine."

The Signori proceeded to observe, "We had no occasion to make trials before we succeeded in the experiments of Mr. Faraday. The first spirals which we brought near the pole of a magnet, quickly manifested their influence on the galvanometer. We observed three facts in succession. Whilst approaching the magnet, the needle of the instrument is, in the first place, seen to deviate a certain number of degrees, which indicates a current excited by the magnetism, in the spirals previously made to communicate with the galvanometer. This current lasts but for a moment, and is then completely extinct, as is proved by the needle returning to its first position. This is the second observation. The third (finally) occurs when the spiral is taken from the magnet: the needle of the galvanometer then deviates on the other side, demonstrating the development of a current contrary to that excited in the first instance.

"On experimenting with an annular spiral between the poles of a horse-shoe magnet, we observed that the action was much less than that produced with the same spiral when the lifter of the magnet was put to it or suddenly taken from it. This fact suggested the idea of rolling a copper wire, covered with silk, round such a magnet, so as to have an apparatus always mounted for the experiment in question. The spiral tube subjected to the magnetic influence is then always upon the magnet, and the immediate cause of the phenomena resides in the lifter, because of the property which that little piece of soft iron possesses, of being magnetized and demagnetized rapidly. When the lifter is detached, the spiral, which, before, was, in the presence of this piece of iron, strongly magnetized, is suddenly removed from its action, and represents the case of a spiral, which having been first approximated to a magnet, is then removed. When the lifter is replaced, it is as if a magnet were caused to approach the spiral, for the lifter becomes magnetic on being attached to the poles of its own magnet.

"This arrangement, besides being very

active, has the advantage of supplying the philosopher with a *constant source* of voltaic electricity. The want of a constant current is often felt in such researches; and if thermo-magnetism offers a plausible means of satisfying such necessities as I have indicated elsewhere ('Ann. de Chimie,' Feb. 1830, p. 130), yet the new method offered us by a magnet covered with electro-dynamic spirals, is not to be despised. Here the currents are always ready to be manifested. Suppose, as is usual, the lifter of the magnet is in its place; nothing more is required to obtain a current in the spiral than to detach the lifter, the current in the wire being, as it were, at first in a latent state.

"There are two modes of using this arrangement: the one by attaching the lifter, the other by detaching it. When the two motions are made with the same rapidity, and with relation to the same points of the magnet, the deviations are in the inverse directions to each other, but precisely of the same value. The detachments are, however, always equally instantaneous, and for constancy of effect are preferable to approximations; for the latter, to be always equally successful, would require a mechanical arrangement, which it is not worth while either to imagine or execute. By taking care that the lifter is constantly in its right place and position, there will always be produced the same deviation of the galvanometer when it is detached from the magnet. This, we repeat, is a valuable result applicable in numerous cases, and perhaps proper to measure the force of large magnets in a more exact manner than by the ordinary mode of ascertaining the weights sustained.

"The arrangement described is highly advantageous; but does it produce the maximum of electro-dynamic effects? There is indeed another, much better, which consists in applying the electro-dynamic spiral to the central part of the lifter, corresponding to the internal, which separates the poles of the horse-shoe magnet. In this position a spiral of a few turns is able to surpass the effects of a far greater number of spirals otherwise disposed. The central part of the lifter is to be entirely covered with wire, leaving exposed only the extremities, which are to come in contact with the pole of the magnet. The ordinary form of the lifter is not the most convenient upon which to arrange this species of electro-dynamic ring, but upon conveniently modifying its shape, the wire may be applied with facility, and thus the effect be obtained at its highest degree of intensity. The reason is evident; for two conditions, in fact, require to be fulfilled: one, that the spiral should be subjected to all the influence of the magnetic force; the other, that this influence should be abstracted in the shortest possible time. Now the wire round the lifter is exactly in the most favourable position for the magnetic force to be concentrated upon it; and this force vanishes the instant the lifter is detached, as is required by the second condition."

Hitherto these philosophers have been anticipated in all their experiments and conclusions by Dr. Faraday. They next inquire into the affections of certain metals as respects the new electro-dynamic principle.

"SPIRALS OF VARIOUS METALS.

"The metals with which we have experimented are four, *copper, iron, bismuth, antimony*. Iron is interesting as the foremost amongst magnetic metals; bismuth and antimony for the distinct position they hold in the thermo-magnetic scale. In experiments made under circumstances approximating to equality, it appeared that copper was the most active in the present point of view; then, at a little distance, iron; afterwards antimony; and, finally, bismuth."

These gentlemen preceded Dr. Faraday in the discovery of the electric spark. That gentleman alludes to this circumstance in the following terms. "Being much engaged in the investigation and confirmation of the laws of magneto-electric action, terrestrial magnetic induction, &c. &c., some of the results of which are contained in my second paper it, will be seen that in the race which Sig. Nobili and Antinori probably inadvertently ran against me, they obtained the electric spark from the common magnet before me. I have great pleasure in bearing witness to the accuracy of their reasoning on this point, and also to the success of the result. Having made a variation of the experiment, by obtaining the spark from the action of a common loadstone, in which their most perfect mode could not be applied, I will take the opportunity of describing the simple adjustment I have devised. (Vide LANCET, No. 457.) A helix was fixed round the lifter, the wire ends were raised upwards; one which may be called *a*, was bent into a hook; the other *b*, after rising, was bent at a right angle, and had a thick, small, circular plate of copper fixed to it, which was made by the spring of the wire to press in the middle slightly against the rounded end of *a*; this plate and the end of *a* were amalgamated. On bringing the lifter down suddenly on the poles, the momentum of the plate caused it to separate from the end of *a*, and the spark passed. On lifting it up the concussion always separates the end of *a* from the plate, and a spark is again seen. When the plate and point are well amalgamated, the spark will not fail once in a hundred times, either at making or breaking contact. I have shown it brilliantly to two or three hundred persons at once, and over all parts of the theatre of the Royal Institution.

" As Professor Richie expresses it, the spark has not yet been obtained, except from a temporary magnet, i. e., from a magnet in the act of being made or destroyed. I obtained the first spark from a soft iron magnet, made by the well-known influence of electric currents. Sig. Nobili and Antinori obtained the second spark from a soft-iron magnet, made so by the influence of a common artificial steel magnet: their result has been repeated by a great number of persons. Mr. Forbes of Edinburgh first obtained the spark from a soft-iron magnet, made so by the influence of the natural load-stone. The latter experiment is also that which I have made with Mr. Daniell's load-stone, lifting only about thirty pounds, and in the manner described. I was not aware of any other modes of performing the experiment, except my original one, and Sig. Nobili and Antinori's."

The following are the observations of the Florentine philosophers on this point:—

" The voltaic pile gives a spark, only when composed of a certain number of pair of plates. A single Wollaston's voltaic element yields it; and when of a certain activity, produces it constantly at the surface of mercury, to which the conjoining wires destined to close the circuit are conducted. In the voltaic pile having a certain degree of *electric tension*, the sparks pass between the zinc and copper poles, either in the case of opening or closing the circuit. In a single Wollaston's element the tension is feeble, and the spark occurs only when the circuit is interrupted. At that moment the current, which before was moving, accumulates, as it were, at the place of interruption, and acquires the intensity necessary to cause the spark. Such tension is wanting in the other case of closing the circuit, and the spark also is absent." The current developed in the electro-dynamic spirals by virtue of " magnetism, are also in motion, but circulate only for the moment during which they are approaching to or receding from the magnet. It was, therefore, we concluded, in one of those two moments, that we ought to open the circuit in making the experiment for the spark."

The improved plan which these gentlemen devised to secure this beautiful result at pleasure, was this:—

" The whole of the contrivance is attached to the lifter of the magnet. This piece, which is a parallelopiped, is surrounded in the middle by the electro-dynamic spiral, to which it is firmly attached by two pieces of brass, so that the latter can enter between the magnetic poles, whilst the lifter comes in contact with the poles in the ordinary way. The extremities of the spiral come in contact, one with each magnetic pole, by means of two little springs in the form of wings attached to the lifter, and which press slightly against the poles when in its place. To leave room for these springs, the lifter is narrower than usual, covering about half the poles; the remaining space serves for the contact of the springs, which are in this way isolated, as it were, from the lifter; and yet by means of the magnet itself, serve to complete the electro-dynamic circuit. Suppose that the lifter is in its place, the springs touch the poles, and the circuit of the spirals is metallically closed by the magnets. On detaching the lifter, the circuit opens in two places, and either at the one or the other interruption, the spark almost constantly appears. When the effect does not take place, it is because the separation has not been well effected; but it is so easy to repeat the experiment, that it is useless to think of a piece of mechanism to remedy an inconvenience which is so readily obviated.

" In this apparatus the spiral on the lifter was of *copper*. On substituting an *iron* wire, the spark also occurred. This experiment was interesting in illustration of an influence which the ordinary power of the magnet over iron, might exert upon the electro-dynamic influence. It did not appear that the one action disturbed the other."

The next essays of Nobili and his confrère were directed to the subject of *terrestrial magnetism*.

" We took a paper tube two inches in diameter and four inches long; a copper wire 40 metres long was coiled round it, the two ends being left at liberty to connect with the galvanometer; the tube was trimmed at the ends, so that it could be placed upright upon the table either in one direction or the other, at pleasure. A cylinder of soft iron, as is well known, placed parallel to the dip, is subject to the terrestrial magnetic influence; the lower part becomes a north pole, the upper a south pole. This is a phenomenon of position always occurring in the same direction with this kind of iron, which is as incapable of retaining the magnetism received, as it is disposed to receive the new magnetism to which it may be subjected.

" In our latitude, the inclination of the needle is about 63°. The paper tube with its spiral was therefore arranged in that direction, and an iron cylinder introduced. Whilst in the act of introducing it, the galvanometer was seen to move, owing to the presence of an electric current excited by the magnetism. On taking out the cylinder the motion was reversed; there is no doubt therefore, that terrestrial magnetism is sufficient of itself to develop currents of electricity. It should not be concealed here, that in the above experiment the electricity is developed by the intermedium of soft iron introduced into the spiral. This, without doubt, is true, but it is also true that it is not essentially necessary to recur to this aid to obtain unequivocal signs of the influence of which we speak. On placing our cylindrical spiral so that its axis should be parallel to the magnetic dip, and then inverting it by a half revolution in the magnetic meridian, we observed at the comparative galvanometer the signs of a current excited in the spiral by the *sole influence* of terrestrial magnetism. It is not even necessary, for this effect, to place the spiral in the direction of the dip. The experiment will succeed in the vertical position; the effect is less, but always so distinct as to remove every error. We experimented with three copper wires of different diameters; the smallest was 0. 5, the second 0. 66, and the third 1., millimetre in diameter. The effects increased with the size: the first gave deviations from 2 to 4, the second from 4 to 8, and the third from 10 to 20."

The trials of these experimentalists to develop the properties of the electro-tonic state were unsuccessful, but they propose returning to the consideration of the question with more efficacious means, and to extend their research to thermo-electric combinations, which deserve to be studied in the same point of view, as they have never yet presented sensible signs of electric tension. Touching the chemical and physiological effects they observe:—

"The new currents of Faraday pass, although with difficulty, through humid conductors; that such is the fact, may be readily verified by introducing a conductor of that kind into the circuit of the electro-dynamic spiral. In the case of other known currents, I have demonstrated elsewhere, that there is always chemical decomposition when they pass liquid conductors; and that, however feeble they may be, the decomposition is always assured by their transit through the fluid. It is, therefore, very probable, that the new currents will produce the phenomena of decomposition, but their distinctive character of brief duration must not be forgotten. I believe that the time, however short, is still sufficient for decomposition.*

"*The physiological effects* consist, as is well known, in the shocks or contractions of the muscles, the acrid and acidulary taste on the tongue, and the light before the eyes. For obtaining these effects, it is absolutely necessary that the electricity should penetrate into our organs,—these latter belonging to humid conductors. This path, as we have seen, is very difficult for the new currents; nevertheless the frog put into the circuit of our electro-dynamic spirals, arranged around the lifter of our magnet, was powerfully convulsed each time that the lifter was separated or attached. The experiment is beautiful and instructive; beautiful because of the energetic convulsions produced, apparently by the immediate action of the magnet, and instructive because it confirms the fact of the passage of these currents through humid conductors, and because also it shows that the frog is in all cases the most delicate galvanoscope. The frog properly prepared is sufficient for the purpose of arriving at the knowledge of thermo-electric currents, and would have been quite adequate to discover the new currents of Faraday. Although it is not by this road these discoveries have been arrived at, still it is not less true that they might have been made by the simple assistance of this interpreter, which astonished Europe in the first times of galvanism."

Dr. Faraday likewise obtained the effects of the sensation on the tongue, and of light before the eyes. Nobili continues his inquiries into the "magnetism of rotation." "In the experiment of M. Arago we have magnetic poles in presence of a disc, which, instead of being quiescent, is continually moving on its own axis. The latter condition is the only one added, and by it we see that the final result of the phenomena will be excessively complicated, but that in reality nothing new will happen. In all cases it is the currents developed by the magnetism at the place of the disc that is directly acted upon by this magnetism, which are concerned. This part is rapidly removed by the rotation, and another comes forward which is subjected to the same influence, which always tends to form currents in the contrary direction to those which may be supposed to exist in the magnetic pole. These currents, by their nature, tend to be inverted so soon as they are withdrawn from the presence of the cause which produced them, and are, in fact, inverted every time that the velocity of rotation will permit it. The theory of this species of magnetism appears mature. The character which distinguishes this, does not consist in momentary duration only, which it has in common with soft iron, but also in being a double magnetism, *inverse* and *direct*; inverse at the moment of its production, opposite to the producing cause; *direct* at the moment after, when this cause disappears."

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* Nobili here speaks of his own researches, published in Bib. Univ. xxxvii, 10.

Nobili and Antinori appear to have mistaken the sense of Dr. Faraday's letter to M. Hachette. They proceed thus:—"Mr. Faraday considers Arago's magnetism of rotation as entirely connected with a phenomenon which he discovered about ten years ago." "*He then ascertained that by the rotation of a metallic disc under the influence of a magnet, there may be formed, in the direction of the radii of that disc, electric currents in sufficient number to render the disc a new electric machine.*" "We are quite ignorant how Mr. Faraday has ascertained the fact; we do not know "how a result of such a nature could remain so long a time generally unknown, and, as it were, lost, in the hands of the author of the discovery. Besides, there is here something very problematical to us, and before we leave the subject we will describe the experiment we made relative to it.

"A disc of copper was revolved, and two long copper wires prepared, attached at one set of ends to the galvanometer, and, at the other, held by the hand against the disc, the one at the centre, and the other at the circumference, in the direction of the radii. In the rotation of the disc, the points of copper pressed against it will be heated, but unequally; that pressed against the circumference will be most heated, and that at the centre the least. This difference is quite sufficient to determine an electric current capable of moving the needle of the galvanometer, and retaining it after a few vibrations at a certain degree of the division. When the needle is thus quiescent, if a horse-shoe magnet be advanced towards the plate so as to embrace it without interrupting its motion, it will be seen that the deviation of the needle will augment or diminish according as the poles act in the one direction or the other. The effect is a sure proof of the current manifested in the disc by the action of the magnet; but because the wires connected with the galvanometer are arranged with their ends in the direction of the radius of the disc, are we to conclude that they are exactly in the direction in which the current excited by the magnetism exists? We do not believe it, and though we should, with Mr. Faraday, admit this species of irradiating currents, there would still exist for us a great difference between this mode of exciting electricity, and the ordinary one of our common electrical machines. There is here a great void to fill, in passing from a superlative conductor, like the metallic disc of M. Arago, to the worst, such as the glass plate of an ordinary machine."

The words of M. Hachette, in the French translation of Dr. Faraday's letter in "Le Lycée," No. 35, are these:—"M. Faraday considère le phénomène qui se manifeste dans cette expérience, comme intimement lié à celui de la rotation magnetique qu'il a eu le bonheur de trouver il y a dix ans. Il a reconnu que par la rotation du disc metallique," &c. &c. The phrase used by Nobili and Antinori, in the body of their paper, is "Egli recourbe fin d'allora che," &c., whilst that which they used at the head of the paper, is "Egli ha reconosciuto che," &c. &c. It was in consequence of the recent researches that Dr. Faraday ascertained the state of the revolving plate, and could then refer the effects in its kind to that which he had so long before discovered. The experiments and speculations of the Italian philosophers must be acknowledged to possess the highest interest, but they are strikingly inferior in point of precision and severity of abstraction, to those of our talented countryman, to whose labours in the evolution of terrestrial electro-magnetism we shall shortly recur.

THE TREATMENT OF CHOLERA

By the Injection of Saline Solutions into the Veins, not warranted by the Pathological Condition of the Blood.

By JAMES M‘CABE, *M.D., Cheltenham.*

OF the several modes of treating cholera morbus which have from time to time occupied the attention of the medical profession and of the public, the treatment by the injection of saline solutions into the veins, appears to have attracted most the attention of the profession. Whether a remedy be found out by fortunate chance or scientific deduction, the medical faculty are equally bound to avail themselves of the discovery; but a remedy laying claim to scientific foundation, is sure to meet with greater favour, and is more likely to be adopted without sufficient inquiry, than that which originates in mere experiment. Having premised these general observations, let us now inquire how far the treatment by saline injections into the veins is warranted by, or deducible from, the pathological condition of the blood in cholera.

Chemical analysis has shown that, in the blood of patients labouring under cholera, there is a deficiency of its watery and saline parts; and Dr. Latta states, that an article published in THE LANCET by Dr. O'Shaughnessy, in which this pathological fact is stated, first suggested to him the idea of treating the disease by restoring to the blood its serum and saline particles. This he first attempted to do by saline solutions given by the mouth, and also introduced by the rectum; but finding this plan unsuccessful, he had recourse to the more direct means of injecting it into the veins.

We find in all the analyses that have been published on the subject, that the deficiency of the watery and saline parts of the blood, in cholera, is referred to the profuse discharges which had previously taken place from the stomach and bowels. Now these discharges from the blood must be occasioned by some morbid condition of the system; and we can scarcely suppose that that morbid condition can be remedied by merely replacing by injection into the veins what that morbid condition had allowed, or probably had caused, to be effused. If we adopt, in our explanation, the humoral pathology, and suppose the cause of these discharges to be the state or condition of the blood itself, we must also adopt the language of the older physiologists, and refer it to an acrimony of the blood and humours. What this acrimony can be, unless it be an excess of saline particles in the blood, it is difficult to imagine; and if so, and if the discharges are occasioned by the vellication of the coats of the arterial exhalents by the saline spiculæ of the blood, it is evident that saline injections would be pathologically contra-indicated. If, on the other hand, we refer the immediate cause of these discharges to the pathological condition of the solids—that is, to the state of the containing vessels—we meet with the same difficulty; for we cannot expect to remedy the relaxed state of the solids by merely refilling the vessels with a fluid analogous to that which they had already effused.

It is, therefore, evident, that in founding the treatment by saline injections on the pathological condition of the blood in cholera, an effect has been taken for a cause, and made the basis of a practical deduction. The loss of the watery and saline parts is the effect of some antecedent cause, over which the return of them again into the system by injection can exert no remedial power. Could it be shown that an excess of salts in the blood was the cause of the profuse discharges, the injection of pure water, of the temperature of the blood, into the veins, would probably be beneficial by diluting the thickened crassamentum; but if the effusion from the exhalent vessels be the effect of diminished vitality of the solids, no benefit would be likely to result from any injection by the veins, as it would pass through the same channels.

There are many circumstances in the history of cholera that would favour the opinion, that an excess, and not a deficiency, of the watery and saline parts of the blood, is a predisposing cause of the disease. In the first place it attacks most frequently the poor and ill-fed portion of the population, more especially those whose constitutions are broken down by disease or intemperance;—individuals labouring under organic obstructions, and in whom dropsical effusions might be expected to occur, from which, in fact, the discharges in cholera differ only with respect to the situation in which the effusion takes place; for were the fluid which in cholera is poured into the alimentary tube, to be effused into the cavity of the abdomen, or cavities of the pleuræ, it would constitute ascites or hydrothorax. We know that the frequent use of aperient medicines, whether drastic or saline, has the effect of draining off, through the mucous membrane of the bowels, a portion of the watery and saline parts of the blood. We know that among the better classes of society in this country, considerable attention is paid to the state of the bowels; in fact, that aperient medicines are frequently taken, and consequently a considerable part of the watery and saline constituents of the blood removed in that way; while among the poorer classes, less attention is paid in this particular. We know that the French, generally, as a nation, are very much averse to the use of aperient medicines, and in fact seldom or never take any. In France the cholera has attacked the rich and the poor indiscriminately;—in this country its ravages hitherto have been very much confined to the poor. In France, the rich and the poor systematically and intentionally avoid taking aperient medicines;—in this country, the poor generally neglect to take them. In the progress of cholera hitherto through both countries, are we merely to consider it as a singular coincidence, or are we to view it in the relation of cause and effect, that the disease has principally attacked those persons who habitually or systematically neglect the use of aperient medicines?

From the latest reports published on the subject of the saline injections, it would appear that excruciating pain frequently follows the use of the injection, particularly when the fluid is again making its way into the stomach and intestinal tube.

From the various facts and circumstances here stated, and the inferences which appear to me to be fairly deducible from them, I am of opinion that there is no deficiency, but probably an excess, of the watery and saline parts of the blood in cholera, previous to the discharges which accompany the disease; and, consequently, that the treatment by saline injections into the veins, if found successful, ought to be referred to fortunate experiment, and not to the pathological condition of the blood.

UNSUCCESSFUL TRIAL OF

DR. STEVENS' SALINE TREATMENT

IN MALIGNANT CHOLERA.

To the Secretary of the CENTRAL BOARD OF HEALTH.

Rochester, July 21, 1832.

SIR,—Being strongly impressed with the hope that the chemical pathology of the blood, as shown in Dr. O'Shaughnessy's able analysis, would lead to a more rational and certain mode of treatment in this disease than any hitherto laid down; and, at that time being strongly excited by the simple mode of treatment recommended by Dr. Stevens (on this principle), and lauded by him with such praise and success in his practice in the Coldbath-Fields Prison, was determined to give this remedy a fair and unbiassed trial on the first opportunity that should occur.

An opportunity soon presented itself, by my having charge of an hospital ship attached to the convict establishment at this port, where the cholera had assumed a very malignant character.

Three cases were selected for trial, of the admissions during one day.

Case 1.—The medicine was given every hour, as recommended by Dr. Stevens, together with dry heat, frictions, mustard poultices, and injections of hot salt and water; but notwithstanding the most unremitting attention of myself and assistants to the faithful exhibition of this remedy, he died in about twenty hours after admission, or after he began to use the medicine.

Case 2.—Used the same remedy as in Case 1, immediately upon admission; died forty-eight hours after he commenced to take the medicine. Neither of these cases showed the least symptoms towards reaction during the whole period under treatment.

Case 3.—Was likewise put under the same mode of treatment as the two preceding cases, but observing them sink so rapidly under the saline treatment, and believing, from much experience in this disease, both in India and in England, that I had superior remedial powers at command, the saline treatment was accordingly laid aside. A mustard emetic was immediately given; bleeding by leeches from the region of the heart and præcordia; ten-grain doses of calomel, five of capsicum, given every hour; the mustard sinapisms, dry heat, and a diffusive stimuli, given as occasion pointed out, with a table-spoonful of cold water, as the craving for drink was excessive. The salt-water enema was continued, with an addition of ℥j of the ol. terebinthinæ to each; by which treatment reaction was soon established, and he is now at duty.

From my own practice and experience, and in that of some of my friends, of the saline treatment, I have no hesitation in stating my opinion that it is a remedy, *per se*, totally inert in the collapsed stage of cholera.

I can, however, state, that the hot saline enemata, when combined with the ol. terebinthinæ and the mustard poultices, as recommended by Dr. Stevens, are most valuable remedies in the stage of collapse from cholera. I have the honour to be, Sir,

Yours obediently, &c.,
JOHN ANDERSON, M.D.,
Surgeon, R.N.

Extract from another letter from Dr. Anderson, same date:—

During the last week I have had twelve boys in a complete state of collapse; they have all been treated under the plan pointed out, and I am happy to say that reaction has been established in all of them, with only one exception; and even this case, I trust, will still do well. It is rather singular, but true, that I have as yet lost no case where reaction has been once fairly established.

(Signed) JOHN ANDERSON.

BARBADOES NAPHTHA IN CHOLERA.

To the Editor of THE LANCET.

SIR,—May I be permitted through your talented publication to offer to the consideration of the medical profession, the probable advantages of employing Barbadoes Naphtha as a preventive against cholera, on the grounds of its being a compound simply of two elements, *hydrogen* and *carbon*, and containing a greater portion of *pure* carbon in a fluid state than any other known natural substance, the proportion of carbon being 85 per cent, whilst the greatest proportion of carbon in each of the subjoined active medicinal products, obtained from the most powerful vegetable tribes, is, " in brucine only 75 per cent, morphine 71, narcotine 68, emetine 64, and quinine 74." Thus demonstrating that the great proportion of carbon, in a soluble form, is the *principle* which accounts for the superiority of this highly curious product, over all other medicinal preparations, in particular cases, and its possessing the peculiar property of counteracting all morbid decompositions.

By a chemical analysis we are instructed, that a *drachm* of this natural product contains 52 grains of carbon, and which can be progressively increased to 1000 grains in

24 hours, whilst in an equal quantity of any other substance there are only four or five grains of carbon that could be safely exhibited, and which would be *inefficient in quantity* to counteract by its preventive powers the introduction of infectious miasma into the constitution.

I am, Sir, your obedient servant,
MEDICUS.
18th July, 1832.

REPLY OF MR. COSTELLO TO THE LETTERS PUBLISHED IN DEFENCE OF BARON HEURTELOUP'S NEW MODE OF BREAKING STONE IN THE BLADDER.

To the Editor of THE LANCET.

SIR,—In transmitting to you for publication in your valuable journal, a translation of my two letters to the President of the Academy of Sciences, I thought I had placed the object I had in view, namely, scientific and useful discussion, in such a clear light, that my motives could neither be misconstrued nor misrepresented. It is not my fault that it is so, and I sincerely regret it. In those letters I declared, as I do now, that I only sought for truth. I have no feeling, no object, in common with those who are interested "in making the worse appear the better reason." I have said then, as I repeat now, "Prove to me the value of your proposal, and I will freely acknowledge it;—show me the justness of your pretensions, and I will recognise them." There was surely nothing in this which could have caused it to be looked on as an improper demand. The truth must have been arrived at by the discussion into which it naturally led. There was nothing to call for a display of petulance or bad temper.

I am sorry that it was deemed advisable to assume so acrimonious a tone in replying to what are called my calumnies. No respectable member of our profession can applaud such a course. Respectful, is quite as effective as intemperate, language, to maintain the truth of our opinions. Both from feeling and habit I am averse to the use of harsh words, and I will not, therefore, allow myself to be betrayed into an imitation of the tone or spirit which breathes in some of the justificatory and reprobatory letters addressed to me in this week's LANCET, in behalf of M. Heurteloup. Angry controversy I will have nothing to do with, if I can avoid it. The discussion which sinks into vulgar abuse, and which employs no better arguments than offensive personalities, I cannot condescend to prolong. It is at once foreign to and beneath my object. With these observations, it would, perhaps, be right to discuss these letters altogether, and I am sure every respectable member of the profession would approve of this course. Still, the fear that some well-meaning persons might form an erroneous opinion of me from these misrepresentations, induces me to take the trouble of replying to them.

Before I come to the letters, I shall take leave to introduce a few remarks. Since the first establishment of lithotrity as a surgical operation by my colleague M. CIVIALE, many persons (and I say it without disrespect) have laboured for an easily-acquired celebrity in connexion with it, by alterations or additions made in the construction of the instruments. No person has toiled more sedulously for this purpose than M. Heurteloup. In the infancy of lithotrity, *brise-pierres* or stone-breakers, with two branches, were proposed, and sometimes used. M. Heurteloup produced one of these. Four-branched pincers were also used; of these M. Heurteloup produced a modification. He also proposed moveable or complex drills, the parent idea of which, with various models, was already in existence; and, lastly, the curved percussor, copied from Weiss's, or even an older instrument. Now be it remembered, these instruments followed each other in regular succession, and as regularly were they laid aside. The appearance of any one of them was always deemed a legitimate opportunity for some oblique reflections on Civiale's instruments. Despite of the good Civiale's did, and are still doing, they were always on those occasions sure to be reviled. Each of these instruments in its turn was lauded *as the "ne plus ultra" of surgical power in lithotrity*, and in its turn was supplanted by another specimen, borrowed, or indigenous of this author's fecundity. It has long been my intention to offer to the profession a reasoned critique on the mechanism and action of this system of instruments. I shall now do so with as little delay as possible. With regard to the curved percussor, I have some remarks to offer, which shall be communicated in a separate paper.

The first of those letters is from Mr. Edwards. It may be disposed of in a few words. It commences by an allusion to the postscript of a paper published in THE LANCET a little time before. This postscript was merely a propitiatory version of the mishap which occurred through the use of the percussor. In a note, he says, "It was a first essay, and therefore little better than a model" (strange admission). Since then, he adds, "Sixty-two have been made, each more perfect than its predecessor; and the weight of the hammer being determined, and the probable resistance of the stone calculated, the whole strength of a man may be applied in striking, without any fear of the beak giving way." Mr. Edwards, it will be perceived, has no mean opinion of man's credulity.

A certificate from Messrs. Brodie and Hume is given with this letter. It is unnecessary to say anything more respecting this certificate, than that it should be placed at the head of my second letter to the Academy. It will be found that they say precisely the same thing.

Next is a letter from Mr. Goodwin, in which this gentleman very quietly accuses me of that of which he is himself guilty—namely, of making statements not founded in fact, and to which, in his regard for truth, he says he feels bound to offer a positive contradiction. Let us put this gentleman's love of truth to the test: he states, " that having heard that there was a safer mode of cure than the use of the knife, practised by two gentlemen in the profession, both strangers to me, I inquired about it, and was recommended by a friend* to Mr. Costello. Upon this circumstance being known to Mr. Brodie and Mr. Morrat, my medical attendants, they both, *for reasons best known to themselves, withdrew*, and I was left alone waiting the return of Mr. Costello to town."

There is an obliquity about this sentence evidently designed to favour the inference that Mr. Brodie would not meet me in consultation, from which an honourable mind recoils. What then must we think of Mr. Goodwin's statements, when this first of them is unjustifiable either in spirit or fact? I felt it my duty, on reading this paragraph, to call on Mr. Brodie. I pointed out the phrase, he read the whole letter. He said "I am at a loss to conceive how such an unwarrantable use could be made of my name. I saw Mr. Goodwin sometime before he sent for you. I told him what I had heard on former occasions, both from yourself and M. Heurteloup, that where there was paralysis of the bladder, it was not a favourable case for your operation. This I mentioned to him, and I heard nothing more about it till long after you had seen him. I saw him afterwards with M. Heurteloup, upon the invitation of Mr. Goodwin, which I should as readily have done with you, had I been invited. I will not be the partisan of any person." He added, "This seems to distress your mind, and you are to blame for it. I should take no notice whatever of these attacks, and you will find out, by-and-by, that they cannot in the slightest degree affect your success in your profession." When I experience such kindness and receive such advice from a gentleman so deservedly distinguished in science, I can well abate the anger which I may have first felt at Mr. Goodwin's ungenerous insinuation.

Mr. Goodwin states, that neither he nor Mr. Hodges ever saw any portions of stone voided—in fact, they believe that I never even felt the stone. Then, let me ask, what was it that was detached from within the bladder, and voided by the sound? What was it we caused to be dried on paper? And what was it that induced me to give him a hope of being relieved without the intervention of any serious operation, if it was not this sabulous or chalky matter which was detached and voided? It would have been singular, indeed, that I should have proposed to use Hunter's forceps to dislodge the stone, which, by the way, would have been as effective for that purpose as the percussor, if I had never felt the stone, and ascertained its position.

Mr. Goodwin further states that three sittings *of a quarter of an hour each or more*, and not two of five minutes, did take place. Out of thy own mouth shalt thou be convicted. The following is copied from a letter from Mr. Goodwin to me, after I had ceased to see him.

> "Dear Sir,—Having *twice* borne the operation of the lithotrite, I trust I have given you a fair opportunity of exercising your skill with success, if mine was a case which came within the range of your instrument; but as I cannot bring myself to think so, or to undergo another trial, I take leave to say that I must decline doing so. I remain, dear Sir, &c.,
>
> "GEORGE GOODWIN.
>
> "Aug. 31."

Now perhaps Mr. Goodwin will be able to recollect which of us is right as to the number of attempts. What would Mr. Goodwin think of four unsuccessful attempts in one case, and five in another, which M. Heurteloup acknowledges that he made? With respect to the quarter of an hour's duration, I have only to observe that both M. Civiale and myself make it a rule never to prolong the sounding or lithotrising beyond from three to seven or eight minutes; we advise postponing the operation, rather than the bladder should be too much fatigued by sounding, for the state of the organ should always be respected. Now I have performed lithotrity in private practice as well as in public hospitals in England, before some thousand surgeons and others, and I defy any one to say that I have acted against this maxim in a single instance. I cannot admit, even for Mr. Goodwin's love of truth, that I departed from it in his case.

Mr. Goodwin is quite deceived, when he says I proposed to call in M. Civiale only.

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* Mr. Bowdery, of Oxford Street, on whom I operated two years ago.—W. B. C.

When Mr. Goodwin declined this, I mentioned M. Heurteloup, and begged him to understand that I had no objection to his consulting him, that I wished not to be considered as standing in the way of his doing so.

At the second attempt, I proposed by means of Hunter's forceps to dislodge the stone, which I distinctly felt in the situation described, beneath the neck of the bladder, and from which it could not be removed, although the bladder contained a pint of injected water, and the position of the pelvis was raised, and varied from side to side. I left him to make up his mind on this matter; some days after, I received from him the above-mentioned letter.

Mr. Goodwin's gratitude to M. Heurteloup is a very proper feeling, I leave him to indulge it; his love of truth is quite another thing, and as it concerns me, I have taken the liberty of setting him right.

The next letter is from Mr. Briggs. It would be a waste of time to consider any communication on a subject of surgical science, emanating from a young gentleman, who has but just entered the profession. To animadvert seriously on anything which he pretends to utter of his own knowledge, would be breaking a fly upon the wheel. But this young gentleman is merely the cat's-paw in this matter; of this, there is sufficient internal evidence in the letter bearing his signature.

Before I come to the statements contained in this letter, I shall say a few words on his English. He begins, "The letter inserted by Mr. Costello in last week's LANCET, being evidently not written in a scientific point of view." As well might this unfledged controversialist talk of a letter written in a sun-beam; and again with risible solemnity, he says, "As Baron Heurteloup's pupil, *which* great advantage has enabled me to become acquainted with all that relates to his operations, &c." Make out who can the antecedent to *which*? it may be *Baron*, or *pupil*, or *As*, aspirated of course. But as I prefer an antagonist more worthy of me, I shall put you aside, Mr. Briggs, with a recommendation to go to school. Indeed you seem to want it; and notwithstanding you say you consider it your duty to take this upon yourself against your patron's wish, I shall persist in regarding your interference as both his work and his wish too.

After alluding to my letters to the Academy, with the flippancy congenial to ignorance, he asserts that those letters were never read in that learned assembly. With you, Master Briggs, I must deal decisively. I received a letter from the Secretary of the Academy, of which the following is a copy:—

> "Institut de France, Academie Royale des
> Sciences, Paris le 23 Avril, 1832.
>
> "Le Secrétaire perpetuel de l'Académie à M. Costello, à Londres.
>
> "Monsieur, l' Académie a reçu la lettre que vous avez bien voulu lui adresser sur les nouveaux instrumens de Lithotritie de M. Heurteloup. J'ai l'honneur de vous prevenir, *qu'apres en avoir entendu la lecture*, l' Academie l'a renvoyée à l'examen de MM. Boyer, Dupuytren et Serres, commissaires déja nommés pour prononcer sur les instrumens du chirurgien Français.
>
> "Agreez, Monsieur, l'assurance de ma consideration distinguée.
>
> "(Signed) ARAGO."

My second letter was also read in the Academy at the sitting of the 18th June, of this he will find a proof in the *Messager* of the 1st instant. What think you now, Mr. Briggs, of the accuracy of *your* statements? If you understand French as well as you write English, and that will not be saying much for you, you will see that you have been misinformed with regard to the reading of my letters in the Academy. You indulge in a sneer at the character of those letters. With lips consequentially pouting and swelling with your important dictum, you exclaim, "O they were not read in the Academy, they were not scientific, they were mere *verbiage*." It certainly is consistent in you to say they were not scientific, for you find it convenient not to take the least notice of the scientific details contained in those two papers, which were listened to with profound attention by the Academy. Of yourself, you are unequal to the task, though you assume the capacity of giving an opinion on the subject. You eschew the scientific parts of those papers, and carefully dwell on those which form a good ground-work for a *puff*. You are too clever by half, Mr. Briggs. With respect to the case of Captain W——, this young gentleman intrudes on me with his advice. I should forsooth have taken my information from Mr. Money or Mr. Green! Does he suppose that I have nothing else to occupy my time, than going about amongst my medical brethren, to ferret out the particulars of the cases treated by his patron? If he had been imbued with that candour for which I was willing to give him and his patron credit, he would have published in the article I am now occupied in answering, the letter which I addressed to Mr. Money on this subject. Such a course would have been manly and professional; but it would not have suited the left-handed policy of my antagonist. Here is the letter in question:—

"To ——— Money, Esq., 3, Hanover-street,

"My dear Sir,—A journey into the country yesterday prevented me from sending you the explanation which you required of me the day before. I could not under any circumstances refuse it, and less so when I consider the conciliatory and handsome manner in which you asked it from me. I therefore do not hesitate to avow, that I am opposed to the interpretation which M. Heurteloup supposes might be made of my statement of the case of Captain W. The only object I had in view, in my letter to the President of the French Academy, was to correct a mistatement made to that body, of the results of my practice in England. It was asserted that M. Heurteloup had succeeded with the curved percussion instrument, in two cases, in which I had previously failed. In order to prove the incorrectness of this statement, it became necessary to enumerate the cases in which M. Heurteloup had operated, after I had been previously consulted; this I did. I gave credit for one case, but I withheld it in the other. My defence before the Academy was called for, and I think I was bound in justice to myself to make it; and although this was the second trespass upon my reputation as an operator (I allude to the misrepresentation of the results of my practice, made in a letter of Mr. Alcock nearly three years ago), the tone of my letter was by no means acrimonious. It was even respectful towards the individual, although condemnatory of the instrument. I trust, whenever I am forced to figure in scientific controversy, that I shall always know how to maintain my opinions by fair argument, without degrading myself or my profession by vulgar or low abuse of persons.

"I do not think that we are in a condition to dogmatize upon the results of surgical operations, or that we are justified in saying, that because such a surgeon has operated, or such a mode of operation has been resorted to, or even that such an accident has arisen in the progress of an operation, that therefore the patient has lost his life; this being my opinion, how should I favour the interpretation, that because M. Heurteloup operated, therefore the patient died? I should be base, indeed, if I could act in this manner. This would be the logic of a furious partisan, and not of a sincere investigator of truth. This is my conscientious opinion. Now, I go farther, and state that if I were even convinced that the operation of M. Heurteloup, or any other man, was the cause of a patient's death, I should not say it. I will not, therefore, suffer such an interpretation to be put on my words, not only because it would be unjustly injurious to M. Heurteloup, but because it would be injurious to myself. Thanking you once more sincerely for your kindness on this occasion, believe me to be, dear Sir, yours, &c.

"W. B. Costello."

"7, Parliament-street, June 29, 1832.

With respect to the case of Mr. Stockbridge, he says, "he need make no comments on it; it speaks for itself." If he had said no more about it, I should have deemed my letter sufficiently long, and dismissed it also. As he has been so imprudent as to speak of it, I shall soon convince him that the less he said about it the better it would have been for the cause he attempts to advocate. "A straight instrument," says he, "could not be introduced into the bladder without violence, which, in this operation, above all others, should be avoided." Although Mr. Briggs is but a meagre authority on this matter, I agree with him, as to the necessity of avoiding all violence. *Sed cecidit in foveam quam fecit*; here he has dug the argument from under his feet. Let us compare what occurred to this patient under our respective treatments. M. Heurteloup introduced a straight instrument once, and attacked a stone, part of which was voided. During nearly six months following this sitting, many others were attempted, but they all failed. These attempts at introducing the instruments were productive of great distress, and were followed by swelling of the parts, by sanguineous infiltration, and leeches were frequently applied to combat the irritation. These attempts were at length abandoned, the impossibility of using a straight instrument was announced, founded on a presumed enlargement of the prostate, and a curved instrument was proposed.

I was at this time invited to see the patient, and although it is insinuated that undue means were had recourse to to bring this about, I declare that this insinuation is utterly unfounded. As I stated on a former occasion, I introduced the straight instrument twice, without violence, and without difficulty: upon the first occasion, to ascertain if it was possible; upon the second, to attack the stone. Upon neither of these occasions was there a drop of blood shed; upon neither was the injected water tinged; upon neither was pain given, upon neither did pain, swelling, or difficulty of making water, supervene. After the sitting, the fragments were voided without difficulty, and the patient resumed his chair as before. Now will Mr. Briggs answer, if any violence were used at all, against which of us will the evidence of it be the strongest? I did not say that M. Heurteloup used violence, I said he found it impossible to introduce the straight instrument. Mr. Briggs retorts and says, "if I succeeded where his patron failed, it could only have been by prevailing by the force of my hand and a metallic instrument over the soft and yielding organs; *for the Baron Heurteloup's acknowledged skill and talent could not have failed* in the usual application of this instrument." Harsh words are not necessary, neither shall I make use of them to wither the man, who could have written the preceding sentence. It comes to this pitch of arrogance,—that because M. Heurteloup failed to do this, no one else could!!!

Happily for me, I have in my possession the means of protecting myself against this malevolent insinuation. In the account of the autopsy, I stated that I preserved the parts; a minute's inspection of them will suffice to show that *the urethra was healthy and the prostate but very little enlarged.* I did not on the former occasion give the document sent to me from the medical men at full length, I shall now add a few lines which are appended to their account:—

"We the undersigned were present at the lithotritic operation performed by Mr. Costello on the late Mr. J. Stockbridge, and certify that it was performed with the greatest gentleness and skill, and with little apparent suffering to the patient. The time occupied was about three minutes.

"Signed, "WM. C. CARVER.
"WM. NUNN, Surgeon.
"Royston, May 4, 1831."

This document was sent to me inclosed in the following letter.

"My dear Sir,—Inclosed is the document required. I should say to you, as a friend and well-wisher, publish the whole case, and I am quite sure it will tell to your advantage, and the sooner the better. You have nothing to suppress; everything was done that could be done agreeably to the present state of knowledge of your art. With best wishes for your success, believe me, my dear Sir, yours truly,

"WM. NUNN."

With the account of the autopsy, and a few reflections which I shall now add, I think we shall have this case fairly before us.

It is an object to remove the stone from the bladder as quickly as circumstances will safely permit, not only because when it is in our power we should restore a sufferer to health with the least possible delay, but because, in the case we are considering, the evils arising from reflected irritation, are the great source of danger in this complaint. As it is admirably, and in accordance with physiological principles, laid down by Sir C. Bell, the urinary organs have throughout their different parts elective sympathies with each other; by elective sympathies we mean, that one portion of this apparatus will call another portion into a participation of its excitement or irritation, in preference to all other parts; thus the neck of the bladder, and the external orifice of the urethra, are in close sympathy, and whatever annoys the one is sure to involve the other in community of suffering. This is shown in common blenorrhagy. The orifice of the urethra is alone the seat of disease, yet micturition is frequent, and the neck of the bladder is the seat of different sensations of fulness, of tingling, or pain. Again, if the neck of the bladder be irritated, the orifice of the urethra becomes a large and faithful sharer of its distress. If the bladder be the seat of suffering, the sympathies of the kidneys are awakened; these organs are strained to overaction, excitement proceeds to sub-acute inflammation, enlargement, and alteration of their tissues, is the consequence, and thus the irritation of the bladder, which was the only disease primitively existing, by exciting the sympathies of another portion of the urinary apparatus, hurries it likewise into disease, and life is destroyed, not by the alteration which disease effects in the first affected organ, but by that which it induces in another organ of greater importance to the animal economy. An acute and sudden attack of nephritis is productive of the keenest suffering, while the alteration which is silently going on in the kidneys, the effect of sympathy with another portion of the urinary apparatus can scarce be suspected, much less diagnosticated. Pathological investigations fully authorise the foregoing observations. Now, from the autopsy of this case we find that death was caused by the disorganization of the kidneys. Was my operation, which was not painful, and which, locally, was followed by a soothing effect, the cause of this disorganization? No candid, no well-informed surgeon, will think so. How then does this case prove the utility of a curved instrument? The stone was broken by a straight one, and the cure would have been perfect if the kidneys had not been diseased!!!

Mr. Briggs says, in the statement made in the French Academy as to the cases in which his Patron was successful with the percussor, I was not the operator alluded to. I am fully aware that attempts have been made by other surgeons with the lithotrite, but is M. Heurteloup so uncandid as to conclude that because surgeons without adequate experience give lithotrity a trial, that their failures are to be ascribed to the inefficiency of the lithotrite? Dupuytren, Roux, and many others, have failed with this instrument in cases where Civiale was afterwards successful. If he did not allude to me in the Academy, he ought to have said so distinctly.

After this comes a summing up under separate heads. 1st. He says it is impossible that I can have treated successfully with the instrument which he improperly calls *pierce-pierre* (for it crushes the stone as well as perforates it), the number of patients I name, because, adds he, it is natural to suppose, if I had effected these cures, I should have published them in detail.

This is tender ground for M. Heurteloup's advocate to tread upon.

A short time before M. Heurteloup came to this country, he stated to the Academy, without futher details, that he had operated in *twenty-three cases*. On the cure of his first case in London, he stated that it made his *forty-third* case. Of his French cases thirteen only are published, some, or most, of them under simple initials, Am I therefore to conclude he has not had forty-two cases in France? If I adopt the reasoning of his advocate, I ought to do so. I should not make this observation, if I were not forced to repel the doubt which it is so ignobly attempted to affix to my veracity. Persons most liable to deviate from truth, are often too apt to suspect the existence of their own failing in others. *Sed fas est ab hoste doceri.* He shall have my cases in full and honest detail.

2dly. According to him, I have never seen the curved instrument, &c. &c.

He will doubtless be surprised to learn that I have *seen it*, that I *possess* it, that I have *operated* with it, and what may grieve him still more to hear, that I have so modified, improved, and strengthened it, as to render it a safe and valuable instrument.

3dly. He says "It is difficult to conceive how Mr. Costello, in no way distinguished by scientific labours, and *comparatively* young as to experience, should, in so positive and unrestrained a manner, give his opinion on this operation," &c.

I fully concur with Mr. Briggs as to my being undistinguished in science, and of slender repute. However fortunate I may have been hitherto, or may be in future, in relieving the sufferings of my fellow-creatures, I shall always be ready to acknowledge the humbleness of my talents. It would be a curse to be endowed with but a moderate capacity, if vanity were allowed to exaggerate it into something stupendous. Still I must take leave, undistinguished though I be, to remark on the indecency of my being told so by a young man, who is still in the rudiments of the science. What he can mean by the words *comparatively young in experience*, I am not able to say. If the comparison be meant between his patron and myself, I must inform him that I have been longer at the profession than his patron, that I taught anatomy, ere yet his patron gave up his situation at the *Bureau de la Guerre* to study medicine, and that I have seen and done more of this operation than any other man living, Civiale alone excepted.

Lastly, he reflects on my delicacy for endeavouring to mar the rewards due to a professional brother; but he adds, when the facts are untrue (Are the facts untrue?) such conduct requires an appellation, which, out of respect, he refrains from applying. This is urged by the advocate of a man, who signalized himself by the acerbity of his *diatribes* against M. Civiale addressed to that same Academy, the effects of which were ten-fold more injurious to the author than the man against whom their shafts were aimed. Delicacy, indeed! When instruments long in existence are presented as inventions, accompanied by claims for reward; when such claims are substantiated at the expense of another's reputation, when theoretic perfections are urged forward, and practical defects concealed, it would be criminal in a man who humbly laboured in the cause of science and humanity, to withhold what he knew on the subject. Adopting the language of Mr. Briggs, I can say with reason, such conduct would indeed justify the appellation which he refrains from bestowing.

I have now done with this heap of letters. In replying to them, my greatest humiliation has been in finding myself compelled to descend to the tone in which they are written. I repeat it, a controversy which can only be carried on in terms of low personal invective, can have no bearing whatever on the objects I have in view. My profession of feelings on this point has been declared long since. The man who devotes himself to succouring his fellow-man, or to the improvement of his art, is respectable in my eyes. I feel towards him that interest, which community of pursuit and object inspires, and I wish him success as sincerely as I wish to be successful myself. From such a man I shall be ever ready to receive any useful suggestion, or real improvement. With such a man I can have no difference. But he who seeks fame to render it subservient to his cupidity, he who denies all merit in others to aggrandize his own, will ever be despicable in my eyes, and whatever he asserts, I must scrutinize before I can believe it.

I have the honour to be, Sir, yours, &c.

Wm. B. Costello.

7, Parliament-street,
Monday, July 16, 1832.

THE LANCET.

London, Saturday, July 28th, 1832.

We take the earliest opportunity of calling the attention of the profession to the fact that the *Anatomy Bill* has passed the House of Lords, and ere this, probably, by receiving the Royal Assent, has become the law of the land. Regarding the provisions of this bill, we have had the misfortune to differ from, we believe, the whole of our contemporaries, a large majority of the members of the legislature, and the Council of the National Political Union. Time, the great Arbiter in such affairs, must now determine between us, as events will soon prove whether this measure was founded upon a correct calculation of facts, and the best means of satisfying the wants of the community, or whether it was framed in conformity with an imperfect knowledge of the existing law, and with ill-conceived opinions concerning the policy to be pursued. For ourselves, we may state unequivocally that our views remain wholly unchanged; we objected to the bill at first, we object to it still, because we can prove that it is an irrational measure, as founded upon the evidence given before the Parliamentary Anatomy Committee. Besides, we have ever contended that the security of the public against *burkers* could only arise out of the enaction of a measure which should go to the total prohibition of *buying*, or *selling*, dead human bodies. With such a conviction most firmly impressed upon our minds, we have, on a variety of occasions, urged our objections against the bill which has just received the sanction of the House of Lords. When, however, those objections were made on another occasion, we were told, with some degree of assurance, that the bill did *not* render the sale of dead bodies legal; but scarcely had this declaration annoyed the ears of those who are better informed, than the Attorney General observed, in his place in parliament, that the bill *would* most certainly legalize the sale of dead human bodies. This point should be borne in mind.

In connexion, then, with this startling fact, let the members of the profession recollect what was the character of the evidence given against "resurrectionists,"—that is, against those persons who gain a livelihood by trading in the lifeless flesh and bones of their fellow-creatures. Were they not represented by Sir Astley Cooper, Messrs. Brodie, Lawrence, Green, and others, as the most abandoned of scoundrels? The worthy Baronet went even so far as to declare that, if they could *get anything by it*, they would not hesitate to make a subject of him at an hour's notice. How was this state of demoralization attempted to be explained? Instantly, by referring to the *trade* in which the resurrectionists were engaged; in truth, we should not be hazarding too broad an assertion by alleging, that the impression made upon the minds of the committee by the evidence given against the resurrection-men furnished a basis towards making them believe that any man who would *sell* a dead human body would not hesitate to murder a living one. Yet, now,—strange, unaccountable anomaly!—we have a bill, passed avowedly for the protection of the living, which renders it *legal* to carry on a traffic in the dead! The trade, then, is to receive a new impetus, and flourish more than ever! But we are informed that one member of the Government has said that the *conductors* of it will be changed, and that the public will derive security from the circumstance that under this Act of Parliament dead bodies will be sold chiefly by churchwardens, and overseers, and *medical practitioners.* Yes, one of the objects in passing this bill has been to transfer that species of commerce which has heretofore been carried on by resurrectionists into the hands of members of the medical profession! Insulting, audacious proposition! Is there, we ask, a surgeon in England, Ireland, or Scotland, who is so lost to every honourable feeling, to every sense of decency and propriety, as to plunge himself so low in degradation by taking up a trade the carrying on of which has subjected other men to every term of reproach that could be applied to the most abandoned of human beings? The cutting edge of the gross insult offered in this supposition and insinuation is, however, completely turned aside by the circumstance that, during the progress of the Bill, not a single medical gentleman has petitioned in *favour* of that measure, whilst all petitions from practitioners,—as well from societies as from individuals,—earnestly prayed that such a traffic should be wholly prohibited by some pungent and dreaded penalty. Here, then, we have at once a convincing and cheering exposure of the infamous calumny uttered against medical men by a member of the legislature.

In a *reformed* House of Commons the most respectable and useful of all the professions will not be thus unblushingly insulted.

Satisfied, as we are, after the most mature consideration of the subject, that this bill, if it be permitted by the new parliament to remain unchanged or unrepealed, will produce in various parts of the kingdom the greatest possible degree of excitement and indignation, we have thought it right, at this first opportunity, to caution medical practitioners against being led away by their pure and disinterested love of science into certain paths of intrigue explored for them by the combined labours of boroughmongering legislators and certain trading anatomists. If proposals should at any time be made, in any of the hospitals, jails, penitentiaries, or workhouses, for disposing of the dead bodies therein, we implore medical practitioners to put the bridle upon their good intentions, and neither directly nor indirectly make themselves parties to any such transfers. Of course there will not be found persons sufficiently ignorant or daring to even hint at pecuniary recompense for engaging in such an agency. Our remarks extend to acts of interference of every description and under all circumstances, knowing as we do from past experience, that that medical attendant who may give a certificate of death preparatory to the removal of a body, will be deemed not less culpable than the parties actually making the sale, because it will be presumed that he does it with a foreknowledge that such sale is actually to take place, and that he voluntarily comes forward to knock down the only effectual obstacle to its perpetration. We would call the especial attention of our readers to this circumstance, because we have been informed, on the very best authority, that several of those members of the House of Commons who voted for Mr. Warburton's Bill, did so on the ground that exposures would not be of frequent occurrence, as the sales would be always managed in the workhouses, through the intervention of the medical attendants. That the Act cannot continue as now framed is certain, but as it is far from being worth the ruin of even a single medical gentleman's character, we have thus hastily and briefly pointed out to the profession the snares which are set to entrap them. Dead bodies may now be sold "according to law," and whoever lends a willing hand to the transmission of human "remains" to the dissecting room while such a state of the law exists, must expect to be branded as a person who has taken up the cast-off trade of the resurrectionists, and to be designated a "body-seller," if not a "body-snatcher." We shall, of course, procure a copy of the Act the moment that it is printed in its amended form, and examine at length the whole of its details

Observations on the General Principles and on the particular Nature and Treatment of various Species of Inflammation. By J. H. James, Surgeon to the Devon and Exeter Hospital, &c. Renshaw and Rush: London, 1832. 8vo. pp. 565. Price 15*s*.

Works in which the great principles of the medical and surgical art are laid down, explained, and fully developed, are scarce with us. Professional literature in England is mostly patch-work. We produce but very rarely indeed good radical works on the science of healing. In fact, we fall far short of our continental neighbours in every kind of production to which the press gives birth connected with the diffusion of anatomical and chirurgical knowledge. While the plates of Albinus, Scarpa, Mascagni, and Tiedemann, contribute to the extension of correct anatomical knowledge in other countries, we content ourselves with humble, and frequently worthless *copies*; or with the production of few and far between originals, which seldom rise above mediocrity. The plates of Mr. Shaw, illustrating the anatomy of the nervous system, are the only examples of national excellence on a large scale to which we can point. In like manner the standard works on anatomy generally, and in this country, are the composition of French and German authors. Until very lately, the names of Soemmering, Bichat, Meckel, and Cloquet, were our chief authorities upon points of general and descriptive anatomy. Fife, indeed, was warmly supported by the nationality of the Scotch, but to an extent which his real merits by no means deserved; and we might have still remained dependent on foreign assistance, had not Quain, Grainger, and Craigie, supplied, briefly, the deficiency in this branch of education. Our surgical writers also have been equally limited, if not in authority and value, at least in the magnitude of the subjects concerning which they have treated. The name of John Hunter certainly is known and respected by the most humble and distant cultivator of surgical knowledge; but we have perhaps no work which has taken an extensive range into our art, if we except the dictionary of Samuel Cooper, by far the best production of the kind in any language.

It will not be disputed, that a correct knowledge of the theory and treatment of inflammation is the corner-stone of surgery, embracing a vast extent and variety of topics, and requiring for its elucidation the utmost practical skill, joined with great literary acquirements, and patient industry. Yet how few of our countrymen have applied themselves to the investigation of this important subject! The celebrated work of John Hunter is chiefly confined to physiological and pathological details; the more comprehensive production of Mr. Thompson has, for some reason or other, been allowed to go out of print.* We are gratified, therefore, to find that a surgeon, so well qualified for the task as Mr. James, has at length undertaken the task, and furnished us with a comprehensive treatise on the nature of inflammation, and the general principles of treatment applicable to its numerous varieties.

The ground-work of the present volume is a thesis to which, in 1818, the Council of the College of Surgeons awarded the Jacksonian prize—a fact, however, which is of no moment in estimating the volume, for a more incompetent set of judges it would be difficult to select with a view to determine the value of anything but the current coin of the realm, or to ascertain how many pounds sterling can possibly be got for a worthless diploma. Mr. James has taken advantage of a dozen years of additional experience to correct and enlarge many of the views expressed in his thesis, and to modify his opinions by the increased light which the labours of Andral, and a host of others, have shed upon the subject of which he treats. From the nature of the volume, it is obviously impossible for us to offer any analysis of its contents. We rest content, therefore, with bestowing our commendation on the work, for the general accuracy of the opinions advocated in its pages, for the clear arrangement of the contents, and for the many valuable observations scattered throughout.

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* A French translation of this excellent work may, we understand, be obtained for the small sum of nine shillings.

A Manual of Percussion and Auscultation, composed from the French of Meriadec Laennec. By James Birch Sharpe, M.R.C.S. London: Highley. 1832. 12mo. pp. 118.

This excellent little volume consists of a translation of the summaries appended by Meriadec Laennec to the last edition of the great work of his illustrious relative, with some essential corrections, &c., by the translator. The whole has been revised by Dr. Elliotson, a circumstance which is sufficient to stamp the character of the work. The translation is faithful and perspicuous, and, altogether, Mr. Sharpe has succeeded in placing a cheap, portable, and valuable manual, within the reach of every member of the profession.

VISCERAL MALFORMATIONS
IN
SMITHERS, THE MURDERER.

There is at present preserved in the Museum of the London University a very complete specimen of that form of anomaly which arises from a change of position, or displacement of the thoracic and abdominal viscera. It occurred in the body of Smithers, the man who was executed a short time ago for having set fire to his house in Oxford Street, and thus caused the death of three of the inhabitants. The displacement consisted in a transposition of the principal viscera of the chest and abdomen from one side to the other. The heart appeared to occupy the centre of the thoracic cavity and its apex, instead of pointing to the left side, pointed directly downwards; the arch of the aorta formed a curve from the left to the right side of the bodies of the vertebræ, and the thoracic aorta descended for a short way on the right side of the spine; it then became placed on the centre of the bodies of the vertebræ, and ran down in the median line to the third lumbar vertebra, where it bifurcated in the usual manner. The right carotid and subclavian arteries arose by distinct trunks from the right side of the arch and the left carotid and subclavian, united at their origin into one trunk, formed an arteria innominata on the left side; the vena azygos ran up along the left side of the bodies of the vertebræ, but we could not learn whether the thoracic duct was displaced in a similar manner, as it was not preserved in the preparation. It is, however, probable, from the analogy of similar malformations, that the duct retained its relation to the vena azygos. The subdivision of the lungs into lobes was effected by the malposition of the heart; the right lung was divided into two lobes, and the left into three. In the abdominal cavity the transposition of organs was not less remarkable. The stomach and spleen were situate in the right hypochondriac region, and the liver filled up the whole of that space usually occupied by the stomach. The œsophagus penetrated the diaphragm to the right side of the aorta, while the opening for the vena cava was to the left. The cava itself ascended by the left side of the aorta to the anterior auricle, and sent off the right renal vein which curved over the aorta in a very conspicuous manner to the kidney. There was no anomaly of the genito-urinary apparatus. These displacements of the viscera are by no means uncommon. Sometimes we find only the contents of the chest displaced; sometimes only those of the abdomen; and occasionally, as in the present instance, the viscera of the chest and abdomen are transposed, at the same time from the left to the right side of the body, and vice versa. Indeed we may assert generally, that there is no thoracic or abdominal viscus, which may not be found in a different position from that which it usually occupies in the human body. In some rare cases the heart has been so far removed from its normal position as to be contained in the abdomen. An example of this kind is recorded by Deschamps.*

An old soldier, who was compelled to retire from the service by repeated attacks of nephritic pains, at length sunk under a very severe attack of the disease. The body was examined by Mons. Deschamps, and to the astonishment of the spectators, he found the heart situate in the left lumbar region; an opening in the diaphragm gave passage to the vessels of the lungs, head, and upper extremities.

Although it may seem strange to a non-medical reader, that life is consistent with the position of the heart in the belly, the possibility of its recurrence does not rest on this single fact. In the year 1788, Ramel examined with the greatest care the person of a young girl ten years of age, who had been subject from her infancy to palpitations and bleeding from the nose. The most accurate exploration of the chest failed to discover any pulsation within this cavity, but the contractions of the heart were visible both to the eye and touch, just below the diaphragm. The girl continued to enjoy pretty good health for a considerable time.*

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* Observations sur un deplacement remarquable du cœur.—*Jour. Gen. de Med.* t. 25.

* Jour. de Med. Chir. et Phor. t. 49.

The literature of this country is rather scanty of information on the subject of monstrosities; we would refer our readers for a full and masterly account of every species of anomaly to the work lately published by J. G. Saint-Hilaire, entitled, *Histoire Generale et Particuliere des Anomalies*, &c. &c.

It is a work which every scientific man should peruse.

HYDRURET OF SULPHUR.

M. Thenard has lately published a very remarkable work on the hydruret of sulphur. This substance, obtained by Scheele, and examined by Berthollet and Berzelius, acquires great importance from the observations of Monsieur Thenard, who remarks, that perfect analogy exists between its properties, and those of oxygenated water.

The composition of hydruret of sulphur is very variable. M. Thenard found in it one atom of sulphuretted hydrogen, or its elements, and six or eight atoms of sulphur. To prepare, M. Thenard employs muriatic acid, diluted with twice its weight of water, and the hydruretted sulphuret of lime, obtained by boiling lime, with great excess of sulphur, in water. The acid being poured into a funnel, the aperture of which is corked, the sulphuret is afterwards very slowly added, taking care to stir the mixture continually. The hydruret of sulphur, which separates first, is more fluid than that which is afterwards obtained. Nothing is easier than to procure it in separate portions.

The hydruret of sulphur is liquid, but its consistence is very variable; it is yellow, sometimes with a tint of greenish brown. It whitens the tongue in the same manner as oxygenated water, and occasions a smarting sensation. It whitens and alters the skin rapidly. The colour of turnsall is immediately destroyed by it.

Its smell is peculiar and disagreeable. At 140° to 160° F. it begins to decompose into sulphur and sulphuretted hydrogen. When suffered to remain, it gives out from time to time bubbles of sulphuretted hydrogen, and sulphur only remains. Charcoal and several metals in powder disengage sulphuretted hydrogen. The same occurs with a great number of oxides, as the per oxide of manganese, silicia, the alkaline oxides; and what is very remarkable, potash and soda in solution produce the same effect.

The easily reducible oxides, as those of gold, silver, &c. occasion instantaneous ignition; the oxide is reduced, and water is formed.

All the sulphurets tend to disengage sulphuretted hydrogen. This effect is especially remarkable with the alkaline per sulphurets in solution; but then sulphur is deposited. Some organic matters tend to decompose hydruret of sulphur, but slowly. The action is rather stronger with animal substances; water and alcohol do not dissolve it; but it decomposes in them, though but tardily. Sulphuric ether dissolves it, and soon leaves crystals which appear to be sulphur.

Acids impart stability to the hydruret of sulphur; a little acidulated water is sufficient to produce this effect. In this case it not only does not decompose spontaneously, but the peroxide of manganese ceases to act upon it.

HYDRIODATE OF POTASH IN NEBULÆ OF THE CORNEA.

To the Editor of The Lancet.

Sir,—From the power which iodine possesses over the absorbents, I was induced to try a solution of hydriodate of potass in nebulæ of the cornea, and having used it in a variety of cases, I can confidently say that I have seen more benefit from it than from any of the usual applications.

I order half a drachm of the hydriodate of potass to be dissolved in one ounce of distilled water, to have the under lid depressed, and to apply a drop or two twice a day with a camel's-hair pencil; it occasions very little pain, and the person is able to walk about as usual in a minute or two after its being applied. Ulcers of the cornea also heal very quickly by using it.

In nebulæ of long standing, where the deposition has been very thick, after the outer edges have been absorbed, the centre often (from its density) remains obstinately fixed, and requires a stronger stimulus. I then apply, in the same way, either the liquor potassæ P. L., diluted with an equal quantity of distilled water, or the tincture of iodine, and by persevering in this plan, I have succeeded in many cases which before I should have considered *hopeless*.

Truly yours,
Edward Toole, M.R.C.S.L.

Bandan, June 13, 1832.

ADVANTAGES DERIVED BY THE SURGEONS AND ASSISTANT-SURGEONS OF THE ARMY, OVER THE SURGEONS AND ASSISTANT-SURGEONS OF THE NAVY.

To the Editor of THE LANCET.

SIR,—So much has been said and written upon the state of the medical profession in the navy, of late, that I have felt surprised mention has never been made respecting the superior condition of the medical members of the sister service.

It is unnecessary to say, that the requisite professional knowledge in the army and navy is identically the same; but the reward is widely different. The surgeon and assistant in the army, mess with their comparatives and superiors in rank in the army. As gentlemen and officers they are entitled to attend the King's levee, a *favour* only lately granted to the naval surgeon and assistant. It is not long ago (before the exclusive order was revoked) that an assistant-surgeon in the army said to an old naval surgeon, "You may be my senior, but not my superior, as you can't attend the levee."

But the principal object of this letter is to point out the superior pay and half-pay of the army as compared with the navy, and in doing so I shall merely select the surgeon and assistant from each service.

Regimental Surgeon.

After 25 years actual service....	£1	2	0
After 20 but under 25 years	0	19	0
After 10 but under 20 years....	0	15	0
Under 10 years	0	13	0

Assistant-Surgeon of the Army.

After 25 years actual service....	0	10	0
After 20 but under 25 years....	0	10	0
After 10 but under 20 years....	0	10	0
Under 10 years	0	7	6

"Surgeons in the navy shall be paid all rates according to their length of service (including not more than three years as assistant-surgeon or hospital mate)"; so that from the expiration of their three years as assistant, until they are promoted, which is generally, on an average, six or eight years, the intermediate time is a hiatus in their service.

	Full pay per diem.		
Surgeons of less than 6 years' service....	0	10	0
Surgeons of more than 6 and less than 10 years' service	0	11	0
Surgeons of more than 10 and less than 20 years' service	0	14	0
Surgeons of more than 20 years' service..	0	18	0
Surgeons of hospital ships of whatever length of service	0	18	0
Assistant-surgeons of whatever length of service	0	6	6

Contrast this with the army. But the half-pay of the *sister* service presents a stronger proof that one sister is a greater favourite than the other.

Excerpt from the Warrant of July, 1830.

34. "The rate of half-pay for a medical officer of the army shall in every case be determined by the service which he may have rendered on full-pay, and the circumstances under which he may be placed on half-pay.

36. "If a medical officer shall be placed upon half-pay, by reduction of establishment, he shall be allowed the half-pay to which his service may entitle him, according to the following schedule, viz.

"*Rates of Half-pay after a Service on Full-pay.*

Regimental Surgeon.

	Per Diem.		
Thirty years	0	15	0
Twenty-five but under thirty years	0	13	0
Twenty but under twenty-five yrs	0	11	0
Ten but under twenty years	0	8	6
Less than ten	0	6	0

Assistant-Surgeon.

Thirty years	0	7	6
Twenty-five but under thirty years	0	7	0
Twenty but under twenty-five yrs	0	6	0
Ten but under twenty	0	5	0
Less than ten	0	4	0

35. "If a medical officer shall be placed on half-pay from any other cause than reduction of establishment, he shall be allowed the half-pay to which his service may entitle him.

"*Rates of Half-pay after a Service on Full-pay.*

Regimental Surgeon.

	Per Diem.		
Thirty years	0	15	0
Twenty-five but under thirty years	0	11	6
Twenty but under twenty-five yrs.	0	8	0
Ten but under twenty	0	6	0
Less than ten	0	5	6

Assistant-Surgeon.

Thirty years	0	7	0
Twenty-five but under thirty years	0	6	0
Twenty but under twenty-five yrs.	0	5	0
Ten but under twenty years	0	4	0
Less than ten	0	3	0

"Every medical officer who may have served on full-pay for 25 years and upwards, shall have the right to retire upon half-pay at the rate assigned to his rank in the schedule subjoined to the 35th article, provided he shall have served three years in the rank from which he retires, but if he shall not have served three years in such rank, he shall receive only the rate attached to the rank from which he was last promoted.

"If a medical officer shall have served 30 years on full pay, including three in the rank from which he retires, or ten in the colonies, or five with an army in the field in any rank, or if his services be recommended under any special circumstance of merit, he may be allowed the rate of half-pay assigned to his rank in the schedule annexed to article 35.

"The rates of daily pay for the aforementioned ranks, are to be in future regulated by the length of time which the officers of each class shall have served on full pay, according to the annexed scale; provided always, that when any officer is hereafter promoted, he shall commence upon the minimum pay of his new rank, notwithstanding his length of service, agreeable to the said scale, may give him a claim to a higher rate of pay, as before he shall be allowed such higher rate of pay, he will be required to serve on each inferior rate of pay attached to his rank, for the following period, namely, one year if he had been in the medical department antecedently to the date of this warrant, and two years if he received his first medical commission subsequently to that date; but if the officer thus promoted had higher pay in his old rank than the minimum of his new rank, he shall commence upon that rate of pay which may be next above his former pay, and before he obtains any further increase, shall serve the period above prescribed, viz.—

"*Rates of Daily Pay subject to the above Provisions.*

Assistant-Surgeon.

After 25 years actual service ..	£0	10	0
After 20, but under 25 years....	0	10	0
After 10, but under 20 years....	0	10	0
Under 10 years	0	7	6

Regimental Surgeon.

After 25 years actual service ..	1	2	0
After 20, but under 25 years....	0	19	0
After 10, but under 20 years....	0	15	0
Under 10 years	0	13	0

Surgeons in the Navy.

	Half pay per diem.		
After 6 years service (as surgeons)	0	6	0
Under that time	0	5	0

Assistant-Surgeons.

Three years service	0	3	0
Two years	0	2	0

After three years specified, his pay and service are stationary."

"Can such things be without our special wonder?"

In what essential are the medical officers of the navy deficient, that they should be treated thus? In what point as a body are they inferior in professional acquirements to their army brethren? And why are they to be rewarded inferiorly? What is the use of granting a new uniform to men who feel that it is only an additional expense, without conferring honour or rank, since they are still classed with warrant officers?

After a life spent in the service of their country, in all seasons and climates, are they not at least entitled to a compensation equivalent to their army confrères? Why is the period of service from the expiration of the first three years, until promotion is attained, not granted, to the naval assistant-surgeon? These are questions easier asked than answered. In the hope of seeing at a future period the services of naval medical officers placed on a similar footing to the army, I take my leave, and remain, Sir, yours truly,

CHIRURGUS.

July 18th, 1832.

EXTERNAL APPLICATION OF THE TARTAR EMETIC IN COLDS.

To the Editor of THE LANCET.

SIR,—A short time since I caught a violent cold on the lungs, accompanied by inflammation of the tonsils, rendering respiration and deglutition very difficult; without applying the usual remedies, I dissolved half a drachm of the antimonium tartarizatum in one ounce of water, and rubbed fifty large drops of the solution into my chest and throat at bed-time, which was about eleven o'clock. I slept as usual till four in the morning, when I awoke, and the application seemed to have produced little or no effect beyond creating a slight diaphoresis, which continued till seven, and then entirely left me. On the subsequent night I applied about eighty drops of the solution, containing, as near as I could calculate, eight grains of the tartar emetic; after a short time there were heat, redness, and itching, and nausea supervened, and about an hour afterwards a profuse perspiration came on. I now sank into a profound sleep, and about seven o'clock the next morning awoke and got up, the difficulty of breathing and impeded deglutition very greatly relieved, and my general health considerably improved. After repeating the dose on the following evening the complaint entirely left me, and my health was improved in a manner which astonished me.

One case affords but weak evidence of the efficacy of a medicine; but I think the decided benefit of the ant. tart. in the above case must be acknowledged. It cannot I think be doubted for a moment that its external application (in certain cases) is highly preferable to its internal administration, particularly when used as an alterative. I intend, when I have an opportunity, to try its effects to a greater extent on myself. I remain, Sir, your obliged and humble servant,

R. Kilby.

South Lincolnshire, July 19, 1832.

LONDON HOSPITAL.

FEMORAL ANEURISM.

On Wednesday, July 4th, Mr. Andrews operated on W. R., ætat. 17, for femoral aneurism.

The lad stated, that about three weeks previous to his admission, while working at his business as a shipwright, he received a wound about the middle of the thigh from a chisel. The cicatrix is very evident; it is situated just about the middle of the thigh, over the femoral artery, where there is a pulsating tumour about the size of a small orange. On applying the stethoscope, the bruit de soufflet is distinctly heard. His general health is very good; appetite excellent; sleeps well; says that he feels no pain in the tumour, and that so far from experiencing any inconvenience from the aneurism, he should not know that any affection existed there, if he did not occasionally put his hand to the part.

On being placed on the table, and having compression made on the artery, some distance above the tumour, Mr. Andrews commenced by making an incision in the course of the vessel along the inner edge of the sartorious muscle. After exposing the femoral artery, there was considerable difficulty experienced in passing the aneurismal needle beneath the vessel, owing to the parts being obscured by a troublesome hemorrhage. After some time the tourniquet was applied, and ligatures were passed above and below the puncture; the artery was then divided between the ligatures. The edges of the wound were brought together, and retained by straps of adhesive plaster. On being removed to his bed, the limb was enveloped in tow.

On visiting him in the evening he was found quite comfortable and free from pain. The limb was of the natural heat, and the anterior and posterior tibial arteries could be plainly felt pulsating.

July 5. Passed a very good night, having slept very soundly for several hours; complains this morning of sickness of the stomach, with a hot skin and quick pulse; bowels not opened since yesterday. Was ordered the saline effervescing mixture, which has allayed the sickness, and he feels quite comfortable; the limb is rather hotter than its fellow. The pulsation in the arteries of the foot are less distinct than yesterday. Was ordered to take a dose of castor oil to-morrow morning, should the bowels not be freely opened without it.

6. Passed a very comfortable night, and is free from pain. His bowels not having been opened this morning, took the castor oil, which has operated very freely.

9. In every respect doing well. The wound was dressed to-day, and looks very healthy. The pulsations in the foot are less distinct, but can be felt; the limb is of the natural warmth.

Immediately after this patient was removed from the operating theatre, Mr. Hamilton proceeded to remove the left testicle of a man 22 years of age, affected with scrofulous disease of that organ. The operation was neatly performed; six ligatures were applied, the edges of the wound were brought together by four sutures, and straps of adhesive plaster. The patient has since gone on very well. The testicle was not examined.

On the 7th instant a man was admitted with compound comminuted fracture of the humerus, having fallen down a cellar while in a state of intoxication. The limb was placed in splints, and the patient was directed to keep quiet in bed. The following afternoon he became slightly incoherent. On the morning of the 9th, violent symptoms of delirium tremens came on; he was ordered 40 drops of laudanum, and a mixture composed of *Mist. camph. e ammon. carb.*, with *sp. æth. nitr.*, and also 16 ounces of wine. At one p.m. he died.

On Friday, 6th July, Mr. Scott operated for hydrocele of the spermatic chord, on a man ætat. 61. The disease has existed for about seven years. About fourteen ounces of fluid were evacuated.

On the same day that gentleman divided the sphincter ani of a man for a deep fistula in ano.

J. W., the lad whose leg was amputated by Mr. Luke on the 27th June last, continues daily improving. The stump looks very well; there is very little discharge. Seven ligatures have come away; his spirits are very good, and he rests well; bowels open without medicine.

The communication forwarded to The Lancet from the Central Board of Health, relative to the impunity with which fruit and vegetables may be eaten during the prevalence of cholera, reached us too late for publication in this number of the journal, but as the document is of public importance, it shall appear in The Ballot of to-morrow, and be farther disseminated in next week's Lancet.

THE LANCET.

Vol. II.] LONDON, SATURDAY, AUGUST 4. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXXVIII.

A SKETCH OF THE NERVOUS SYSTEM—THE ORIGINS AND FUNCTIONS OF THE NERVES OF PECULIAR SENSATION, THE OLFACTORY, OPTIC, AND AUDITORY; AND ALSO OF THE NERVES OF PURE MOTION, THE THIRD, FOURTH, SIXTH, AND TWELFTH PAIRS.

I have observed in a former lecture, that the cerebral and the spinal nerves present themselves symmetrically, that they are arranged in pairs, one on each side of the mesian line, that runs from the anterior portion of the brain to the cauda equina. I will first enumerate them in the order in which they present themselves at the base of the brain; rapidly describing their origin, destination, and function. We shall then, perhaps, be able to class them according to their respective functions, and thus, perhaps, get rid of a little of the mystery which has been supposed to encircle the nervous system. Having done this, I shall enter at some length into the consideration of the office of each, and that will be the time for me to endeavour to justify my departure from the nomenclature and physiological exposition hitherto adopted by veterinary writers.

Description of the Cerebral Nerves according to their Situation at the Base of the Brain.—Commencing anteriorly, we find,

1. *The Olfactory* nerve arising from the corpora striata and callosum; passing through the cribriform plate of the ethmoid bone, and distributed on the membrane of the nose—*a nerve of peculiar sensation*, and its minute ramifications, affected only by the odoriferous particles of bodies.

2. *The Optic* nerve arising from the thalamus nervi optici, and connected by medullary striæ with the corpora quadrigemina,—pursuing its course to the back of the eye, piercing the sclerotic coat, expanding over the choroid coat, and forming the retina; *a nerve of peculiar sensation*, and affected only by the particles of light.

3. *The Motor Oculi*, from the middle of the crus cerebri to all the muscles of the eye, except the superior oblique and the abductor—*a nerve of voluntary motion.*

4. *The Patheticus*, from a little sulcus on the crura cerebri at the base of the testes to the superior oblique muscle,—*a nerve of voluntary motion.*

5. *The Trigeminus*, evidently proceeding from two roots: the inferior one from the crus cerebri—the superior one from the crus cerebelli. The superior one forming or passing through a ganglion—the inferior one unconnected with the ganglion, but uniting with the other branch immediately beyond the ganglion, and both contributing to form one compound nerve distributed over the face,—*a nerve of common sensation and of voluntary motion.*

6. *The Abducens*, from a transverse sulcus between the pons varolii and motor root of the eighth pair (the portio dura of the seventh), and going partly to the retractor, but principally to the abductor muscle of the eye—*a nerve of voluntary motion.*

7. *The Voluntary and Organic Motor Nerve of the Face.*—The portio dura, springing evidently from two roots; the first to be traced from the central column of the inferior surface of the medulla oblongata, and the other from the head of the lateral column, or corpus olivare, if it might be so termed in the quadruped;—united together as they escape from the brain, and forming a compound nerve, *the voluntary motor nerve of the face, and directing also those motions, partly voluntary and partly involuntary, which are connected with the respiratory system of organic life.*

8. The *Auditory* (the portio mollis of the seventh), traced from the floor of the fourth ventricle; passing down the side of the medulla oblongata; entering into the same sheath with the eighth (the portio dura);

escaping through the foramen auditorium internum, and spreading its pulpy ramifications over the internal chambers of the ear,—*a nerve of peculiar sensation,* and affected only by the vibrations of the air.

9. The *Glosso-Pharyngeus,* arising from the lateral column of the medulla oblongata by several little filaments, partially ramifying over the constrictor muscles of the pharynx, but chiefly expended in the base and substance of the tongue. *An organic motor nerve* associating the action of the muscles of the pharynx and tongue, with the discharge of the function of respiration and deglutition.

10. The *Par Vagum* of our anatomists—the *pneumo-gastric* nerve of the French, but, more properly, the *cerebro-visceral organic motor nerve.* It arises from the lateral column of the medulla oblongata, by numerous filaments, a little posteriorly to the glosso-pharyngeus, and escapes from the cranium in the same sheath with it. After distributing several branches to the pharynx, the larynx, and the œsophagus, it gives large and singularly involved plexuses to the heart and the lungs; and having entered the abdomen, its ramifications on the stomach are numerous and complicated. Some plexuses go to the liver, and an important branch, uniting with the grand organic nerve of the abdomen, helps to form the similunar ganglion,—*an organic motor nerve.*

11. Included in the same sheath with the glosso-pharyngeal and the cerebro-visceral, is a singular nerve, which anatomists have agreed to call the *spinal accessory nerve.* It is formed by the union of several minute filaments derived from the lateral column of the spinal chord, some of which may be traced as low as the fifth cervical nerve. Having, in the manner I shall hereafter describe, reached the cerebro-visceral and glosso-pharyngeal, it neither identifies itself with them, nor communicates with the substance of the brain, but turns round and escapes in the same sheath with those nerves, and, soon separating from them, is distributed over some of the muscles of the neck and shoulder, associating with them in the involuntary discharge of the function of respiration, and therefore being *an organic motor nerve.*

12. Still posteriorly, on the medulla oblongata, but rising from the central column of its inferior surface, we find the *linguales* escaping through the anterior condyloid foramina, running between the pterigoideus, and the larynx, penetrating into the substance of the tongue, and giving motion to that organ. *It is a voluntary motor nerve.*

Here ends our brief catalogue of the cerebral nerves; but our subject would not be complete if I did not glance at the origins of the nerves along the course of the spinal chord. At the very point where the medulla oblongata terminates in the spinal chord, we find the first pair, *the sub-occipital,* and we may take it as a specimen of all the rest. Numerous little filaments arise, from the longitudinal sulcus between the central and lateral columns on the inferior surface of the chord, but which are derived from, or may be traced to, the central column; they approximate and unite, and form a nervous chord. Other filaments spring from the corresponding sulcus on the superior surface, but they are not so numerous: and they are larger and bolder in their origin. They also approximate and unite, and form a nervous chord; but at the point of union, and still within the dura mater, these latter form a ganglion,—a little swelling or knot,—immediately beyond which, the chord shrinks again to nearly its former bulk; and then, and still within the dura mater, or at the moment of escaping from it, the two chords unite and form a perfect but compound nerve, of which I shall say no more at present, than that it combines the properties it derived from these central columns on the inferior and superior surfaces of the spinal chord, and is *a nerve of sensation and voluntary motion.* It is distributed to the neighbouring parts, and it anastomoses with those above and below, for important purposes, which will be the subject of future consideration. Given out at each articulation of the cervical bones, as we proceed down the neck we find one of these compound nerves; and towards the lower part of the neck, we observe filaments from the fourth, fifth, and sixth cervical nerves, uniting to form a singular nerve, which takes its course down the neck, without anastomosis, and traverses the chest, and ramifies upon the diaphragm. This *phrenic nerve* is the motor nerve of the diaphragm,—the grand agent in respiration, and, therefore, we have no difficulty in denominating it *an organic motor nerve.* I shall have to show, that it is not improbable, that although the filaments which compose it are derived from the cervical nerves, yet they may actually have their origin from that portion of the spinal chord which seems to be devoted to the purposes of organic life.

The dorsal, lumbar, and sacral nerves, have the same structure, and discharge the same function. They are all nerves of sensation and voluntary motion; and those of the extremities, derived from plexuses of the cervical and dorsal, and the lumbar and the sacral, possess the same character.

The Ganglial Nerve.—There is one nerve wanting to complete our list. It belongs neither to the cerebral nor the spinal system, and seems in its function to be independent of both. At the base of the cranium, and in front of the atlas, I find a pyriform reddish body, which gradually contracts and terminates in a nerve. I inquire not now into the origin of this ganglion, but a superficial glance at the nerve tells me that it is performing some important office. It is connecting itself with the cerebro-spinal, and with every cervical nerve, but more particularly it is forming complicated plexuses on every neighbouring blood-vessel. I trace it particularly on both the external and the internal carotid, and I follow it in the subdivisions of these vessels, until, from the minuteness of the vessel and the pulpiness of the nerve, it eludes my sight. Hereafter I shall have to trace the course of the nerve in the thorax, forming, with the cerebro-visceral, a plexus, or rather an investing membrane around every vessel of the heart and lungs; and then having reached the abdomen, and, in the semilunar ganglion combined its own influence with that of the cerebro-visceral and the phrenic, it becomes the sun or centre of organic nervous power, diffusing its radiations over every artery and absorbent, and gland and ganglion—everything connected with secretion, nutrition, and life—itself the very principle of life and action—the soul of the organic system. It was termed, before its character and power were suspected, the *sympathetic nerve*, because it seemed to connect the whole system together; it is denominated by others the *ganglial nerve*, from its supposed origin either in the superior cervical, or the semilunar ganglion; but it would more properly be designated *the great organic nerve*—the *secretory*, *nutritive*, *chemical*, while the cerebro-visceral is the *motor* organic nerve—the power which presides over, and effects the changes in, that fluid which the motor nerve keeps in circulation. But we are not yet quite prepared for this.

My object was to give you a kind of bird's-eye view of the nervous system, that we might perhaps be enabled so to class its different portions, and trace its connexions, as to remove much of the mystery that has erroneously been supposed to hang over the subject.

Divisions of the Nervous System.—One division is plain and palpable—*the animal* and *the organic*—that which is concerned in the conveyance of impressions to the brain, and volitions to the muscles, and that which is connected with respiration, circulation, digestion, and life itself. The six first of the cerebral, the eighth (portio mollis), and the twelfth (the linguales), and all the spinal nerves are those of *animal* life. All the others except the seventh (portio dura) are nerves of *organic* life. Both the animal and the organic nerves admit of a simple subdivision into those of *pure sensation*, concerned alone in transmitting impressions to the brain as the first, second, and seventh; or *pure motion* conveying the volitions of the mind; as the third, fourth, sixth, and twelfth (linguales)—or, in accomplishing both purposes, as the fifth pair within the cranium and all the spinal nerves. The division of the organic nerves is not so evident; we have, however, plainly enough the simple motor-organic nerve in the ninth (the glosso-pharyngeus), the eleventh (the spinal accessory), and the phrenic—and we shall cautiously inquire whether we do not find it also in the tenth (the par vagum), and the chemical and vital one in the ganglial nerve; while, plainly enough, the seventh pair occupies a neutral ground, or belongs both to animal and organic life—it is both the *voluntary and the organic motor nerve of the face.* When we have made ourselves masters of this theory of the origins and functions of the nerves, we shall not have much difficulty in unravelling many of their anastomoses, and comprehending the wise and benevolent purposes that are accomplished by them.

We will commence with the animal nerves, and, first of all, with those of pure or peculiar sensation.

The Olfactory, or First Pair of Nerves.—The nerve which first presents itself at the base of the brain, if we commence anteriorly, is *the olfactory nerve.* It arises partly from the *corpus striatum*; but a medullary tractus, terminating in it, and evidently giving one origin to it, may be traced to the base of the *corpus callosum.*

Its Character as a Nerve of peculiar sensation.—Whether we trace its origin to the corpus striatum or the corpus callosum, it is derived from the *superior portion* of the medullary matter at the base of the brain—that which would be a continuation of the superior surface of the spinal chord if so far prolonged. If we follow either root to the corpus callosum, the olfactory nerve is evidently referable to the *centre* of that superior surface, and there is nothing inconsistent with the same derivation, when we attribute it to the corpus striatum. It proceeds in a manner *bodily* from the substance of the brain—it is an apparent *prolongation of the medullary matter of the brain.*

Comparative anatomists should not describe it as a mere mamillary process—"the ashen-coloured termination of the brain abutting upon the ethmoid bone." Although the nervous chord is not separated from the brain to so great an extent as that of the optic, or even the auditory nerve, and not nearly so much as we find it in the human subject, yet it has decidedly left the brain before it reaches the cribriform plate. *Its course is short, and it has not* *a single anastomosis.* I would beg you to pay attention to these peculiarities, these characteristics (as I shall, by-and-by, be justified in calling them) of a nerve of *peculiar sensation.* Its minute ramifications I have said are affected only by the odoriferous particles of bodies.

Size of the Nerves.—Observe the development of this nerve, bearing a beautiful proportion to the necessities of the animal. First, compare it with the same nerve in man—in him simply connected with pleasure—in the brute with life itself. In a brain not more than half the size of that of the human being, namely, that of the horse, the olfactory nerve is four times as large as in man. Compare its bulk in our different domesticated animals. In the horse it is large, for in a state of nature it affords his only warning against poisonous plants—but, almost all over the world, he has become our stabled servant, comparatively rarely sent to collect his own nutriment amidst the herbage of the field, and having the greater part of his food provided for him. The ox is oftener driven to shift for himself, or, if worked by day, he is usually turned out at night, and needs a somewhat acuter sense of smell. Observe, that although his brain is but little more than half the size of that of the horse, the olfactory nerve is nearly as large. In the sheep and swine it is comparatively as large as in the ox, or even more so; and in the dog you cannot fail of remarking its still greater comparative bulk.

Peculiarities about them.—The olfactory is, next to the fifth, the largest of the cerebral nerves, and it is the softest in its texture, nevertheless its fibrous structure has been demonstrated, and it is the excessive looseness and thinness of the neurilema that gave it its pulpy appearance. It has a singular cavity in it, the use of which I am unable to state. That cavity is a prolongation of the anterior cornu of the lateral ventricle; it terminates however in a blind pouch before the nerve reaches the ethmoid bone, and there is consequently no channel by which, as was formerly supposed, any fluid can be conveyed from the brain into the nose.

Termination.—In company with, and under the real mamillary process, it abuts upon the cribriform or thin perforated plate of the ethmoid bone, and sends its pulpy fibres through it, which are visibly spread over the upper part of the septum and superior turbinated bone, and which probably extend over the whole of the nasal cavity.

The Olfactory Nerves in Birds.—In our occasional patient, the feathered biped, the olfactory nerve comes nearer to a mamillary process, for it arises from the very point of the anterior termination of the brain, and seems to be a mere continuation of it. Having left the cranium it enters into a bony canal, and so passes through the orbit which interposes; and it is finally distributed, not on the ethmoid bone, for that is in a manner wanting in the bird, but on the pituitary membrane covering the superior *conchæ* of the nose.

The Optic, or Second Pair of Nerves.—On the crura cerebri, when they first appear at the base of the brain, there are seen two prominent medullary chords, winding their way over the crura in a direction inwardly and anteriorly. They first emerge from under the hemispheres, opposite to the corpus albicans, and they meet each other immediately anterior to the infundibulum and the pituitary gland.

Their Character as Nerves of a peculiar sensation.—The medullary chords of which I have just spoken—the *tractus optici,*—can be traced to the *thalami nervorum opticorum* in the centre of the brain; they are, in fact, a continuation of the thalami; or the thalami contract, and are prolonged into these medullary chords. I have also said that they are connected by medullary striæ with the corpora quadrigemina, and particularly with the nates. Now the thalami and the nates evidently occupy that portion of the base of the brain which would correspond with the *superior surface of the spinal chord;* and they occupy a *central* situation upon that surface. The optic nerves are also a palpable *prolongation of the medullary substance of the brain.* The course is *somewhat longer* than that of the olfactory nerve, but through the whole of that course, *they have not a single anastomosis.* They are also nerves of *peculiar sensation,* and their minute ramifications are affected only by the particles of light.

Peculiarities of the Optic Nerves,—Their Decussation.—I have shown you these medullary chords, winding their way over the crura, and meeting at the centre, and not only meeting, but apparently decussating—crossing. There is a certain identification of medullary matter of the two nerves, to secure the more perfect discharge of their function—to assist experience in correcting the errors which would arise from the somewhat dissimilar impressions made, or the pictures (if we so dare to call them) painted on the two retinæ, and to ensure perfect and distinct vision. The impression made by the odoriferous particles of bodies on the two nostrils, or the two olfactory nerves, are, in a state of health, precisely the same; the vibrations of the air, as they reach the tympanum of the ear on either side are the same; but from the construction of the organ of sight, the picture on each retina must differ, and that very materially, with regard to a near object. Look at any object first with one eye, and then with the other, and you will understand what I mean. This partial mingling of medullary matter in the two nerves, was probably designed to prevent or to remedy, the confusion of vision which would otherwise result. We have, however, abundant proof that there is no actual decussation. This brain was taken from a dog that had been blind in the right eye for several years. Observe how the nerve on the right side is shrunk, and what a curious semi-transparent hue it has assumed. Our principal patient, the horse, is sadly exposed to inflammation of the eye, which too often destroys the sight: but there is this peculiarity about the disease, that when blindness is produced in one eye, the other is spared for a long time, and often for life. If we examine the brain of one of these horses, we uniformly find the nerve on the blind side shrunk; it obeys the law which governs every other part of the frame, and when it is no longer useful, it gradually withers away. It has been long settled, I believe, among human anatomists, that there is no decussation of the nerves. The examination of some Russian criminals determined the matter. Theft was in some part of that vast empire punished with the loss of an eye for the first offence, and death for the second. Some pirates who had suffered the first penalty, were afterwards convicted of a second offence and executed, and the nerve on the blind side was shrunk in every one of them. In birds, the optic nerves, which are exceedingly large, manifestly decussate; and in fish, and particularly in those with a bony skeleton, they cross each other without mingling at all.

Their Fibrous Structure.—The fibrous structure of the optic nerve is sufficiently evident in all our quadruped patients, and these fibres are so symmetrically arranged, as to leave a canal in the centre of the nerve, which had been observed by some old anatomists, but the function of which had been misunderstood. It contains *the central artery of the retina*, derived from the ophthalmic artery, which penetrate through the dura mater after the nerves leave the cranium, and enter with the optic nerve into the globe of the eye, for purposes which will be described when we treat of vision. This canal also contains the central vein of the retina.

The Reticulated Structure of the Optic Nerve.—When, however, I speak of symmetrical arrangement, do not misunderstand me; I do not mean to say, that the exact parallel direction of these fibres can always, or scarcely ever be traced. It would seem to be necessary in such a nerve as this, of peculiar sensation, that there should be an intercommunication of nervous influence and matter through its whole extent, otherwise accident or disease might, oftener than it does, cause strange confusion in the impression conveyed; therefore there is a continual exchange of minute branches in every direction going forward between the fibrils, in some nerves through their whole extent, in others more plainly to be traced in certain parts, and occasionally carried to such an extent, that the interior of the nerve presents only a complicated reticular appearance. It is so here; although we can trace the central canal plainly enough, the interchange of branches is so numerous, that the true parallel direction of the fibres cannot be distinguished.

Termination of the Optic Nerves.—Each nerve, after having separated from its fellow, pursues its course on its own side, escapes through the foramen opticum, and enters the cavity of the orbit; it there continues its path obliquely, penetrates between the muscles, and, particularly surrounded by the retractor muscle, reaches the inner and inferior and posterior part of the eye-ball, the coats of which it penetrates, and, entering the globe of the eye, it expands over the choroid coat, and forms the retina.

Sheathed in Dura Mater.—The dura mater quits the nerves as they emerge from the cranium, and they afterwards pursue their course surrounded and defended by their own neurilema. The optic nerve is, however, an exception to this rule, for the dura mater continues to encircle it until it reaches the outer or sclerotic coat of the eye. This, according to some, is for the purpose of forming the sclerotic coat, which certainly bears considerable resemblance to the dura mater; while the pia mater, according to the same anatomists, is expanded within the globe of the eye. More of this when we treat of the structure of the eye; at present I will only remark, that the difference in situation, and length of course, between this and the other two nerves of peculiar sensation, and the difference in function too, will sufficiently account for this prolongation of the dura mater. It is necessary that the impression or intelligence conveyed by a nerve of peculiar sensation, should be distinct and pure. The olfactory, the moment it quits the cranium, ramifies upon the membrane to which it is destined. We shall presently see, that the auditory nerve is defended by a bony canal until it arrives at the labyrinth of the ear. Nothing can cause confusion in the impressions which are conveyed along these nerves, but the optic nerve has to travel through the orbit before it reaches its point of destination, and is encircled by muscles of strong and rapid action; this dense covering, therefore, is necessary to protect it from that compression which might occasionally interfere with the perfect discharge of its function.

The Optic Nerve in Birds.—The structure of this nerve is singular in birds. The fibrous conformation is evident; but these fibres are curiously collected into laminæ in birds of prey, and I think I have seen something like it in some of the wilder birds which we have partially domesticated, as the pheasant. The nerve cannot be dissected, as in the quadruped, into a collection of fibres, more or less numerous and complicated, but into a congeries of leaves; it seems like the folding of one continuous membrane, and the retina is the expanding of the plaits. We can connect this, as we shall have to do many other peculiarities in the eye of the bird, with the almost inconceivable acuteness of sight in birds of prey particularly, and of which natural history affords us a thousand proofs.

The Auditory Nerve.—This nerve is the eighth in order, reckoning anteriorly posteriorly. English anatomists have termed it the *portio mollis* of the seventh pair; I will venture, following the example of most continental anatomists, and for reasons that shall be stated hereafter, to call it simply the auditory or eighth pair of nerves.

Character as a Nerve of peculiar Sensation.—I find it on the floor of the fourth ventricle, which is not only a prolongation of, but may almost be said to be the *superior surface* of, the spinal chord: I trace it from the slit of the calamus scriptorius, and therefore from the *centre* of that surface. Its pulpiness and transparency render it now and then a little difficult to follow, and I mutilate or lose it in the dissection, but you here plainly perceive it passing down the side of the medulla oblongata, meeting the seventh nerve (the portio dura of the seventh), and then, in union with it, taking an outward direction, and entering the *foramen auditorium internum*. Its course there is short; it soon abuts upon a cribriform or perforated plate of bone closing the passage, through which it passes, and then dividing into various portions, spreads itself over the internal chambers of the ear, while there is not throughout the whole of its course *one anastomosis*. Its root cannot, like the first and second nerves, be traced to distant parts, for it at first appears on that track of medullary matter to which the others are ultimately referred. It does not seem so much a prolongation of a part of the brain, and yet it springs, as it were, *bodily*, and *at once*, from the floor of the ventricle. Human anatomists have spoken of fasciculi of fibres proceeding from above and below to form the root—something like striæ I have seen; but the fibres, if they do exist, are indistinct.

There is a circumstance respecting one of the divisions of this nerve, that we have not observed in any portion of the first or second nerves, namely, that those fibres which go to the trumpet-like extremities of two of the semi-circular canals, pass through a little knot or ganglion. I must not at present draw any conclusion from this isolated fact, but I shall have hereafter to remind you of the ganglion on a part of the 8th nerve of peculiar sensation. The minute ramifications of this nerve are affected only by the vibrations of the air.

Comparative Bulk of the Auditory Nerve.—When I come to treat of hearing, I shall have to point out many peculiarities, accounting for the greater acuteness of this sense in the quadruped, than in the human being, arising from the external ear—the tympanum, the ossiculi, &c.; but at present I must only point out to you in each of the brains before us, the greater bulk of the auditory nerve compared with that in man; on the other hand, the adaptation of the human ear to the various modulations of tone, will be an interesting subject of inquiry.

Comparison of these Nerves of peculiar Sensation.—Now, then, Gentlemen, let us for one moment recapitulate. We have examined the three cerebral nerves of *peculiar sensation*,—those of smell, sight, and hearing. We have traced each of them to that portion of the medullary matter at the centre of the brain, which answers to, or may be considered as a prolongation of, the surface of the spinal chord;—we have further traced them to the centre of that surface;—we have observed, that they seem to be prolongations of the substance of the brain, or to spring bodily from that organ, and that through the whole of their course they have no anastomosis. To the two first of these peculiarities I shall hereafter recall your recollection, when I shall have to speak of the relative situation of nerves of certain functions—the prolongation of the substance of the brain—the long root that can be traced deep into the brain, seems natural enough, when we consider, that from these three nerves the animal derives almost all his knowledge of surrounding objects—all the materials for thought and improvement. It seems reasonable that these impressions should be conveyed securely and deeply, there to be preserved, arranged, and compared. They are as much identified with the mind and with animal life, as the well-protected organs of the thorax are with the inferior organic principle,—and the wisdom and benevolence of the arrangement by which these impressions of smell, and hearing, and sight, are preserved distinct and pure, and by which no anastomosis with other nerves is permitted, needs no illustration of mine. But

I proceed to other nerves, differently situated and of different function, viz. the nerves of pure motion.

The Motores Oculorum, or Third Pair of Nerves.—About the middle of the crura cerebri in the horse, (and to this animal, if an especial exception is not made, all these anatomical details refer,) and rather nearer to the corpus albicans than to the pons varolii, the third pair of nerves take their origin. They seem to rise superficially from the crura cerebri; they cannot be traced at all into the substance of the brain, except in a way that I shall presently notice, and are clearly referable to another surface of the spinal chord, or its supposed prolongation—the *inferior* one. Let me here remark, once for all, that from the difference of the attitude of the human being and the quadruped, the *anterior* surface of the spinal chord in man, is the *inferior* one in the brute; and the *posterior* one in the biped, is the superior one in the quadruped. These nerves spring from the *central* column of the inferior surface, that track which I have already traced under the pons varolii, and along the crura cerebri. These nerves, do not, however, rise *bodily* from the crura, but by *several minute fibriculi ranged in a line,* following the course of this medullary track. These fibriculi approximate to each other, and unite either before they penetrate the dura mater, or as they are leaving it, and form a nerve. This is very different from what we have observed in either of the nerves of peculiar sensation.

Course of the Nerve.—The motor oculi takes its course obliquely across the crus cerebri, and winds downwards, and enters the cavernous sinuses, and escapes through the foramen longum orbitale into the orbit;—there it divides, and the smaller branch goes to the levator oculi, while the larger subdivides, and goes to all the other muscles of the eye, except the superior oblique and the abductor, for the purpose of moving the eye in its socket. Then it is *a nerve of voluntary motion.*

Its Anastomosis.—As we follow its course we observe another circumstance in which it differs from either of the sensitive nerves,—it connects itself with other nerves. It communicates with minute branches of the ophthalmic division of the fifth pair, and more particularly unites with them that curious ganglion *the ophthalmic*, whence the iris derives its supply of nervous influence. I enter not into the cause of this yet. We shall know more about it by-and-by, for a thousand anastomoses will come before us, the great utility of which will be evident on the slightest inspection; but I content myself with pointing out to you how far we are departing from the character of a nerve of peculiar sensation.

The Pathetici, or Fourth Pair of Nerves.—I feel considerable difficulty in classing this nerve. It goes to the trachealis or superior oblique muscle of the eye, and governs its motion, but whether it is a nerve of pure voluntary motion, or merely associating the motion of the eye with some organic function, and probably that of respiration, I am not assured. It arises from a singular part of the brain,—from the crus cerebri at the base of the testes, and, consequently, from the superior surface of the prolongation of the spinal chord; it arises by filaments. I can plainly trace two, but it does not arise either from the centre, or the side, of the crus—more inclining, however, to the latter than to the former;—and when I make a longitudinal incision through the cerebellum, and lay open the whole of the fourth ventricle, I plainly see that it does spring from that portion of the crus which may be considered as a possible prolongation of the lateral column of the medulla oblongata. In this respect it differs from the voluntary motor nerve which I have just considered, and from every other voluntary motor nerve of the brain or the spinal chord. On the other hand, it curiously winds its way through the other nerves, and proceeds to this single muscle without an anastomosis; nor can I trace to the trachealis muscle any fibre from any voluntary motor nerve. This is very singular, for with regard to every other muscle (the visceral ones excepted), if there be communications from an organic nerve to associate the action of that muscle in the discharge of some vital function, there are also communications from some spinal cerebral nerve for the purpose of voluntary motion.

Difficulty about the Fourth Pair of Nerves.—Sir Charles Bell, to whom, more than to any other physiologist, we are indebted for, our knowledge of the nervous system, considers this as a respiratory nerve, partly from its situation; and there I confess I am a good deal disposed to acquiesce with his opinion; but I do not understand the function which he assigns to it, of *relaxing*, not *contracting*, the trachealis muscle, and thus causing the eye to roll upwards, under the superior lid, in every forcible effort of respiration, as sneezing or coughing, and so tending to the security and preservation of the eye. I can form no clear conception of this *relaxation* of a muscle by the *direct* agency of the nerve that supplies it—nor can I see the great danger to which the eye is exposed in more than usually forcible respiratory efforts, nor a purpose sufficiently important to lead to the introduction of a separate nerve, and from so singular an origin. I must, therefore, at present class it with the nerves of voluntary motion. I know it to be a voluntary motor nerve as it regards the usual action of the superior oblique muscle, and yet there is something about this nerve which I do not understand, and with which I am not quite satisfied. I shall enter a little more into this, and you will be better prepared for the inquiry, when the muscles of the eyeball come in order before us.

The Abducentes, or Sixth Pair of Nerves.—These nerves arise from a transverse sulcus on the medulla oblongata, between the pons varolii and the proper motor origin of the 7th pair, and they are clearly referable to the central columns of this prolongation of the spinal chord. Therefore we find them on the *inferior* surface of the spinal chord, and springing from its *central* column; and when we look more attentively at it, we can trace *the fibriculi springing in a line* from which it derives its origin, and uniting to form it. We trace it to the cavernous sinus, and through the foramen lacerum into the orbit, where it divides, and some branches of it are distributed over the retractor muscle, a muscle peculiar to quadrupeds, answering the purpose of hands in withdrawing the eye from danger, if that danger cannot be warded off, and the main bulk of the nerve going on to the rectus externus or the abductor of the eye, by means of which the eye rotates outwardly, and which is also a kind of antagonist to the rectus internus, and the superior oblique. Then I can clearly understand that this is *a nerve of voluntary motion.*

Its Anastomoses with the Great Organic Nerve.—There is a peculiarity about it, however, which I do not understand. There are *some minute anastomoses with the other motor nerves of the eye*, and with the ophthalmic branch; that I can comprehend; but there is a branch, a very considerable branch, given to each of the abducentes from the *great organic nerve.* They evidently go not from the sixth to the organic, but they come up in bulk through the base of the skull, and unite with the sixth. I shall, by-and-by, be assured, and I shall understand the reason of it, that this organic nerve freely anastomoses with every other nerve and every part, but the reason of these considerable branches being sent to the sixth, is not yet apparent to us. For the present I leave this part of our subject, again reminding you, however, that this abducens is a kind of antagonist of the superior oblique, the character of whose nerve a little puzzled us. If it should prove to be an organic nerve, why here is plain and palpable union with the great organic to correspond with it.

The Linguales, or Twelfth Pair of Nerves.—Still proceeding posteriorly, and in the same track, we find the lingualis, the last of the simple nerves of the cranium, arising also from the *inferior* surface of the medulla oblongata—arising also, but not so decidedly, from the *central* column, and *formed from fibriculi arising in a line.* And here I begin to have a clearer conception of another nervous character, of which I had not a trace in the nerves of pure sensation. This nerve is scarcely formed before it anastomoses, and that by many a filament with the cerebro-visceral—its union with the sub-occipital and the first cervical cannot escape my observation; and when I dissect it carefully, and especially its descending branches, I find it allied with the second cervical, with the spinal accessory, and with the great organic nerve; and then its superior branch unites with the gustatory branch of the fifth, in endless anastomoses, making a complete plexus, or network, of nervous filaments. I have many an important office to be performed by this little agile member the tongue, and I must call in the aid of many a neighbouring muscle, or almost all of them, in order to effect them. After all these anastomoses with motor and organic nerves, the main branch, accompanied by the fifth, penetrates into the substance of the tongue, pervades every part of it, and is lost in innumerable minute ramifications. This then is plainly *a nerve of voluntary motion.*

Characters of the Cerebral Nerves of Voluntary Motion.—And now, Gentlemen, after a somewhat long and intricate, yet I trust not quite unpleasant journey, we have arrived at a resting place. This is the last of the pure voluntary motor nerves; and, comparing them together—leaving out at present the fourth, with which I have said I scarcely know what to do—we obtain these characters of the cerebral nerves of voluntary motion, which may guide us materially in our future inquiries. They arise from the *inferior surface of the prolongation of the spinal chord, and from its central column, by numerous fibriculi attached in a line to the surface of the brain, and they anastamose freely with neighbouring nerves.*

TEMPERATURE OF WHITES AND NEGROES.

In a manuscript memoir on his voyage to Central Africa, presented to the Academy of Sciences by M. Douville, he has mentioned some experiments made in Africa on the difference which exists between the temperature of these two races, according to age, sex, &c. It results from the researches of M. Douville, that the temperature of the negro is *cœt. par.* much superior to that of the white;—that the heat of negresses is greater than that of negroes up to the fifteenth year of their age, but after that period less, but still greater than that of whites;—that the negroes diminish in temperature as they grow old; and, finally, that the old negroes have still a higher temperature than the whites.

ON THE IMPORTANCE OF
HYGROMETRICAL OBSERVATIONS
IN METEOROLOGY.

Read before the Franklin Institute of Philadelphia.

By James Espy, *Esq.*

The importance of hygrometric observations, as forming a part of a meteorological journal, especially as we are now furnished with a hygrometer, which will inform with certainty, not only of the relative quantity of vapour in the atmosphere, but of the absolute quantity, induces me to offer a few remarks on this subject to the members of the Franklin Institute, for their consideration.

The object I have in view is, to induce scientific bodies to use their influence in having meteorological journals kept in different parts of the country, on the same plan, and to be published monthly. Many persons, for their own private gratification, are now keeping imperfect meteorological journals, who would feel a tenfold interest in the subject if they knew how to keep a more perfect journal, with no more trouble, and at the same time contribute greatly to the public good. As many persons in the country may be willing to keep a journal who are not furnished with a Daniell's or Jones's hygrometer, it may be useful to know how its place may be supplied. Any contrivance to obtain water in a common tumbler, cold enough to cause moisture to settle on the outside, will answer the purpose. During the summer months, I have generally found pump-water sufficiently cold, and in other months, I have been obliged to resort to salt and snow. Any of these methods I consider preferable, both in simplicity and accuracy, to Daniell's or Jones's, for by mingling waters of different temperatures, one above the other below the dew-point, the exact point may be obtained to great nicety in a short time. Whereas in using the evaporation of ether, the temperature is suddenly depressed below the dew-point, sometimes to a considerable distance, and it requires much more time and skill to arrive at even tolerable accuracy.

However, when I could not procure ice or snow, I attached a small piece of muslin, or a thin piece of sponge, to one side of the bulb of a common thermometer, and poured a small quantity of ether upon it, carefully watching the naked side, to see when the dew began to settle on it. When it settled very rapidly, the thermometer had sunk below the dew-point, but by wiping off the dew, and letting it settle again repeatedly,' I could obtain a pretty accurate result. I have frequently compared these results with those I obtained by means of the cold water in the tumbler, and the agreement was very satisfactory.

In taking the temperature of the air, also, great care is required, or errors will be committed. If the thermometer is surrounded by bodies, either hotter or colder than the air, it will be affected by radiation; in the former case, it will be a temperature too high; in the latter too low. In a clear calm evening, after sunset, I have known a thermometer placed near the ground, and exposed to the open sky, to indicate from eight to twelve degrees below the temperature of the air, which may be obtained nearly by swinging the thermometer rapidly round. So during the day, if it is exposed to bodies heated by the sun, it will sometimes be several degrees warmer than the surrounding atmosphere, which is not heated by radiation, but only by contact with solid bodies.

In cloudy weather, however, no such effects take place, for then the surface of the earth, and all other solid bodies, have their radiated caloric returned by the clouds, and consequently retain the temperature of the air. On cloudy nights the surface of the earth does not cool below the *dew-point*; and, therefore, there is no dew. Even in clear nights, when the dew-point is far below the temperature of the air in the day, the surface of the ground does not sink in temperature to the dew-point by radiation, and in such case no dew is formed. I have observed in August, when the dew-point has been as high as 76° Fahr., when the temperature of the air during the day was above 90°, that but little dew was formed during the night in the city of Philadelphia. No doubt more was formed in the country, where the surface of the ground was not exposed to radiation from hot brick walls. Be that as it may, I never saw dew begin to form in the grass in my yard, until its temperature had sunk to the dew-point. Indeed no reason can be imagined why it should settle on the grass before it cooled by radiation to the dew-point, any more than in a tumbler of water above the dew-point. The phenomena are the same. The dew does not, in either case, fall through the air; but the vapour which is in the air, equally diffused by its own elasticity, when it comes in contact with solid bodies, cooled by radiation or otherwise, below the dew-point, is deprived of so much of its caloric, that it is no longer able to maintain its elastic state, and is precipitated in the form of dew.

An objection may be urged against this theory, which at first appears plausible. It is known that dew does not appear under a shed, or even under a covering of thin muslin, spread out at a distance of some feet from the ground; and, therefore, it is inferred that dew falls through the air from the upper regions. The true reason, however, is, that the temperature of the grass or other bodies under the cover does not fall to the dew-point, the radiations being returned by the cover, which may easily be ascertained by placing one thermometer under the cover where there is no dew forming, and another in the grass beyond the cover where the dew is forming, when the difference will be detected. I have seen a difference of eight or ten degrees, under the protection of a silk handkerchief, even when the air had a free passage between it and the ground.

Hence is demonstrated the great utility of even thin coverings over tender plants during clear nights in the spring, when the surface of the ground is likely to be cooled by radiation down to the freezing point. Plants so protected may be preserved, while those in their unprotected state will be entirely destroyed.

Another argument against the theory of dew here proposed is, that the lower parts of the atmosphere do not contain vapour enough to form such heavy dews as sometimes appear, when more vapour is precipitated than is contained in a hundred feet of the atmosphere, in height, and it cannot be presumed that all this air comes in contact with the surface of the earth during a calm night, when the dew is the greatest.

But this objection will have no weight, when we recollect what Dalton, I believe, first demonstrated—that aqueous vapour is not supported by the air, but by itself; or, in other words, if the air should be suddenly annihilated, the aqueous vapour which is diffused through it by its own elasticity would neither sink upon itself, nor expand to a higher elevation; but its own elasticity would just be able to support its weight without further condensation. If now we suppose the lowest stratum of this vaporic atmosphere to be changed into dew, it is manifest the whole vaporic atmosphere must descend; and this takes place whether we suppose an atmosphere of common air or not: a longer time will be required to settle through the air than if there was none; but the ultimate effect is the same in both cases.*

Indeed the hygrometer itself affords a most satisfactory proof that aqueous vapour is not supported by atmospheric air; for it is no uncommon thing for the dew-point to remain the same through the night, indicating that the density of the vapour continues unchanged, which could not be the case if vapour were supported by the air. For as vapour is about one-half the density of air, even when at its greatest density, unless compressed by art, it would rise rapidly through the air, from its levity, and leave the lower parts absolutely dry during the night, whilst no vapour is sent up from the ground to take the place of that which, by supposition, has ascended into the upper regions of the air. Surely it will not be said that vapour is sent up from the ground at the very moment dew is deposited there.

It must not, however, be supposed, that after vapour is condensed into cloud, it continues to press only on the vapour below it. Part of its weight is then supported by the air, and the vapour below being relieved from part of its pressure, immediately upon the formation of the cloud, expands, and the dew-point falls, as will be shown hereafter.

It is well known that the increase of aqueous vapour in the atmosphere over a particular region, will depress the barometer, because this vapour is only about one-half the specific gravity of common air; but there are so many causes which produce a depression of the barometer, besides the rise of the dew-point, or increase of moisture in the air, that a fall in the thermometer is not an infallible sign of rain; yet I have no doubt that these two instruments united, will enable the meteorologist to predict rain with absolute certainty, after a sufficient number of observations have been made to eliminate by induction a general law.

In the course of these observations, no doubt many curious facts will be discovered which the present state of science does not enable the philosopher to predict.

I will mention one here, which was so contrary to my anticipations at the time I first observed it, that for a while I doubted the possibility of ever arriving at any useful knowledge on the subject.

In the summer of 1829 there had been a very long period of dry weather, and I had been for many days taking the dew-point very frequently, with much anxiety to discover the first indications of rain, when suddenly all my hopes were extinguished, by a fall in the dew-point of several degrees in the course of half an hour, but to my great astonishment, in about two hours and a half, there came on one of the most violent showers I ever witnessed. For several days previous to this, the barometer had been falling, and the dew-point rising, (showing that the watery vapour was increasing), whilst the temperature of the weather had remained nearly stationary. I had been very particular in taking the dew-point, so that I felt pretty confident no error was to be attributed to the observation; but still I was anxious to have an opportunity of witnessing a similar result.

Nothing similar was observed, though it might frequently have occurred, until the 16th July 1830, when I witnessed the following remarkable phenomenon. The weather had been very warm and dry for some days, the hygrometer being nearly stationary at 74°, for I had taken the dew-point very frequently, and on the day of the present occurrence I was sitting with the means of taking the dew-point with me in my yard, when I perceived it suddenly sink three degrees in a few minutes, and, at the same time, I observed a cloud forming immediately over my head, and, in a few minutes more, the whole time, not more than half an hour, a gentle shower of rain succeeded, hardly enough to wet the pavement. Immediately after the shower the dew-point rose again to its former elevation 74°. Not long after this I had the satisfaction of recording an observation of the same phenomenon, which renders it absolutely certain that sometimes, at least, the dew-point sinks rapidly, and to a great extent, while it is raining in the neighbourhood. From July the 16th to the 19th, the dew-point gradually rose from 74° to 76°; it then remained stationary at that point night and day, the mean temperature of the air being about 90°, till the 22nd, at five o'clock p. m. At five the dew-point was 76°, as it had been for four days before. At twenty minutes after five, the dew-point was 70°; at thirty minutes after five it was 76°; and at six o'clock it had fallen to 62°, that is 14 degrees in one hour.

During all this time there was a great appearance of rain in the north—vivid lightning, thunder audible, and a strong wind blowing towards the cloud; and the temperature sunk twelve degrees. At forty-five minutes after six, the wind had nearly ceased; but it did not begin to rain till nine, and rained but very little; but, as I understood the next day, it had rained very hard between five and six within a few miles of the city; the barometer sunk $\frac{5}{100}$ of an inch, while the wind was blowing; but rose again when it became calm. The next morning the dew-point had risen nine degrees at six o'clock, and at two p. m. one degree more, being then within four degrees of its previous state.

It would not readily occur to any philosopher, a priori, that the dew-point would fall just previous to the commencement of rain, or while it is raining in the neighbourhood; but after the fact is known, it is easily explained, on the principle first demonstrated, I believe, by Dalton, and afterwards discussed at large by Daniell—that aqueous vapour forms an atmosphere independent of the atmospheric air, and that these two fluids press only on particles of their own kind, and not on each other, consequently as soon as it begins to rain over a small portion of country, as it often does in a summer shower, the equilibrium of the vaporic atmosphere is disturbed, a partial vacuum of vapour is formed over the region where it is raining, and the vapour from the surrounding regions rushes towards the cloud when the pressure is less, diminishing the tension of the vapour in the regions around the cloud, and thus lowering the dew-point. How so rapid a depression of the thermometer took place, I am not prepared to explain, but the wind towards the cloud was no doubt produced by the impulse of the aqueous vapour against the particles of atmospheric air.

The only objection occurring to me which can be brought against this explanation, is, that the wind is known to blow in all directions from a falling shower, just at the borders of the shower. But this does not invalidate the explanation given above, for this effect is produced by the pressure of the drops of rain, which may be considered as exerting their whole force of gravity on the air beneath, as soon as they cease to accelerate in their descent towards the surface of the earth, but the wind thus produced is known to extend not far beyond the skirts of the shower, and, of course, at the distance of two or three miles, it may yield to the more extensive influence of the cause assigned above.

My object, however, at present being not to assign causes for phenomenon, but to cause them to be observed and recorded, I proceed to another subject, which I hope will engage the particular attention of all those who may hereafter keep meteorological journals. That is the evaporating point. By the evaporating point, I mean the point to which the thermometer will sink by being surrounded by wet paper.

I some time ago instituted a course of observations on this subject, constantly comparing the evaporating point with the dew-point, principally with a view to ascertain if any law could be established by which the dew-point could be known, by having the distance of the evaporating point below the temperature of the air. In the first part of these experiments I took it for granted, as stated by Mr. Leslie, that the evaporating point is the same, whether the thermometer is at rest in still air, or moved with rapid motion; but I have ascertained by not less than fifty observations, made with a view to decide this one point, that this is not the fact; for the thermometer covered with wet paper invariably stands lower when moved rapidly, than when permitted to remain at rest in still air, sometimes to the amount of four degrees, and this too when the surrounding solid substances are known to be of the same temperature as the air, so that the thermometer could not be affected by radiation. The reason assigned by Mr. Leslie is so plausible, that nothing but experiment could have led me to suspect its fallacy. He says that the motion produces a more rapid evaporation, and of course more cold; yet the latter effect is exactly neutralized by the warm air coming in contact with the thermometer. I have not yet been able to ascertain the law desired; but that such a law exists I have no doubt; and if it could be ascertained, the trouble of ascertaining the dew-point would be entirely avoided; for the evaporating point can always be obtained with as much ease as the temperature of the air. The simplest method which I have been able to think of, to obtain the evaporating point, is to take an earthen pot unglazed, which I cause to be made for the purpose, and keep it filled with water, either when the air is still, as in a room, or where it is in motion, as you choose. The surface of the pot being constantly wet, the water within will always give you the evaporating point at any moment you may wish to make your observation. This much my observations will warrant me to say on this subject, that in high temperatures in summer, the evaporating-point is nearer the dew-point than the temperature of the air; and in low temperatures in winter, it is nearer to the temperature of the air than to the dew-point, consequently there is some intermediate point where it stands half way between those two temperatures. Perhaps, also, the ratio may depend on the distance of the dew-point from the temperature of the air. It would appear that when the dew-point is very far below the temperature of the air, the evaporating point recedes from the temperature of the air, and approaches the dew-point.

By using the earthen pot to obtain the evaporating point, another very important inquiry may be answered, without any other trouble than measuring the water used in replenishing the pot; i. e., How much water is evaporated from a humid surface in a given time?

Another subject which is at this moment exciting considerable interest among the philosophers of Europe, is to ascertain the difference of temperature with shade and with sun, both with the bulb of the thermometer naked, and with one covered with black silk. Mr. Daniell thinks there is much greater difference in high latitudes than in low. In Philadelphia I have frequently found the naked thermometer rise forty degrees, and once fifty-six, above the temperature of the air, while the one covered with black was generally twelve degrees higher. Mr. Daniel also thinks, that in high latitudes, radiation after sun-set cools the surface of the ground much more than in low latitudes. In this latitude I have seldom found the temperature of the grass more than eight degrees below the temperature of the air. Whereas Mr. Wells, who wrote the beautiful little essay on dew, says he has seen it as low as sixteen degrees below the temperature of the air. This would allow frost to occur in low situations, when the temperature of the air during the night would not be below forty-seven degrees. Whereas here, frost can hardly occur, unless the temperature of the air falls below forty.

Captain Scoresby mentions, as a strange and unaccountable phenomenon, that he has frequently observed, when sailing in high latitudes, the surface of the sea begin to be covered with thin ice, immediately after sunset, while the temperature of the air was still several degrees above the freezing point. When he called it unaccountable, he probably did not think of the principle of radiation. In Peru, where they have such heavy dews every night, the power of radiation must be very great, or if not, the dew-point must be but little below the temperature of the air during the day. Some traveller, I do not recollect his name, says he has seen frost in Africa in the torrid zone. Now I do not think this at all incredible; for the temperature in those regions must fall as far below the *mean* in the night, as it rises above the mean in the day. The mean temperature of the torrid zone is about 84°, and as loose dry sand is a very bad conductor, and a very good absorber and radiator of caloric, the surface of those wide-extended deserts of sand in calm weather, may easily be conceived to become fifty or sixty degrees above the mean during the day, and fall as far below it during the night, which would bring it down to the freezing point.

The time of taking the hygrometric observations is not important; it is not so, however, with the barometer. This instrument has four diurnal fluctuations: just before sun-rise, when the air is the coldest, and about two o'clock, when the air is warmest, and of course neither contracting nor expanding by change of temperature, the barometer stands at its mean height; but sometimes between sun-rise and the hottest part of the day, when the air is expanding most rapidly by increasing heat, the barometer will stand at its greatest diurnal elevation, and some time after the hottest part of the day, when the air is suffering the most rapid contraction from cooling, the barometer will stand at its greatest diurnal depression. In the torrid zone, the point of greatest elevation is found by observation to be between nine or ten a.m., and that of greatest depression between four and six p. m. Now as it would be very desirable that two observations should be taken every day, I would earnestly recommend that between nine and ten, and five and six in the afternoon, be the time of taking observations—and if any observer wishes to take a third observation, or if any should be unwilling to take more than one per day, let two o'clock p.m. be the hour.

From the following table it will appear what proportion of the atmosphere in weight is vapour, when the dew-point is at the different temperatures from 32° to 80°. For example, when the dew-point is at 32, the pressure of the vapour alone is $\frac{2}{10}$ths of an inch of mercury, that is, $\frac{1}{150}$th of the whole, if the barometer stands at 30 inches; and if the dew-point should rise to 80° Fahr., which I believe it never does, for I have never seen it above 76°, then would the pressure of the vapour alone be equal to one inch of mercury, that is, one-thirtieth of the whole atmospheric pressure.

TABLE.*

Temp. of Dew-point.	Weight of Vapour, Inches.	Temp. of Dew-point.	Weight of Vapour, Inches.
32	0.200	57	0.474
33	.207	58	.490
34	.214	59	.507
53	.221	60	.524
36	.229	61	.542
37	.237	62	.560
38	.245	63	.578
39	.254	64	.597
40	.263	65	.616
41	.273	66	.636
42	.283	67	.655
43	.294	68	.676
44	.305	69	.698
45	.316	70	.721
46	.328	71	.745
47	.339	72	.770
48	.351	73	.796
49	.363	74	.823
50	.375	75	.854
51	.388	76	.880
52	.401	77	.910
53	.415	87	.940
54	.429	79	.971
55	.443	80	.1000
56	.458		

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* Vide Turner.

TREATMENT OF THE

MALIGNANT CHOLERA

ON BOARD THE

CUMBERLAND, CONVICT HULK.

Efficacy of Mustard Emetics—Failure of the Saline Treatment and Venous Injections—Evidences of Contagion.

To the Editor of THE LANCET.

SIR,—The malignant cholera attacked the convicts confined on board the *Cumberland* convict hulk on Sunday the 10th ult. As in most other instances, the first seizures were the most malignant; every case for the first ten days was severe: they then assumed a less violent form, and by the 27th it ceased to attack them. During that period 198 patients were under treatment, of whom 33 died, including one petty officer, and one nurse, and not including the case of my friend Mr. Conway, surgeon of the Ordinary, who caught the disease while zealously and faithfully assisting me in my duties. He was struck with collapse at once, and never rallied. Instead of entering into an account of symptoms and cases, I will merely state, that in the admirable details of Mr. Scott, in his reports, are to be found the exact picture of the disease, as it happened in this hospital. The symptoms we observed show the fidelity with which he has pourtrayed the disease. In the general treatment of the complaint, I have to state, that when the men were attacked, in the first instance, with severe purging and vomiting, accompanied by prostration of strength, and a sense of sinking at the pit of the stomach, the mustard emetic was invariably administered. It is impossible to speak too highly of its value; immediate, copious, and powerful, vomiting follows its exhibition; the blood is driven to the surface, the pulse is raised, the skin becomes warm, and a speedy expression of relief is uttered by the patient. Sixty-nine prisoners required no other remedy. Several had it repeated two or three times; one, an old man (82), had it given six times in the 24 hours, and recovered. Since the disappearance of the cholera, we have had several cases of simple diarrhœa in which it has also been given with the same beneficial result. In some cases of protracted recovery it was prescribed in drachm doses at intervals of two or three hours; it was there also advantageous.

When the vomiting, purging, prostration of strength, and sense of sinking, were accompanied by spasms, bleeding followed the exhibition of the emetic, and was repeated, if it did not arrest the symptoms; in the early stage there is seldom any difficulty of procuring blood, particularly if attempted after the emetic. I never saw the least injury result from its employment; on the contrary, to its effects, preceded by the mustard emetic, I attribute the few cases of death, 19 only having fallen a sacrifice, who were received from the *Cumberland* between the 10th and 27th. The other 14 cases of death were patients who were in the hospital at the period when the disease broke out, and who were infected by the cholera patients. Of these, one whose thigh had been amputated a few days before, one with jaundice, two with fever, one with a severe contusion on the abdomen, two under salivation, three under phthisis pulmonalis, and four old men, were struck at once with constant purging, that is, an uninterrupted flow of serous fluid from the rectum, and complete collapse; it was evident that no treatment could avail in these instances. When the disease continued in spite of the V. S. and mustard, calomel in 10 or 20 grain doses, in combination with camphor or capsicum, ammonia, ether, and mustard cataplasm, were chiefly relied upon. The acetate of morphine was found decidedly superior to opium in substance or tincture. The coldest water which could be procured was the only drink permitted, and though we have numerous high anathemas against its use, I venture to assert that it is the best which can be given, and that it is the fluid of all others most liked by the patient.

The saline treatment recommended so strongly by Dr. Stevens and his advocates was fairly and fully tried. It was on the faith of their statements that I ventured to deviate from the above plan. Their cases and mine, however, must have widely differed in their nature, for assuredly never was mode of treatment more unsuccessful. I deeply lament ever having trusted to it.

The favourable manner in which the saline injections have been introduced to our notice, could not fail to demand our confidence. You will see in a letter from Dr. Anderson, surgeon to the *Aboukir*, extra Cholera Convict Hospital Ship at Chatham, to the Central Board of Health, in THE LANCET of the 23d ult., that he speaks with exultation of the success of the practice, and the letter is actually headed, "Injection of saline fluids into the veins. Five cases, three of which were successfully treated." If any one has been induced to try the practice on my friend's hurried statement, it would be a matter of regret, for those whom he thought convalescent have since been numbered with the dead. The proportions of the fluid injected, the temperature, and the method employed, were such as have been recommended by Dr. Latta. The phenomena attendant on the injection were, an augmentation of temperature, a gradual disappearance of the blue colour, a restoration of arterial action, an increase of respiration, and a general feeling of comfort by the sufferer. In one case, sleep took place during the process. All these effects were, however, transient—in a very short time rigors supervened, and the train of symptoms which had been averted reappeared, and required the repetition of the injection, or the aid of other remedies. While the fluids were passing by the rectum it seemed to do no injury, but when this was not the case, the injection required to be very slowly introduced, the organs of respiration, and, eventually, the brain, being evidently affected by it. Death ensued in every case except one, who was attacked by phlebitis in a most aggravated form, and now lies in a precarious state, and has one large abscess on the shoulder, and another on the hip. The following table shows the general result.

Cases of Cholera in which Saline Injections were employed.

Name.	Stage of Disease.	Quantity injected.	No. of times injected.	Event.
Thompson ..	Second	72 ounces	Once	Died 1½ hour after injection.
Healy	Third	144 ditto	Thrice	Ditto 56 hours
Rowlands ..	Ditto	304 ditto	4 times	Ditto 8 hours
Birch	Ditto	96 ditto	Twice	Ditto 4 hours
Johnson	Ditto	216 ditto	Twice	Ditto 13½ hours
Eccles	Second	96 ditto	Once	Ditto 9 days
Higgens	Third	96 ditto	Twice	Ditto ½ hour
Longbottom..	Ditto	80 ditto	Twice	Ditto 2 days
Cooper......	Second	192 ditto	Twice	Alive.

In the cases of Johnson, Rowland, Healy, and Birch, the insensible draining had begun before the injection was attempted, but not so in the others. Eccles, who lived nine days after the process, was incessantly restless, and exhibited well-marked symptoms of cerebral irritation, but decidedly different from the affection of the head met with in the consecutive fever.

It is difficult to account for the appearance of malignant cholera on board the *Cumberland*. Only one case had previously occurred at Rochester, and two at Chatham; but the period of their occurrence is too remote to authorise the suspicion that they had any connexion with the irruption of the disease on the 10th of June. A boy, however, was some days previously attacked with cholera on board a barge at a short distance from the hospital ship, and brought on board; he had medicine administered, was carried to Gillingham, and died. At the same time, at a short distance from the *Cumberland*, the *Brothers*, a timber ship, was in a most unhealthy state, having discharged several of her labourers on account of bowel complaints—one or two of which afterwards terminated in most malignant cholera. One of their men was brought to us by the local board of health on the 23d, who had been ill for several days; he was in the collapsed stage, but recovered. We received a second from her, but he died immediately after his admission. In the absence however of any positive proof of its introduction by contagion, there is abundant evidence to prove its contagious character when once introduced. It attacked equally all the decks of the *Cumberland*, all the hospital nurses with two exceptions, some of the invalids on the Invalid Hospital Deck, 28 of the prisoners previously in the hospital with other diseases, one of the guards, and two of the medical officers, and several of the others had severe premonitory diarrhœa.

The most decidedly marked evidence of contagion which I have met with in this neighbourhood, was the introduction of the disease into the village of Lower Upnor. One of the labourers on board the *Brothers* obtained for his mother (Mrs. Collins) the washing of the linen of the crew, which he carried backwards and forwards as he went to and from his labour. She caught the disease and died; several persons in communication with her met a similar fate, and the complaint became general in the village. The number of prisoners on board the *Cumberland* and *Canada* at the period of invasion, was between seven and eight hundred. The sufferers from cholera were therefore few, and the small amount should be attributed to the constant inspection kept up by the officers, and the care they took to report every case of bowel complaint immediately on its occurrence. They were unfortunately cut off from their usual labour, and it was even gravely recommended to send the hulk to Standgate Creek. In such weather, and in such a crowded condition, many of the prisoners labouring under strong depressing passions, this measure must have proved a very fatal one. The public mind, however, was systematically worked up to this point; so much so, that my own appearance on shore, after four days and four nights incessant labour in the sick wards, was looked on with as much horror as if a mad dog had been in the street. They managed things more wisely at Deptford and Woolwich, where the existence of the disease among the prisoners made no interruption to their usual labour in the Arsenal and Dock-yard; they infected no artillery man, marine, or civilian, proving, that though contagious, the contagion is, like all other contagion, communicable only in a very low degree *when freely diluted*. It is confinement under exposure to the atmosphere of many cholera patients, which forms the greatest danger.

In forwarding this sketch, I beg to conclude with returning my grateful thanks to Dr. Anderson and Mr. Whitmarsh; also to the naval and civil assistants, whose thirst for knowledge expelled every idea of personal danger. To those officers in the naval medical department who frequently favoured me with their advice, my obligations can never be repaid. Poor Conway, alas! was cut short in the midst of his active exertions to investigate the nature of this singular disease, while on a branch of public service to which he did not belong; his death was as much a sacrifice as ever was that of the most desperate "forlorn hope" in battle. I am, Sir, your obedient servant,

ARCH. ROBERTSON, Surg.

Rochester, July 19.

TREATMENT OF THE

MALIGNANT CHOLERA

AT THE CHOLERA HOSPITAL, WESTMINSTER.

THE house fitted up as a Cholera Hospital for the united parishes of St. Margaret and St. John, Westminster, having been placed under the care of Mr. Furnivall, with Mr. Ollier as the consulting surgeon, the cases (treated conjointly there and at their own homes) now exceeding 100, I, as the partner of the first-named gentleman, having been actively engaged in attendance upon the whole of the above, have (with their permission) compiled a few facts relating to the disease and its treatment, which I trust will be found interesting.

With the above object I have taken the first 100 cases, and have arranged them into a tabular form, so as to show the proportion of deaths and recoveries, as modified by age and sex, and the treatment having been conducted at their own homes, or at the hospital.

At the first view it will be seen that the advantage is greatly on the side of youth, the male sex, and treatment at an hospital.

One Hundred first Cases.	Number of Patients.	Deaths.	Recoveries.	Total.
Number of Males	46	32	14	46
Number of Females........................	54	41	13	54
		73	27	100
Number treated at their own homes	34	27	7	34
Number treated at the Hospital..............	66	46	20	66
		73	27	100
Number of patients under the age of 15 years ..	13	9	4	13
Ditto between 15 and 30	13	5	8	13
Ditto between 30 and 50	32	21	11	32
Ditto between 50 and 70.....................	37	33	4	37
Above the age of 70.........................	5	5	0	5
		73	27	100

With regard to the number of deaths, it should be observed, that no cases have been returned as cholera, unless the evacuations unequivocally presented the true characteristics of that disease. Also it has been very rare for any of us to see the case before it had been far advanced in the collapsed stage. Many have been for some hours previously under the treatment of others, and have been sent to the hospital in a dying state; some have actually expired on the road. Again, when it is noted (as the table shows) that one-half. the deaths have taken place in persons exceeding fifty years of age, the recoveries above that period amounting only to four, I think our exertions have not gone altogether unrewarded.

There have been few exceptions to the fact, that a premonitory diarrhœa of from some hours to three days duration takes place. We have never failed to check the diarrhœa, and our cases have been numerous, and many of them severe. From the tendency of the true choleric attack to take place during the night, an opiate at bed-time is always requisite when the diarrhœa is urgent.

It is by no means my intention to give any detailed plan of treatment, but a few observations on some of the principal points seem necessary.

The hot-air bath, tins, or bottles of hot water, are always desirable; as an external stimulant the mustard cataplasm is the most efficacious. With regard to internal stimulants, where there exists much irritability of stomach they invariably increase it; few patients will continue for any length of time to take brandy-and-water as common drink, even when cold and much diluted.

To allay the excessive thirst, any fluid may be allowed, in a quantity not exceeding a tablespoonful at one time, a large quantity generally exciting vomiting. In some cases great benefit in this respect was derived from frequently sponging the palm of the hands with cold vinegar.

Bleeding in the stage of collapse was positively injurious. In the early stages, and after reaction has commenced, it may be used with much benefit, but requires judgment. Injections into the bowels have never been omitted, warm water alone has been found beneficial, it may be rendered more stimulating by the addition of brandy and a little common salt; if the cramps are violent, the addition of a drachm of laudanum rarely fails to relieve them, tincture of valerian or assafœtida may be conjoined. The stimulating enema should be repeated at intervals.

We have found by far the greatest benefit to result from moderate but frequently-repeated doses of calomel, or blue pill and opium; we generally prefer the latter combination, the interval between the doses being gradually lengthened, but persevered in at least until the evacuations contain feculent matter. Care should be taken that very little fluid be given for some minutes after the pill is swallowed, or its rejection may be the consequence. When reaction has been established, it must be moderated and treated according to circumstances and the judgment of the medical attendant, but officious interference is to be avoided.

I am afraid that theory, hypothesis, and empiricism, have too far superseded science and judgment in the treatment of this disease. The saline mode of treatment was most assiduously tried in several cases, but I regret to say the result has not been so favourable in our hands as stated to have been in those of others.

In four cases the injection of saline fluids into the veins was tried, but not with sufficient success to induce us to persevere.

Latterly, several cases, after recovering from the stage of collapse, have become comatose, with stertorous breathing, and other symptoms of effusion on the brain; they have generally terminated fatally.

J. Wright, M.D.

3, Princes-Court, Storey's-Gate,
Westminster, July, 28th, 1832.

CASES OF

MALIGNANT CHOLERA,

TREATED PRINCIPALLY BY

CORROSIVE SUBLIMATE AND ACETATE OF LEAD.

By F. J. Hart, *Esq., Surgeon.**

Case 1.—James Snell, aged 33, a leather-dresser, of rather a spare habit of body, had been labouring under diarrhœa for three or four days, but on the morning of Thursday the 12th inst., was seized with violent cramps in the stomach and legs, and excessive vomiting and purging, which obliged him to apply for assistance.

At 12 o'clock I saw him. At this time there was no pulse to be felt at the wrist or ancles, the feet and hands were cold, contracted, and of a leaden hue; the eyes very much sunk and ghastly; the vomiting and purging still continued, and the cramps in every part of the body very distressing; but the skin, except the extremities, was quite warm; the pulse in the upper part of the arms and legs, and the heart, was at 90, small and hard. Ordered mustard cataplasms to the feet, mercurial friction over the abdomen, to take brandy-and-water in small quantities, and a teaspoonful of the following mixture, in brandy and-water, every quarter of an hour until the sickness ceased, with one pill every half hour.

℞ *Acid. nitrici*, gtt. xxx;
Tinct. opii, gtt. xl;
Aquæ puræ, ℥iss. *M. ſt.*

℞ *Hydrarg. oxymur.* gr. iv;
Plumbi s. acetas, gr. iv;
Pulv. opii, gr. vj;
Ol. Caryoph. q. s. M. ſt. pil. xii.

At 6 p.m. very little change had taken place, except that the sickness had somewhat abated, and he complained of his head being light. At 10, the sickness had entirely ceased, the purging was become less frequent, the pulse could now be felt at the wrist, though very feeble, and the cramps were wholly confined to the body; the hands and feet were regaining their natural state; he complained of great thirst, and had passed no water. To continue the pills every hour.

On the following day (Friday 15th), he was much better, had been in a violent perspiration all night, the purging had ceased, no return of sickness, the cramps much abated, the hands and feet perfectly recovered, and the pulse 120, soft and full; complains of his head, and great thirst. Ordered to drink lemonade; the abdomen to be bathed with hot water; to discontinue the cataplasms, and to take two tablespoonsful every three hours of the following mixture:—

℞ *Quininæ sulph.* gr. vj;
Acid. sulph. dil. ʒi;
Tr. card. co. ʒiij;
Aquæ ad ℥vj.

In the evening no return of the violent symptoms; head much better; perspiration still continues; pulse 100, and soft.

On Saturday he was gaining ground very fast, and on Tuesday I met him, when he only complained of great weakness.

Case 2.—John Hubbard, aged 30, the captain of a vessel, of good health and sober habits, was seized on Tuesday, the 10th inst., with a violent purging and vomiting, which obliged him to go home. At six o'clock in the evening I saw him, when the vomiting and purging had increased, the pulse hard, small, and feeble, cramps in the legs and stomach, great prostration; still, both the hands and feet were warm, and the eyes were not sunk; ordered warm bottles to the stomach and legs, to take nothing but a little brandy and water, or gruel with brandy in it, and one of the pills every half hour. At nine o'clock he was much altered for the worse, the pulse quite gone at the wrist, hands and feet cold and blue, the eyes sunk, and countenance ghastly, great oppression at the chest, and the voice nearly gone; the cramps were so severe that the poor fellow writhed from one side of the bed to the other with agony, the sickness and purging occurring every five minutes. I considered the case hopeless, and ordered him a teaspoonful of the mixture in brandy-and-water every five minutes until the sickness ceased, and then to take, in addition to one of the pills as above, one containing two grains of *P. Opii* every half hour, until the violence of the symptoms was abated. I saw him in the morning, and never did I see so great a change produced in so short a time; the sickness had ceased in half an hour, and the purging about four o'clock; the cramps had entirely subsided; the feet and hands were quite warm and natural, pulse very full, 120, and hard; the tongue from white had become of a vivid red, and the difficulty of breathing was gone; but his head became exceedingly light. I bled him ad ℥xx, ordered to continue the mixture every hour, and drink lemonade. In the evening every unpleasant symptom had subsided; he had passed water freely, and been in a violent perspiration all day; his mouth rather sore, and complained of weakness. Ordered the quinine mixture, a repetition of which completed his cure.

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* Communicated by the Central Board of Health.

OBSERVATIONS ON MERCURIAL FRICTIONS IN MALIGNANT CHOLERA.

By J. Tytler, *Esq., Surgeon, Bengal Service.*

(*Abstracted from the "Transactions of the Medical and Physical Society of Calcutta," vol. 5,* 1831.)

Of the innumerable remedies proposed for this terrible epidemic, scarcely one is in the least to be depended on. Antiphlogistics, antispasmodics, narcotics, stimulants, cathartics, diluents, have been tried in every possible combination, and every one tried in vain.

In the midst, however, of these innumerable trials, it appears to me that one remedy has been constantly and unaccountably overlooked; although I trust, before concluding, to be able to demonstrate that its efficacy is great and unquestionable, and that it may be used with wonderful success in cases where everything else is either useless or impracticable. This remedy is mercurial friction on the skin.

Mercurial frictions do not accord very clearly in character with any of the classes of medicine above enumerated; they can scarcely be said to be antiphlogistic, antispasmodic, narcotic, stimulant, cathartic, or diluent. In what point of view, then, are they to be considered, and on what theoretical principle are we to employ them?

To me there appears but one such principle; though it is, I believe, the fashion of the present day, to neglect and to decry it; I mean Hunter's theory of the incompatibility of morbid actions. It is to be inquired, whether mercurial irritation is not also incompatible with cholera, so that, if the one be artificially excited, the other must spontaneously cease. Were we fortunate enough to be able to establish this as truth, the treatment of cholera would be most simple and satisfactory; and would consist only in exciting salivation by the shortest and least irritating means; after which, we might hope that the cure would be ensured.

The advantage of mercurial frictions over the administration of that medicine by the mouth, are great and many. By rubbing in, all irritation to the stomach is avoided. In the second place, it appears to me, that in cholera there exists a spasm of the absorbing vessels of the intestinal canal, which prevents their conveying into the system even such of the calomel as may be retained. Accordingly, it may be observed, that it is extremely difficult, if not impossible, to salivate persons under such circumstances, by any quantity of calomel whatever; while, as I trust to be able to show, no such spasm exists in the vessels of the skin, and the system is affected through them very readily. I shall proceed to the detail of those cases which have led me to the above opinions.

Case 1.—T. R. B., a country-born lad, of sickly constitution, subject to attacks of fever, and suspected to have disease of the spleen. He had eaten a very heavy dinner of fat broth the day before, and at four o'clock in the morning of the 17th February, was attacked by cholera morbus. * * * Fixing my attention on that very remarkable feature of the disease, the total absence of bile, either in the matter vomited, or in the alvine discharge, it suddenly occurred to me, that mercurial frictions might have a tendency to retard this suppressed secretion. I therefore directed a quantity of mercurial ointment to be rubbed into the thighs every two hours, and 12 grs. of calomel, and 40 drops tinct. opii, to be given at the same intervals. I visited him twice more in the course of the day, and found that he vomited every dose of calomel as fast as he took it, and that the purging went on uninterruptedly; in short, there was little appearance of amendment, and on leaving him, I felt fully persuaded that next morning would give me notice of his death. To my great surprise I was informed next morning, that he was considerably better, and on going to him, found almost every symptom of cholera removed. The vomiting had ceased, but he had had one or two watery stools. The skin and pulse were natural, and he was able to take a little light food. He was gently salivated. It was unnecessary to do more than give a dose or two of purging medicine till his mouth got well.

Remarks.—If it be here said that calomel and opium, and not the frictions, cured the disease, let it be remembered, that each dose was vomited almost as fast as it was taken: the cure evidently depended on the salivation, and the salivation surely was not produced by the rejected calomel. We may observe, too, that the cure was not effected by a restoration of the secretion of bile, as I had expected; for the watery stools continued next morning, when all other symptoms were relieved. It seems to have been simply the irritation producing ptyalism that removed the cholera.

Case 2.—About a year ago, a country-born lad at Monghyr, of almost the same age and constitution, was taken ill early in the morning, in consequence of having eaten some bazaar sweetmeats the night before. I saw him about eight a.m. Calomel, laudanum, and stimulants, were administered the whole day, with the most scrupulous punctuality, but without producing the smallest change in the disease, and he died at four p.m. These two cases were as nearly parallel as can be imagined, and to what can the difference of event be reasonably ascribed, but to the difference of treatment?

Case 3.—William Holman, private of H. C. 2nd European regiment, was left here sick by a detachment proceeding up the country. On the evening of March 7th he was attacked by cholera, and continued vomiting and purging all night. He was brought to hospital next morning, and I there found him in a very advanced stage of the disease. I directed a quantity of mercurial ointment to be rubbed into his thighs directly, and 12 grs. of calomel given internally. This was done, but the calomel was immediately vomited. Shortly after, the frictions were renewed, and the calomel and opium again given, but again rejected. This treatment was continued every two hours during the whole day. In the mean time the sickness and purging continued; the cramps were frequent and severe, and to use his own expression, when they came on, his legs became as stiff as deal boards. The cold perspiration and smallness of pulse continued; the only thing he had been able to keep in his stomach was ℥ii of Madeira wine, which he had taken by sips in the course of the day. In this situation I left him, after he had been rubbed in for the last time, about eight p. m., fully expecting to hear next morning that he was either dead or dying; for I can truly say that I had never known a patient recover from such desperate circumstances. My surprise and gratification were, therefore, extreme, when on visiting the hospital next morning, I found him almost quite well. His skin moderately warm and moist; pulse 105; his mouth was very sore, and he was spitting copiously. He had a few minutes before vomited a little, in consequence of drinking some cold water, but otherwise his stomach was free from disturbance. He had taken another allowance of wine, and was so well, that I was induced to order a dose of infusion of senna. This he rejected, but found no other inconvenience from it, and had three natural stools in the course of the day. No other symptoms of disease remained, except a violent salivation, from which he gradually recovered, and was discharged cured on the 22nd.

Remark.—If it be again urged, that the calomel and laudanum cured this man, I must answer, as in the last case, that these medicines were rejected as fast as they were swallowed, and that I never saw any one restored by these medicines from such a situation before.

Case 4.—Serjeant Benjamin Church, a sober man, of the European invalids, on the 13th of March, after eating his breakfast, was seized with cold shivering and great thirst. On drinking a little water, he was immediately attacked by watery vomiting and purging. What he vomited tasted intensely bitter; but the vomiting soon stopped, and was succeeded by a sense of violent heat in the stomach, and a feeling of cutting across the navel. The purging continued, and he was brought to hospital about noon, and had there, immediately, two watery stools. When I saw him, his skin was warm; pulse 120, strong and full. Very thirsty, but the moment he tastes water, is obliged to run to stool; no actual vomiting, but very squeamish. *Venesection ad* ℥ xvi. After this he became faint; thirst continued, and other symptoms much the same. Almost immediately after the bleeding, had a copious watery stool. *Calomel* gr. xii. At next visit, half-past one p. m. complaining of extreme pain of the stomach, as if cutting. Skin very warm; pulse 120; says his fingers and toes are cramped, and feels quite cold. Is very desirous of sleeping, but as soon as he begins to dose, is roused by the cramps, and a shooting pain in the stomach. No vomiting, but one watery stool since my last visit. *Repet. inunctio mercur. Tinct. opii* gtt. l. *spt. æther.* gtt. xl. *Aq. fontan.* ℥ss. Soon after getting these medicines he fell asleep, and continued so for a long time. On awaking, appeared greatly better. 14th, Dilucul. seen by the garrison assistant-surgeon, Dr. Smith, who reports that he appears tolerably easy. Pulse qute regular; tongue moist; complains of pain in the abdomen. He ordered *ol. ricin.* ℥i, and this operated three or four times. 15th. Pretty well, but very weak. Thinking all danger over, and wishing to give rather a placebo than an active medicine, I incautiously ordered *mist. camphor.* ℥iss; this he took about nine o'clock, and had scarcely done so, when he was obliged to run to stool, and there was attacked by cold shivering and vomiting, as before. Vomited all his breakfast, and the retching continued very severe. What he brought up was yellow and bitter. I saw him at noon, after he had had three or four watery stools: pulse quick; skin hot; a burning sensation at the stomach, and wishes much to have a fomentation to it. *Tinct. opii* gtt. l, *spt. æther. nitr.* gtt. xl. *Calomel.* gr. xii stat. *Inunctio mercurial. femorib. fomentat. cacumin. papaver. abdomin.* The mercury was rubbed in at twelve and two o'clock; at four p. m. he complained of very severe pain in the abdomen, with coldness in the hands and feet; pulse soft and slow; he begins to have a clammy perspiration. Has eaten nothing, and has had several watery stools. The mercury was again rubbed in at five and six p. m. Seen by Dr. Smith, who reports pulse small and quick; the pain at the stomach continues. He directed *tinct. opii* gtt. xl, *pergat. c. frictione.* Eight p. m., sleeping gently. 16th, a good deal salivated; vomiting and all bad symptoms ceased. Had one black stool in the night; pulse 99; skin natural. He took some breakfast, and immediately after vomited up some black stuff, which gave him immediate relief. He has been well ever since, and was discharged on the 18th, at his own request.

Remarks.—In this case it is demonstrable that neither the bleeding, nor calomel given the first day, effected anything towards a cure. The disease returned in all its violence two days after they were given. It is impossible to ascribe it to anything but the mercurial frictions too sparingly employed the first day, but used abundantly afterwards.

Case 5.—The two next cases will, I think, show the advantages of mercurial frictions over every other practice, even yet more strongly than the preceding. They both occurred on the same day.

Serjeant John Mackenzie was left here in the same party, and on the same account, as Holman. He is a man of sober habits, but of much-debilitated constitution. He had suffered much at Cheduba from fever, terminating in enlargement of the spleen. On this account he had been a considerable time in hospital here, and was but lately discharged. On the 18th of March, after breakfast, he was suddenly attacked by excruciating pain of the stomach coming on in fits, attended with severe sickness, and attempts to vomit. The substance he vomited was, by his own account, lumps of coagulated blood about half the size of his fist. On being brought to hospital about noon, I found him rolling in agony in his cot; pulse 120; excessive and unquenchable thirst. Skin hot and clammy. In my presence he retched violently, and brought up a good deal of mucus tinged with a little blood. *Venesect. ad* ℥xx *stutim.* While the blood was flowing he complained excessively of sickness and thirst; drank frequently small quantities of water, and made several attempts to vomit. At last, when the bleeding was nearly finished, he suddenly became more than formerly sick, and vomited a large quantity of the watery secretion peculiar to cholera. The pain, sickness, and unquenchable thirst, continued perfectly unabated. *Calomel* gr. xx. *Tinct. opii* gtt. xl. *Spt. æther. nitr.* gtt. xl *statim, et inunctio mercurialis femoribus diligenter.* In an hour afterwards no alteration. Vomiting, purging, and pain, as before, and so restless, it is difficult for the coolies to hold and rub him. *Repeat friction, et habeat opii* gr. ii. Six p.m. has been rubbed in three or four times as his agitation would permit. Pulse 120, and small; skin cold and burning. The very acute pain of the stomach has ceased, but he now complains of pain in the region of the liver. *Repet. frictio interval. 6 hor.* Eight p.m. much better; skin warm and comfortable; has not vomited since my last visit, but he has had several stools; has drunk and retained some tea: gums a little inflamed. Nine p.m. In all respects better; vomiting quite ceased, but still purging a little; pulse 84, and regular; skin warm and natural; inclined to eat. Next morning all disease gone, except pain in the enlarged spleen. *Magnes. sulph.* ℥i. This was retained and operated well, and he was discharged cured on the 21st.

Remarks.—In this very violent case the bleeding, though apparently indicated in the strongest manner, both by the symptoms and the constitution of the patient, was so far from being advantageous, that it seemed to accelerate the progress of the malady. Only one dose of calomel was given, and that was immediately rejected.

The opium does not deserve consideration.

Case 6.—While in hospital, attending Mackenzie, my attention was suddenly called to William Bowers, a man of Herculean strength and constitution; invalided for the loss of one of his arms; of excessively drunken and disorderly habits. He had been brought to hospital on the evening of the 18th in a state of great nervous irritation from recent intoxication. No very urgent symptoms appeared till near the morning of the 18th, when, after attemping to eat some breakfast, he was attacked by severe sickness, vomiting, and purging, and all the symptoms of well-defined cholera. I found him with pulse small and quick; skin hot, and covered with perspiration. *Venesectio statim.* After about ℥xii were taken away, he became excessively faint, and begged that the bleeding might be stopped, which was done, and his arm bound up. Directed immediately *calom.* gr. xx. *Tinct. opii* gtt. xl. *Spt. æther. nitr.* gtt. xl, *et frictio mercurial. femorib.* At six p.m. has been rubbed in twice, and is rather easier. Pulse 120, quick, and small. skin warm; has had ten watery stools, and repeated vomitings since my last visit. Longs much for a little sago and wine; this was ordered for him, and the frictions directed to be continued. Eight p.m. A good deal better; the bandage accidentally loosened from his arm, and the blood burst out a second time, and he says he lost more now than before. 19th. Seven a.m. Much better; skin cool; no more vomiting, but several watery stools. Eleven a.m. From some unknown cause the symptoms of cholera re-appeared with even greater violence than before. Skin hot and covered with perspiration; pulse excessively quick; vomiting copiously, but no stools; complains of intolerable uneasiness about the abdomen, which is hard and tumid; says he feels cold, and yet is sensible he is hot; excessively restless and agitated. *Enema sodæ muriat.* ℥i. *Ol. ricin.* ℥ii. *Aq. tepid. q. s. habeat calomel.* gr. xii, *et repet. frictio mercurial.* One p.m. The injection produced these scanty stools; vomited the calomel almost immediately; is much dejected in mind, and persuaded he is dying; nausea remains, but is unable to vomit now; longs much for beer. Repeat frictions, and give him a bottle of beer, with a caution to drink but a little at a time. Three p.m. Much better in all respects; sickness less; drank a mouthful of the beer, and then was disgusted with it; feels his gums a little sore. Repeat frictions at the end of an hour. Six p.m. Still uneasy about the abdomen, but less sick; no stools. *Magnes. sulph.* ℥i. This was vomited almost as soon as taken, and a good deal of vomiting succeeded.. *Calomel* gr. xii, *et rept. frictio.* Nine p.m. Asleep; much better, and he has ever since continued to improve, with the exception of one fit of vomiting, which he had two days afterwards. This case has been the most tedious of all, owing no doubt to his previous excessive intemperance; and he is still in hospital with pain in right side, all symptoms of cholera having disappeared.

Remarks.—On this case the same observations are to be made as before. The first calomel and laudanum could have had no share in curing the disease; since, from some unknown, though probably trifiing cause, it returned in all its violence the next morning. The mercurial frictions at first employed were sufficient to check, but not subdue it. On the second day they were more freely and successfully employed. The bad effects following the exhibition of the salts strengthen what was before noticed of the impropriety of employing medicines of doubtful efficacy.

General Remarks.—No time should be lost in having recourse to mercurial frictions; and they may be begun, in a severe case, by two drachms of the strong ointment on each thigh, for the space of an hour. After the interval of an hour, this may be repeated; then after an interval of two hours, and then of two hours more. After these four inunctions, the disease will probably begin to yield, and the mouth to be effected; and the practitioners must then be guided by circumstances as to the propriety of continuing, diminishing, or leaving off the frictions. In the mean time, to support the strength, a little wine may be given, from time to time. When the irritation of the stomach has subsided, and the vomiting has ceased, whether salivation has actually commenced or not, if the constitution appear to have suffered severely, and the debility be great, it may be proper to leave off mercury, and to exhibit tonics. Of these, the most convenient will probably be found to be tincture, or extract, of bark, and when procurable, sulphate of quinine.

P. S.—Since writing the above, three other cases of cholera have come under my observation. One in a bearer, the second in an Ayah, and the third in a European invalid. All of them were treated on the foregoing principles. The bearer was brought to me about twenty-four hours after the accession of the disease, and appeared almost in its last stage. He was rubbed in three or four times, after which his friends carried him away, but I have heard that he is since well. The case of the Ayah was particularly severe. She was taken ill in the middle of the night, in my own house; and next morning, when I saw her, had all the most aggravated symptoms of the disease, and in addition, appeared in a state of delirium. Her pulse was scarcely perceptible. After four repetitions of the frictions, she also was carried to her house by her friends. Next morning the symptoms of cholera had ceased. She appeared, however, so weak, that I judged it improper to continue the frictions, and in the course of the day gave her nine grains of solution of sulphate of quinine, with three drachms of tincture of bark, and twenty grains of blue pill. This restored her very much, and in the course of next day her mouth became sore. She required nothing else but a dose of infus. sennæ. She is now nearly well, with the exception of violent rheumatic pains in her limbs. The European invalid had been drinking the day before, and was brought to hospital, suffering from the effects of intemperance. He was attacked with exquisite symptoms of cholera about an hour afterwards. He was rubbed in three times in the course of the day, and in the afternoon was much better. He then got twelve grains of calomel. Next morning, he was salivated, and is now convalescent. In all of these cases opium and calomel were given in the first instance, but in all they were immediately vomited.

IMPUNITY WITH WHICH VEGETABLES AND FRUIT MAY BE EATEN DURING THE PREVALENCE OF CHOLERA.*

To the Central Board of Health.

Gentlemen, — An impression having existed in the public mind that the eating of fruit and vegetables has tended to promote the disease now unhappily prevalent in this country, the persons whose names are undersigned, growers and producers of these commodities, beg leave most respectfully to state for your information, and that of the public in general, that among one thousand and ten persons, employed by twenty-one individuals, being a very small proportion of the number of those connected with horticultural pursuits in the vicinity of this metropolis, the average expenditure of many of the female part of whom does not exceed 1*s.* 6*d.* or 2*s.* per week, and who are engaged in the cultivation and gathering of fruit and vegetables, *no case of cholera has presented itself*, although a great number of those so employed subsist almost entirely on a fruit and vegetable diet; exposed at the same time to the various privations necessarily attached to a life of laborious exertion, and to the frequent consequences attending incautious habits.

Feeling, therefore, most acutely the injury to which the interests of the growers are exposed, as well as those of their labourers and venders, they venture to submit to you the following statement of the number of persons employed, who both are and have been entirely free from the prevailing disease, and beg leave to subscribe themselves, Gentlemen, yours most respectfully,

(Signed) S. Hutchins, Chairman.

24th July, 1832.

Names of Cultivators.		Persons employed.
— Hutchins	Earl's Court ..	90
J. Wilmot	Isleworth	60
H. Fitch	Fulham	70
— Stoneham	Crayford, Kent	75
R. Attwood	Kew	36
H. Knevett	Isleworth	65
C. Knevett	Acton........	55
J. Norris	Isleworth	200
J. Jefferys	Chiswick	40
J. Chapman	Isleworth	85
J. Poupart........	Fulham	30
S. Poupart........	Battersea	23
H. Myers	Ealing	21
C. Allen..........	Old Brentford.	18
W. Attwood	Mortlake	20
A. Dancer	Fulham	40
— Gainsford	Acton........	20
— Foster	Kensington ..	14
— Humphrey	Hounslow	10
W. Wright	Charlton	30
— Barnard	———	8
21 Growers.	Total ..	1,010

All in health but one individual at Crayford, who was unwell when the paper was signed.

It may be added, in confirmation of the above, that in the extensive parish of Fulham in particular, which is principally occupied in gardening cultivation, *not one instance of the cholera has appeared.*

LONDON HOSPITAL.

The Election for an Assistant Physician to the London Hospital, vacant by the resignation of Dr. Macbraire, terminated on Wednesday afternoon in favour of Dr. A. Frampton, who was elected by a large majority. The Candidates were, Dr. A. Frampton, and Dr. C. J. Fox.

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* Communicated by the Central Board of Health.

CALCULI IN THE BLADDER.

ON THE ADVANTAGES OF, AND OBJECTIONS TO, THE USE OF THE

PERCUTEUR COURBE A MARTEAU.

With a Case illustrating its efficiency when the Prostate Gland is enlarged.

By Baron Heurteloup, *M.D.*

To the Editor of The Lancet.

Sir,—To comminute a stone in the interior of the bladder by means of a hammer, is a process sufficiently new and unlooked for, to render it difficult for some at first to believe that it can be exempt from danger; and it is a matter of great difficulty to do away with the unfavourable impressions which are mostly made by those who, without having on the subject very accurate ideas, launch out into conjecture, and reason on hypotheses instead of reasoning on facts.

Allow me, Sir, through the medium of your widely-circulated journal, to rectify some of the incorrect ideas which may have been formed respecting the *percuteur courbe à marteau.*

It has been said of this instrument, in the first place, that it is liable to break in the patient's bladder; secondly, that its beaks were liable to separate, and so prevent it from being withdrawn; thirdly, that during the percussion with the hammer the instrument might vibrate, and cause considerable pain; fourthly, that during the percussion, the fragments might fly against the bladder and injure it: fifthly, that the instrument was not well calculated to seize the stone; sixthly, that curved instruments were useless, since straight ones always sufficed; seventhly, that it could only be employed in cases of small friable calculi; and, finally, eighthly, that it produced fragments but no powder, which rendered the evacuation of the stone more difficult.

I will endeavour to answer all these objections, which, for the most part, were made by the members of the committee, chosen by the Institute of France, to examine my labours. In the numerous opportunities which these competent judges afforded me, I proved to them, *by facts*, that their objections were unfounded; and, in order to render myself clear and methodical, I shall now notice and refute them one by one.

1st. Can the instrument break in the patient's bladder?

I have placed several instruments in the *point fixé* of the *rectangular bed*, and all my efforts to break them have proved unsuccessful. When I employed a hammer of *considerable weight*, I succeeded in forcing the branches apart, but only when I placed between them some tough, unyielding substance, such as a piece of wood or steel. Whenever I acted upon a calculus, however hard, it always gave way. I asked several persons to use their utmost efforts to break an instrument; but their attempts were equally fruitless. These experiments prove, that it is not possible to leave a portion of the *percuteur courbe* in the bladder; for the most determined resolution to produce a fracture of this instrument, outside the bladder, is unavailing. This will be readily conceived, when it is known, that no part of it is tempered.

2ndly. Are the beaks liable to separate and to prevent the instrument from being withdrawn?

Without doubt—if, as I said in the former experiment, a hammer be employed not in proportion to the power of resistance of the instrument. But, as I never operate on a patient with an instrument which has not previously been submitted to *repeated trials* with a *hammer twice the weight of the one employed in the operation*, it will be admitted that a separation of the branches need not be feared, since there is not in the hand of the operator sufficient power to produce it. All will, I think, admit, that it was not possible *at once* to perceive all the properties which were required to render this a good and serviceable instrument, or to discover what was the most advantageous construction to be adopted in its numerous details; it was only by *long* and *persevering study* that such a knowledge was to be acquired, and this explains why, in one of the first attempts that I made with the *percuteur*, the branches were warped. At that time I was inexperienced,—I employed a hammer much too heavy, and the instrument itself was imperfect, and bore but little resemblance to those which I now make use of. The proof of this is, that in the presence of the committee chosen by the Academy, I comminuted hard and voluminous calculi, without the instrument yielding in the least. For upwards of a year, I have been constantly engaged in seeking to distribute the force equally and advantageously in the different parts of the *percuteur*; for this instrument being restricted to the narrow limits of three and a half or four lines, it is in this equable distribution (so difficult to obtain) that its power and perfection consist. Sixty or seventy different models have been constructed, in order to discover in which the greatest power for breaking could be united to the greatest delicacy for seizing. And it is only after making the successive alterations which these trials suggested, that the instrument was brought to its present state of perfection.

I have studied the force of resistance of the *percuteur*, as regards its construction and the bodies to be acted upon. I have submitted it to varied trials with hammers differing in weight, lever, and form, with solid and elastic handles; and, lastly, I have examined, on real calculi, the comparative effect of slow or sharp, gentle or hard blows; and I discovered that even this part of the operation, apparently so simple, had its difficulties and secrets. To understand this better, it is only necessary to consider the ease and nicety with which a practised hand detaches and works large pieces of flint, whilst another man, howsoever clever, if without practice, fatigues himself by efforts as clumsy as they are useless, and for a long time vainly tries to execute what appeared so simple and easy. Thus in conclusion, we find that there is no danger of producing the separation of the branches of the *percuteur*: 1st, From the far greater perfection of the instrument as regards its construction. 2dly, From the incapability of the operation to cause this separation, on account of the lightness of the hammer he employs. And 3rdly, From the additional knowledge acquired of the manner of striking that is most likely to break down calculi, which, from their shape and toughness, would perhaps resist the efforts of an inexperienced operator. As a last proof, I might state, that I operate nearly every day with the *percuteur* without ever finding the branches give way.

3dly. During the percussion does the instrument vibrate so as to cause considerable pain to the patient?

This objection is a very natural consequence of the series of ideas which must have been formed concerning an operation, consisting in conveying into the interior of the bladder the powerful impetus of a hammer. Were I not the author of this operative process, it would be one of the first arguments that I should adduce in objection to it. I was not therefore surprised when the members of the Institute, who were examining the instrument, not only expressed doubts as to the fear of vibration, but intimated also the possibility of the entire instrument being made to move forward by the blows of the hammer. In order to reply to these objections, I put the instrument repeatedly to the test before the members themselves; and these trials were so much the more convincing and conclusive from being made comparatively with instruments acting by *progressive trituration*, such as the *perce-pierre*. To the surprise of the examiners, the result of them was, that with the latter instrument, which they supposed acted upon stone without causing much vibration, the bladder of the subject and water it contained, were subjected, during the action, to a considerable tremulous motion,[*] whilst hard blows dealt on the *percuteur* with a hammer, produced no vibration whatever. Although this may at first cause surprise, a moment's reflection will render it clear. An instrument acting progressively on a stone can only work efficiently by a rotatory movement, produced by the agency of a pulley; this movement must necessarily cause vibration, which is transmitted more or less to the organ in which the operation takes place. By *percussion*, on the contrary, the only manner in which the instrument can move is from before backwards, and no vibration can ensue, since no lateral movement takes place. The movement from before backwards, however, is completely obviated by placing a steel wedge, constructed for the purpose, between the anterior curve of the steel bar forming the *fixed point*, and the corresponding angle of the mortice in which this bar plays. I think I shall have given sufficient proof of the immobility of the *percuteur* during the percussion, when I have added, that during the action of the hammer, the patients never give the least sign of suffering, but, on the contrary, all agree in stating that this is precisely the moment of the operation which is most exempt from painful sensations, and that it is considered, as it were, a moment of rest. This will be fully concurred in by those who have seen the instrument act outside the bladder, and who have placed their hand in contact with that portion of it where the comminution of the stone is effected, and have felt its perfect immobility. If, therefore, the hand placed in close contact with the instrument does not experience the least disagreeable sensation during the action of the hammer, how can the bladder be in any way exposed, which, by being previously distended with water, is in no sort of contact with the instrument?

4thly. During the percussion, do the fragments of stone fly against the bladder and injure it?

The bladder being filled with water, we should be rather inclined to consider this objection as chimerical; for, physically, a body propelled through this fluid, is almost entirely deprived of the power of injury. This will be clear to those who, under the water, have tried to deal a blow with any degree of force; the most violent efforts have but a trifling effect. I shall not dwell any longer on this point, having said enough to prove, that if even pieces of stone were propelled by force with the blows of the hammer, no inconveniences would result from it; but this *propulsion does not take place*, and consequently the objection of the possibility of injuring the bladder falls of itself, since there can be no effect without cause. The members of the commission paid special attention to this point, and I proved to them, by acting on hard and very dry calculi, well calculated to exemplify the above-named objection, that the broken pieces fall from the instrument almost perpendicularly, and receive scarcely any other impulse than that of their own specific gravity. I also proved, that these fragments are only projected at all forcibly when I begin the percussion by hard and repeated blows, without having first by softer strokes, loosened the layers of the stone, and so prepared it to fall more readily to pieces. In operating, I always commence by gentle blows, so that this objection is in every way answered.

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[*] It must here be remarked that, in the experiments to which I allude, M. Leroy (D'Etoiles) was the operator, and he made use of the *rectangular bed* and *fixed point*. When the *perce pierre* is employed with a *chevalet* (hand-vice), held by an assistant, there not only must be very considerable vibration, but with such unsteady means of support as a hand, the entire instrument must unavoidably be in continual movement.

5thly. Is the *percuteur courbe à marteau* ill calculated to seize the stone?

I cannot better answer this objection than by following the example of a celebrated philosopher, who, to prove the existence of motion, *walked*. Since *lithotripsy* has engaged attention, some persons have received, and still retain, an impression that an instrument with *two branches* cannot seize a stone properly, so as to break it down, although I have repeatedly proved the fact, by the use of the *brise-coque,* that such instruments are much better calculated to seize oval and flat calculi, than those composed of a greater number of branches. The percuteur, which has been similarly found fault with, furnishes another proof, that of all instruments, those with two branches *seize* calculi (especially oval and flat ones) with the greatest rapidity and facility; *hold* them with the greatest security, and require to accomplish this, the least searching and manœuvring, and are consequently least injurious to the organ. This objection, having some weight in Paris, it was named to me by the Members of the Institute, but I unanswerably proved to them that it was unfounded, by first making this instrument act before their eyes, and seizing a stone 20 or 30 times in the space of a minute, and afterwards by renewing the experiment in the bladder of a dead subject, and seizing *immediately* at their command the stone. These trials were repeated 50 or 60 times before the members of the committee, with equal success. I would state as a further proof, that almost every day some of the most distinguished surgeons in London see me operate with the *percuteur*, and I do not think any of them can say that they ever saw me introduce it into the bladder without directly laying hold of the stone. Proofs of this have lately been given *publicly* at the Greenwich Hospital, in a case which I treated there with complete success. This fifth objection is therefore fully cleared up.

6thly. Are curved instruments useless, and do straight ones always suffice?

Admitting that it is always possible to introduce straight instruments into the bladder, this is no reason whatever why curved ones should be deemed unnecessary; for, in *lithotripsy* there is more to consider than the mere introduction of an instrument; it is requisite, to seize the stone easily, to comminute it rapidly, and avoid irritating the bladder. It is to this end chiefly that the *percuteur* was constructed, and a surgeon, accustomed to use it, will I think find, that he derives these advantages from it. The curve, which is merely a *consequence* of what the instrument was intended to fulfil,* is a secondary property, although it becomes one of the greatest importance when straight instruments cannot be introduced into the bladder, or at least only with difficulty, and consequently with pain and danger to the patient. To contest the existence of such cases is confessing a want of experience, for they are by no means rare. I myself have met with eight or ten such cases, some of which I could not treat from want of a curved instrument. With this letter I have forwarded proof of this, in the case of a patient in whose bladder it was impossible to introduce a straight instrument. The curve is therefore a useful and valuable property of the *percuteur*.

7thly. Can this instrument be employed only in cases of small friable calculi?

Such an objection as this could only be made by those who judge at once, and do not take the trouble to perform trials, so as to speak from experience; or else by those who have made essays with imperfect instruments, and conclude accordingly. I sufficiently proved how completely inapplicable the objection was to the *percuteur*, by the experiments which I made before the members of the Institute; in three experiments I completely pulverised very large, and extremely hard calculi, with which I was furnished by Dr. Souberbielle, who, some years before, had extracted them by lithotomy. The cases in which I have operated, and daily operate, tend still more strongly to do away with this imaginary objection. When a stone, besides being very large, is round—a shape which presents very great resistance to the action of the *percuteur*, an excavation is first made with the *evideur à forceps*, and when it is thus reduced to a sort of thick shell, it yields readily to the blows of the hammer.

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* Refer to my work, *Physical Properties of Calculi*, page 155.

8thly. Does this instrument produce fragments and no powder, and thus render the evacuation of the stone more difficult?

The Baron Dupuytren particularly named and attended to this point; and I proved to him and his colleagues, by repeated operations on the dead subject, that a much larger quantity of powder was produced by the action of the *percuteur*, than by that of instruments *acting progressively* on the stone; for when a portion of stone is seized between the beaks or branches of the *percuteur*, it is not only crushed, but the considerable portion of it which is directly between the branches, is forced out by the blows of the hammer in a sort of pulp, which, when dried, is found to be fine powder; the fragments formed are, in general, smaller, less numerous, and more regular in shape, than those produced by any other instrument; the reason of this is, that the effect of percussion on a calculus is to loosen, and at last completely to disjoin, its layers, and in this manner cause it to fall to pieces; and when these pieces are again taken and acted upon by the instrument, they usually tend, by their composition, to assume a cubical shape. With the *perce-pierre* it is very different; for it is clear that the pieces which result from the action of an instrument that bores holes in a stone, and consequently leaves sharp edges, must necessarily be both sharp and irregular in form.

Such are the observations which I think right to publish, with the hope of doing away in some measure with the false ideas which may have been formed respecting this new and important operative process. I hope I have been sufficiently clear and concise to be understood by all; and that I have been able to render more evident the value of an instrument which, by the numerous successful cases in which it has already been employed, and the testimony given in its favour by so many distinguished members of the profession who have seen it in use, seems to deserve attention, and call forth the careful examination of men of science. I have the honour to be, Sir,

Your obedient servant,
Baron Heurteloup, D.M.P.
1, Vere Street, Cavendish Square.
July 28, 1832.

Case of Mr. S., alluded to above.

Mr. S., residing in London, fifty-nine years of age, rather stout, but of a good constitution, whilst in the country about a year ago, observed, after a day of hunting, that his urine was deeply tinged with blood, and that he voided it with difficulty. This continued for three or four days, accompanied by a frequent and urgent desire to make water. Mr. S. consulted his medical attendant, who, by an appropriate treatment, subdued this irritable state. Four or five months afterwards Mr. S. had a second attack, but of a more serious nature. Besides considerable hemorrhage, there were fever and considerable pain on making water, and the urine became thick, offensive, and catarrhal; these symptoms continued with little abatement for nearly a month, when Mr. S. came to London and consulted Dr. Prout, who allayed the pain and irritability of the bladder, and obtained an almost complete cessation of the catarrh; Dr. Prout then called Mr. Brodie in consultation, who sounded the patient, felt a stone, and conjointly with Dr. Prout, did me the honour to place the patient under my care.

When I proceeded to sound Mr. S. with the *recto-curvilinear catheter*, I found that I could not make the instrument advance any further than the triangular ligament; I placed the catheter in Mr. Brodie's hands, who found it equally impossible, notwithstanding the most varied and careful manœuvres to introduce it into the bladder. I gave up making any further attempts with that instrument, and concluded that it had failed on account of its small diameter causing its extremity to become entangled in the folds existing at the superior part of the membranous portion of the urethra; I therefore employed a more voluminous instrument, but with precisely the same curve as the *recto-curvilinear catheter*, and it immediately passed into the bladder. I distinctly felt a moveable calculus of small size. The bladder retained a considerable injection of water, but the *bas-fond* was exceedingly narrow; so much so, that I could not turn the curve of the catheter, either to the right or to the left; the instrument had not more than an inch play from the anterior to the posterior part of the bladder, and sometimes this movement was completely prevented. When the instrument was drawn towards the neck, with the curve directed upwards, the hand experienced a sort of shock such as would be caused by the angle of the curve sliding down a sort of *elevation* or *step*. Wishing to ascertain whether a straight instrument could be introduced, I employed a smooth metal sound of about three lines in diameter; but in spite of all the most persevering and careful manœuvres, I could not succeed in passing it into the bladder; its point always struck against the lower part of the prostatic portion of the urethra, where there existed the sort of elevation like a *step*. Finding that this patient could not admit straight instruments, I considered myself fortunate in possessing the curved *percuteur*.

With that instrument I thought I might undertake the case with confidence. The operation was performed three days after the sounding; the stone was immediately seized and broken with the hammer; two or three fragments were afterwards taken and comminuted, and the instrument was withdrawn. An injection brought away a considerable portion of detritus, and Mr. S. voided in the course of the three succeeding days the remaining portions; I introduced the *percuteur* a second time without feeling the smallest fragment; three days after I sounded him with the greatest care with the same result, and, finally, Mr. Brodie having also sounded him without feeling any stone, we had the satisfaction to inform the patient that it was completely extracted.

Mr. Brodie and Dr. Prout were present during all the steps of this case.

Reflections.—This is certainly a case worthy of remark, inasmuch as patients in whom there is so decided an obstacle to the introduction of straight instruments are seldom met with. The end of the straight-sound invariably struck against the prostatic *cul de sac*, and notwithstanding every possible manœuvre to disengage it, I could never succeed in surmounting this obstacle. This affords another proof, that the curve in the *percuteur* is not one of the least important of its advantages, since without it, in such cases, its other properties would be useless, for before we can comminute a stone, it is necessary to arrive in the organ that contains it.

The stoutness of Mr. S. was the principal cause of the difficulty in introducing the straight instrument; and, in most cases, it is the principal cause. The bladder, in these patients, is very much raised in the pelvis, by the quantity of fat which accumulates in the lower part of this cavity; the neck of the bladder is consequently also very much raised, and as the perforation through the triangular ligament does not alter its position, it evidently follows, that a straight sound, which has passed through it, is inconveniently directed to pass through the neck of the bladder. If, in these cases, the prostatic *cul de sac* be at all enlarged, either naturally or by the enlargement of the prostate, it is clear, that the difficulty of reaching the bladder with a straight instrument is the greater. It is in such cases that the curved *percuteur* becomes of absolute necessity.

THE LANCET.

London, Saturday, August 4, 1832.

A Correspondent inquires why we have been so silent respecting the second irruption of cholera in London, and speaks of the omission as a dereliction of public duty on our part. He assumes that as soon as the second epidemic commenced we were bound to announce, and comment on, its existence, with the double object of warning foreign nations against the dangers of trading with us, and of contributing towards domestic safety by affording our fellow-citizens a true view of the magnitude of the existing danger. We proceed to state the reasons of our silence, and to reply to the grounds on which our correspondent supports his complaint.

On the first occurrence of malignant cholera in London, our readers will remember with what rabid mouths and "*a la lanterne*" outcries all those were assailed whose principles or occupations involved them in announcements of the actual state of the disease. Every man who dared to speak the truth was pointed out as the enemy of his country and its commerce, and was abused as energetically as if he had been the actual introducer of the pestilence. The reporters of cases were openly accused of forgery, and of being parties to a system of bribery; nay, it was even hinted in a zealous mercantile periodical (not the *Times*) that the cholera hunters, for want of genuine game, were trying what they could do with poison. Well, the disease worked on in its steady way. It silenced, effectually, a few of the libellers, and the rest grew ashamed of their occupation, and sneaked into their native darkness out of the basking places which they had occupied in the sunny columns of the *Times*, and other public journals. Meanwhile the epidemic departed,—but to return in a few weeks. As soon as we became acquainted with its second visit, we determined to ascertain, as far as possible, how far the suppression of reports or notices of the disease would influence the extent of public alarm. This experiment we have now fairly tried. Possessing ample means of procuring the most correct information from every parish in the metropolis, we were soon enabled to perceive an almost incredible extent of exaggeration and terror on the part of the public. One gentleman of undoubted veracity, but large imagination, about ten days since, accosted us with paleness and fright in his countenance, and informed us that he was just leaving town "because three hundred persons had died of the cholera in his parish the day previous." Gentle and simple, young and old, rich and poor, all talked of nothing, dreamed of nothing, but the cholera: many persons, we know, actually frightened themselves into the disease, and all this at a time when the deaths from cholera did not exceed a score and half a day in the whole metropolis.

If then we admit, what cannot be denied by any sane individual, that public terror is an evil of the most serious character during the prevalence of this disease, that its influence on the public health is of the most pernicious description, it follows that the wisest plan is the publication of reports stating the true account of the evil.

But independent of the salutary effect produced by such reports in checking alarm while the disease is moderate (and no pestilence is so destructive of which rumour will not magnify its virulence tenfold), they are calculated also to work much good, even under more auspicious circumstances. While suppression of the truth is practised, the heedless traveller falls into the theatre of contagion and miasm, without any warning of his danger. In an infected town, the inhabitants are not enabled to distinguish the healthy from the insalubrious quarters. Lastly, in withholding correct information, we are unable to trace the pathways and habits of the malady, and allow it to quit the country without those statistical records which are so invaluable in the scrutiny of all pestilential diseases.

For these reasons, and we could add many others, we agree with our correspondent as to the necessity of breaking silence, and as to the propriety of the publication of official reports of the state of the epidemic. His Majesty's Government has, however, determined to withhold them for the present, on what grounds we are utterly at a loss to understand. The quarantine of Spain is as strict as ever on the port of London. Holland is now infected from north to south, *despite the most rigid quarantine.* The disease has reached America, and France has all but abolished her sanitary restrictions. So that with Spain we cannot be worse, and with France, Holland, and America, we are upon equal terms. At any rate, it is now quite apparent that quarantine laws can no more restrict cholera than they can keep off a descent of congreve rockets. Where then, we may ask, is now the advantage of the secresy which is practised by the Ministry ?

With respect to the document issued by the market gardeners, which we publish in another page, we are bound, on consideration, to say, that it by no means proves the case they desire to establish, inasmuch as all the *fruit-fed* individuals whom they specify, have hitherto lived beyond the bounds of the epidemic influence. Had a similar number of exemptions been recorded *in a place infected with cholera*, the evidence would then be conclusive for the gardeners. At present it is by no means sufficient to induce us to overlook the fact, that in three-fourths of the numerous cases which we have met with in the last month, fruit was clearly ascertained to have been the *exciting cause* of the attack.

We are happy, however, to be able to add, that the disease is so much on the decline in the metropolis, that in all probability, the early autumnal fruits may be consumed with the usual impunity. *The deaths in the metropolis have not for the past week averaged more than thirty a day*, and there remain now, under treatment, not more than 350 cases. The document to which we have alluded, however, may fairly be allowed to have its effect in moderating the alarm felt in districts which the cholera has not yet visited.

The correspondence which has lately passed between Dr. Mackintosh of Edinburgh and certain of the "powers that be," relative to the approaching appointment of a professor to the chair of medical jurisprudence in that University, has been laid before us. With nine-tenths of the pamphlet we have nothing to do; so much of it, at least, is altogether of a personal nature; though we have no objection to make to the form which the question therein assumes. Dr. Mackintosh is the champion of a free and just election, and as every attack and defence must have a leader, we are rather gratified than otherwise at the spirit and boldness, though marked by much egotism, with which the charge has been headed by him—in print, at any rate. Dr. Mackintosh is a very able and zealous man, and can fight energetically when his sword is drawn. We disregard, however, after a careful perusal, nine-and-twenty of his pages, and from the thirtieth select the only passage which awakens our attention to this controversy. It is contained in a petition to the House of Commons from eleven lecturers on different branches of medicine in the northern capital, and furnishes the public in a short space with the whole gist of the question in dispute.

"It is with the deepest and most unfeigned regret that your petitioners find themselves obliged to call the attention of your Honourable House to the late appointments which have taken place to those Professorships in the University of Edinburgh which are in the gift of the Crown. Your petitioners are prepared to prove, that the late appointments took place *in a hurried and clandestine manner*, thereby preventing the claims of other members of the profession from being duly considered; and it is the general belief of the profession that the chairs were bestowed *more as the reward of political than of medical zeal.* Your petitioners approach your Honourable House, not for the vain purpose of making complaint, but to pray that, in your wisdom, you will *institute an inquiry into the late appointments*; that you will take such measures as to prevent improper appointments *in future*; and that as there is at present a vacancy in one of the medical chairs, your Honourable House will take measures *to prevent the interests of medicine and science being sacrificed to political intrigue.* And your petitioners will ever pray. (Signed)

"John Lizars, Prof. of Surgery to the R. C. Surgeons, &c.
John Thatcher, M.D., Lect. Midwifery.
R. Knox, M.D., Lect. on Anatomy.
J. A. Robertson, M.D., Lect. on Mat. Med.
Alex. J. Lizars, Lect. on Anatomy.
W. Fergusson, Lect. on Surgery.
John Mackintosh, M. D., Lect. on Pathology and Prac. Phy.
John Murray, M. D., Lect. on Chemistry.
T. J. Aitkin, M. D., Lect. on Instit. of Med. and on Mat. Med.
W. Campbell, M.D. Lect. on Prac. of Phy. and Midwifery.
John Fletcher, M.D. Lect. on the Instit. of Med."

This is a most serious charge, but what is there in it that is rare? Why "the interests of medicine have been sacrificed to political intrigue" in this country for centuries. The immediate cause, however, of this protestation is a confident rumour that the chair of Medical Jurisprudence, vacant by the appointment of Dr. Christison to the chair of Materia Medica, is to be given to Dr. Maclagan, through the influence of the leading Whigs of Edinburgh, now in office, whose interests at the various elections which have taken place for some years back, the Doctor has had many opportunities of warmly and efficiently supporting, and whose political views he has ardently supported at the public meetings. Warned by the past, the lecturers are now in arms, and in anticipation of an undue election, petition the House of Commons to interpose in this particular instance between the unholy connexion which politics and science have so long maintained.

Of Dr. MACLAGAN we may add our opinion, that with the desirable qualifications of being a good speaker, and a man at any rate of liberal *professions*, he wants almost all the most important requisites for filling such a chair as that to which his advancement is threatened. The appointment of Dr. MACKINTOSH would scarcely be open to any objection but that of the mode of election. Let us hope that "the Crown" will reconsider its present intentions.

THE pamphlets of Drs. CRAIGIE and LATTA, of Leith, have reached us. Dr. CRAIGIE, in a letter on the treatment of the cholera which he published on the 14th of June in the *Edinburgh Courant*, did not make that "honourable mention" of Dr. LATTA, as the first physician who tried venous injections in cholera, which Dr. LATTA and his friends considered to be due from one who was writing at that particular time, and under existing circumstances. The omission was accordingly noticed in an anonymous letter in the *Edinburgh Observer*, as very unfair towards Dr. CRAIGIE. This letter contained remarks which Dr. CRAIGIE, in a subsequent epistle, spoke of as "false and unfounded." The writer of the anonymous letter proved to be Mr. JOHN MITCHELL, Jun., a ship-broker, of Leith, who confessed the authorship for the purpose of "demanding an explanation" from Dr. CRAIGIE as to the above two offensive words. An interview took place, casually, in the street, when a trifling assault with a child's whip was believed by Dr. CRAIGIE to have been committed on him by Mr. MITCHELL, who is an intimate friend of Dr. LATTA. The parties were afterwards bound over to keep the peace, although (most unaccountably to the reader) Mr. MITCHELL denies ("does not admit") that any assault took place, declaring "that he had not any whip in his hand when he asked for the apology."

This avowal certainly puts to rest the whole affair, but it is one which a peruser of the pamphlets is little prepared to meet with only in the last but one of forty-two pages.

We are disposed to doubt the justness of the original quarrel; at any rate its right to be made a subject for public discussion. Our impression is, that although Dr. LATTA may have thought he had reason to be surprised at the omission complained of, Dr. CRAIGIE did not commit an intentional offence. As for the part taken in the affair by Mr. MITCHELL, the right of a strictly anonymous correspondent to assume that he is personally affronted by any man, is perfectly new to us, and one which we think no court of honour would for a moment allow. We are glad the assault is disavowed. Had it taken place, it would have been an unwarrantable outrage both on the man and the physician. Dr. CRAIGIE was not bound to carry his inquiries as to the name of the author farther than he did.

In perusing the letters which have passed, we recollect so little that is offensive uttered by the two physicians, that either party we are sure may advance the hand of friendship without the slightest compromise of personal dignity. As for the neglected compliment to Dr. LATTA, that gentleman can afford to lose it, and no man will think so more readily than his opponent.

The importance of meteorology as a constituent in the etiology of diseases has rarely been duly appreciated by the medical profession; the most ordinary occurrences of life, however, afford us abundant proof of the great influence exercised by atmospheric changes on health. We advert to the subject here, to recommend to the reader's notice the essay of Mr. James Espy on meteorology in our present Number. It is highly ingenious, and points out accurate and easily attainable means for investigations which are replete with interest to medical men, and require but little expense or trouble to conduct them. We hope to attract attention to this much-neglected branch of science.

The length of the lecture on the nervous system of by far the most important of all our domestic animals, which we this day publish, precludes us from giving in our present Number several valuable papers which we had marked late in the week for insertion. The extent of Mr. Youatt's luminous and admirable sketch of a most intricate, and, usually-esteemed, mysterious subject, will be regretted, however, by none of the thousands of attentive readers who are following this highly philosophic lecturer in his course, through the medium of our pages. It would have been great injustice to all parties, indeed, to divide the lecture at any part. To allow its completion, in fact, in the most perfect manner, we delayed its publication from our last Number, and now submit it to public perusal as an excellent example of deep research, original observation, acute reflection, and a terse and clear style of expression, happily combined within the compass of one hour and a half's discourse, on a highly abstruse and hitherto most wretchedly investigated subject. The lecture, in truth, is quite a model in its way, and, from the care and laborious investigation which distinguish it, is one of the most valuable which has ever been presented to the readers of this journal. Indeed the entire of this *unique* course, demands the high estimation of the profession, and its value, we are rejoiced to find is, as regards the portion already published, fully appreciated by every medical reader who derives advantage in his daily labours from the services of the invaluable domestic animal which is the chief subject of the learned lecturer's precepts.

AN INQUIRY INTO THE
ORIGINAL COUNTRY
OF THE
SOLANUM TUBEROSUM (Potato).

By John Edward O'Reilly,
A.B.M.B. & M.R.C.S.L.

The writer of the article Potato in *Rees's Cyclopedia* asserts, that it was brought into Europe from Virginia by Sir Walter Raleigh in the year 1623. This is probably on the authority of Wildenhow, who makes the like statement; and Sir Joseph Banks, in a paper published in the Transactions of the Horticultural Society, arrives at almost the same conclusion, save that he fixes the year 1586. Humboldt, in his New Spain, book the 4th, also appears to think that the first English settlers found it in Virginia; and to these authorities may be added the illustrious name of Jefferson. (*Notes on Virginia.*)

The views which I shall attempt to sustain in this paper are, first, that the potato was wholly unknown to the original inhabitants of Virginia; secondly, that Peru, and probably Chili, were the countries where the first European visitors of America found it; and, thirdly, that there exists some probability, very slight I admit, that this plant was not quite unknown to the ancients.

The principal arguments in favour of its having been found native in Virginia are—That Thomas Herriot, who visited Virginia with the first discoverers, describes the potato as the "openawk;" that Gerard, in his Herbal, published in London in 1586, describes the potato, or, as he terms it, openawk, as being an inhabitant of Virginia; and, what I do not find mentioned by Sir Joseph Banks, John Parkinson, in a very elaborate work entitled Theatrum Botanicum, published in London in 1640, corroborates Gerard.

The statements of Wildenhow, and of Rees' Cyclopedia, are manifestly inaccurate. Raleigh having been beheaded in Old Palace Yard on the 29th of October, 1618, could not bring potatoes into Ireland in 1623; and indeed Dr. Barton Smith, of Philadelphia, asserts that Raleigh never was himself in Virginia.

I have very carefully examined Herriot's work, published in De Bry's collection (Ed. Frankfort on the Main 1690), and do not think the openawk described by him is the potato; and I am the more confirmed in this, as, though Herriot gives no figure of this plant, yet De Laet does, in his Novus Orbis (*Editio apud Elzevirios* 1633), which assuredly is quite a different plant from our potato; moreover it is not at all proved that Novembega, where Herriot found the openawk, was Virginia.

Gerard and Parkinson were both compilers, and participated in the mistake, as they both call the potato the openawk.

Mr. Jefferson does not state his authority. He says, "Most probably they were natives of more southern climates, and handed along the Continent from one nation of savages to another." Now this account is at variance with Humboldt's positive assertion (lib. cit.), "That there is not a single fact connecting the history or races of the two Americas."

Colonel Benjamin Hawkins, who resided as United States agent to the Creek Indians, assured Dr. B. Smith (Nicholson's Journal, vol. xxviii) that the Indians say they received it from the Europeans; and Capt. John Smith, who visited Virginia in the early part of the 17th century, says that the potato was brought to that colony in 1613 by the ship Elizabeth from England.

I shall now proceed to demonstrate where the first settlers from Europe in America found the potato. Peter de Cieza, who resided seventeen years in Peru, and who wrote in 1541 (translated from the Spanish by Stevens), says, in describing the neighbourhood of Quito, "Besides the maize or Indian wheat, there are two other sorts of food natural to the country; the one they call pappas, which is like large pig-nuts, and when boiled are as tender as large chestnuts, and has neither shell nor core, but grows under ground like the pig-nuts aforesaid. This root bears a flower exactly like a poppy."

This account of Cieza is confirmed, as to its principal bearings, by the Inca Garcilasso de La Vega, whose mother was a native Peruvian. (Royal Commentaries of Peru.) The authorities on this subject are very numerous, and can be all found in the Fagel Library of Trinity College, Dublin, and are all corroborative of the account of Cieza. I shall merely mention some of the principal.

Gulielmi Pisonis Indiæ utriusque de re Naturali et Medica, libri xxx. Historia Nova Plantarum, Animalium et Mineralium Mexicanorum, a Francisco Hernandez Histoire Generale de F. Lopez de Gomara.

Mr. Humboldt found in Chili a wild potato differing certainly from ours, but cultivation frequently makes a very great change in plants.

The third proposition is founded on the description given by Theophrastus* of a plant which he calls the arachidna (*αραχιδνη*). His description wonderfully applies to the potato, but that he says it is leafless. (*φυλλον δε ουτερον εχει τουτων ουδ' ομοια τοις φυλλοις.*) This, Clusius, an eminent botanist of the 16th century, endeavours to get over, by supposing an inaccuracy in Theophrastus, and stoutly maintains the arachidna and papas punanum (potato) to be one and the same. (*Rariorum Plantarum Historia.*)

The arachidna is also described by Pliny, but he copies Theophrastus literally. (*Plin. Secund.*, lib. xxi. cap. 52, *cura Valpy.*) His commentator styles it—"Herba hoc ævo ignota."

Since writing the above, I find that Don Joseph Pavon, in his Flora Peruviana, fully supports Cieza, and the other authorities cited above, as to Peru and Chili being the original country of that plant, which many a one of my poorer countrymen may address—"Salve, magna parens."

Dublin, June, 1832.

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* Three hundred years before the Christian era.

CORRESPONDENTS, &c.

Mr. Joseph Hickman. The works of Beck and Christison. We do not know of a good work on the subject of the analysis.

ERRATA.—In Mr. Jackson's case of melæna page 498, line 2, for "quorum" *read* quarum; line 35, for "sumat" *read* sumitur; line 45, for "aque linnam" *read* aquæ cinnam; col. 2, line 9, for "enemata" *read* enemate. These errors occur only in a part of the impression.

In the note at the end of Dr. Elliotson's lecture, page 518, line 13, the *comma* after the word "absence," should be transferred to the close of the word "intervals," in the next line. The error is in a few copies only.

THE LANCET.

Vol. II.] LONDON, SATURDAY, AUGUST 11 [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,

BY

MR. YOUATT.

Lecture XXXIX.

THE FIFTH PAIR OF NERVES, THEIR DOUBLE ORIGIN AND FUNCTION.—THE STRUCTURE OF THE SPINAL MARROW, AND DOUBLE ROOT OF THE SPINAL NERVES.—THE ORIGIN OF THE FIBRES CONVEYING SENSATION FROM THE SUPERIOR SURFACE OF THE CHORD, AND OF THOSE CONNECTED WITH MUSCULAR MOTION FROM THE INFERIOR SURFACE.

The Double Origin of the Fifth Pair of Nerves.—The fifth pair of nerves has been described by our anatomists as springing by a multitude of filaments from the crura cerebelli, and forthwith running for security into the cavernous sinuses, and there suddenly enlarging into and forming a ganglion. Human anatomists, however, had long spoken of the double origin of the fifth pair, and the researches of some modern physiologists, and particularly of Sir Charles Bell and Mr. Mayo, have attached great importance to this double origin. The experiments were made on our patients, and therefore we have a legitimate right to refer to them, and to reason upon them.

The Difference between the Two Roots.—The double origin of the fifth pair is plain enough in this dissection, as well as the inequality in the size of the two roots, the inferior being very much smaller than the superior; likewise the difference in the structure of the two, the fibres or fibriculi of the superior root being much larger than those of the inferior. On examining a brain that has been long macerated in alcohol, and thus more easily resolvable into its fibrous structure, the smaller portion can be traced through the pons varolii to the crura cerebri, the prolongation of the *inferior* surface of the brain, and also close to the medullary track in which we have found the motor nerves. The larger portion springs from the lower part of the crura cerebelli, winding from that which answers to the *superior* surface. Again, we find that the fifth pair, or a portion of it, after entering the cavernous sinus, swells out into, or passes through, a ganglion, and, examining more closely, we discover that it is the larger or superior part that forms the ganglion, while the inferior portion passes by the ganglion, and afterwards unites with its fellow to form the perfect nerve.

The Double Function of the Fifth Pair.—These things had been pretty well known to some of the best anatomists; but it remained for Sir Charles Bell to draw from them the legitimate and important conclusion that this is a nerve of double function—of sensation and of voluntary motion. His experiments were simple and satisfactory. He laid bare the root of the fifth pair in an ass, immediately after the animal was killed, and on irritating the nerve, the muscles of the jaw acted, and the jaw was closed. He divided the root of the nerve in a living animal, and the jaw fell. There seemed to be no contradicting this: it had been admitted on all hands, that the fifth was a nerve of sensibility; it was now evident that it was also a nerve of motion. Sir Charles, however, pursued his experiment one step further; he selected the superior maxillary branch of the fifth pair, whose ramifications extend to the muzzle, and he divided it on both sides: the power of elevating and projecting the lip, as in gathering the food, was lost, and in order to open the lips the animal pressed the mouth against the ground, and at length licked the oats from the ground with his tongue.

Mr. Mayo's Experiments.—Mr. Mayo instituted experiments of a similar nature, and although he differed from Sir Charles Bell in some conclusions which he drew respecting the respiratory nerves, yet he arrived at a similar result as to the loss of power in the lips; for when he divided the second and third branches, the lips no lon ger remained in perfect apposition, and the animal ceased to use them in taking up his food. He, however, ingeniously accounts for this by the loss of sensibility in the part, and he tells us very truly, that when the sense of feeling is taken away from a part, the voluntary muscles cannot be efficiently excited. We have a thousand proofs of this, and therefore this experiment of Sir Charles Bell would not be perfectly conclusive.

Some Muscles are supplied by the Fifth Pair alone.—Leaving these eminent physiologists to settle this matter as they please, there are some anatomical facts, and to which Mr. Mayo properly alludes, which incontestably prove that the fifth is a motor nerve; for there are certain muscles of great power, and particularly the masseter, the temporal, and the two pterigoids, which are supplied by the fifth pair alone, and therefore must derive from those nerves their power of motion as well as of feeling.

The probable Roots of Motor and Sensitive Power.—Then as the inferior root arises from the inferior surface of the prolongation of the spinal chord, and (what we shall better understand when we have examined the spinal chord) it passes by, and does not help to form the ganglion, and its fibres are minute, it is probably the motor root; while the larger and ganglionic, and with coarser fibres, is probably devoted to sensation.

Peculiarities and course of the Fifth Pair.—I will not now minutely follow the course of the fifth pair, I shall have to speak of this when describing the different parts of the face; but I must slightly glance at this portion of our subject. Here, in harmony with what we have already seen, we are first struck with the increased bulk of the trigeminus in all our patients, with regard to both roots of it, and more particularly the motor root. It has far more to do in the quadruped than in the human being. It has to give power of motion to the jaws, which are not only the agents of mastication, but in many animals the weapons of offence; it has to bestow additional sensibility on the lips, which in all of them to a certain degree, and in some of them to a remarkable extent, are to supply the place of hands, not only by executing every prehensile function, but being, as it were, the peculiar seat of the organ of touch. The fifth pair of nerves exert also a peculiar influence over the organs of sense, of which we shall have numerous instances, sometimes very interesting, and at other times not a little mysterious; and which influence will necessarily increase with the acuteness of the sense, greater by far in the quadruped than in man, and therefore requiring greater development of the nerves.

Division of the Nerve.—After the ganglion has been formed by the greater root, and the ganglionless part has joined the other, and a perfect, but evidently compound nerve has been the result, the trigeminus very curiously divides into three parts within the cavernous sinus; and each, before it quits the cranium, assumes to itself its distinct investment of dura mater. These divisions or branches are called from the parts to which they are destined—the *ophthalmic*, the *anterior* (superior), and the *posterior* (lower) *maxillary* nerves. I shall give an exceedingly short, perhaps unsatisfactory, sketch of them. If amidst the multiplication of subjects which these lectures embrace, any are neglected, they must be those of pure anatomy.

The Ophthalmic Nerve.—This is the smallest of the three. It is found within the sinus in conjunction with the superior maxillary; which it soon leaves, and passes through the foramen lacerum into the orbit: there it subdivides and forms three distinct branches—*the supra-orbital* (the frontal), *the lachrymal*, and *the lateral nasal* (the nasal). The *supra-orbital* climbs behind the muscles of the eye, giving some filaments to the rectus superior and to the superior oblique, and some to the fatty matter of the eye; and the main branch, escaping through the superciliary foramen, is soon lost in ramifications on the elevator of the superior eyelid, the integument of the forehead, and the periosteum. *The lachrymal*, as its name would import, is chiefly given to the lachrymal gland; some few ramifications are previously sent to the conjunctiva and the ciliary glands of the upper lid, while a distinct twig of it passes out at the angle between the zygoma and the frontal orbital process, and anastomoses with the supra-orbital, and with ramifications from the superior maxillary, and is also lost on the integument and muscles of the forehead.

The Ophthalmic Ganglion.—The third and the largest is *the lateral nasal*. Almost at its commencement we observe the filaments that help to form *the ophthalmic ganglion*, to which I alluded when speaking of the optic nerve. They are more numerous, and more easily traced, in some of our domesticated animals than in others, and the ganglion itself is differently developed, but for what physiological purpose I know not. It is comparatively larger in the ox than in the horse, and sends more nervous filaments to the iris. Four distinct ones may be traced in the ox, but seldom more than two in the horse or the dog. To these filaments, others of the ophthalmic that have not passed through the ganglion, afterwards join themselves, so that the ciliary nerves are also minute compound ones of motion and sensation; but more of this when we treat of the eye.

Course of the Ophthalmic.—After running between the rectus superior and the retractor, it gives a branch to the membrana nictitans, and then takes a singular course. Some ramifications go to the frontal sinuses and the foramina, and which piercing the orbit of the eye for this purpose, are beautifully seen in some young animals, and particularly in the sheep; the main branch then enters the cranium again through the internal orbital foramen, passes under the dura mater, and, in the manner that I have described in the fifth lecture, returns through the cribriform plate, and ramifies on the membrane of the nose, and sends some branches as low as the false nostril and the alæ of the nose.

The Anterior Maxillary Nerve, or Second Division of the Trigeminus—This contains so little that is peculiar to the quadruped, or of practical tendency, that I shall pass it very briefly over. The different situation and conformation of the bones of the face, cause the principal or only variation in the distribution of this branch in the biped and the quadruped. It leaves the cranium through the foramen rotundum; and at the base of the skull gives off small ramifications to the inner canthus of the eye, the antrum and the two posterior molars; also to the cavity of the nose through the spheno-palatine foramen, supplying the lateral portion of the nasal cavity; while filaments are given from the origin of this trunk to the temporal muscle, and a branch also runs along the upper border of the septum to the palate, and a larger branch, which traverses the palate in company with its blood-vessels, passes through the foramen incisivum to the upper lip.

The main trunk of the nerve now enters into the superior and exterior foramen in the hiatus, between the palatine bone and the tuberosity of the superior maxillary bone, and which leads into a bony canal easily to be traced in the horse between the maxillary sinus and the antrum, and appearing as a great pillar passing through the palatine sinuses in the ox;—it traverses this canal, and at length emerges on the face, through the foramen infra orbitarium, and under the levator labii superioris. It no sooner escapes from this canal than it forms the pes anserinus, the goose's foot,—it divides something like the foot of this bird. It anastomoses with, or receives numerous branches from, the seventh pair, and forms a strangely intricate plexus of nerves about the lower part of the face and muzzle, yet one wisely and beautifully contrived, for here, as being the peculiar seat of touch, are required all the flexibility and more than the sensibility of the human hand.

The Posterior Maxillary Nerve, or Third Division of the Trigeminus.—This escapes from the cranium through the foramen lacerum basis cranii, and very soon gives off some important branches.

The First Branch of the Posterior Maxillary, reckoning posteriorly, proceeds backwards below the condyle of the jaw, where it divides into two portions; the first runs up to the parotid gland, ramifies into many filaments, and unites with the seventh pair; it also dips deep into and principally supplies the temporal muscle, and penetrates into and is distributed through the masseter muscle. In this division there would seem to be concentrated the greater part of the motor fibrils of the trigeminus. These are muscles of extensive and powerful motion; and minute anatomical dissection proves to us, that it is with the third division of the trigeminus that the motor nerve is most connected; yet with the motor fibrils those of sensation are doubtless conjoined.

The Second Branch is a long and slender one. It first dips into the pterigoideus muscle, and which is supplied by it, consequently it is here also a motor nerve as well as a sensitive one; it then passes round or behind the tuberosity of the upper jaw, supplies the buccinator muscle, possibly with sensitive fibrils alone, for others go to this muscle from the seventh pair. In the buccinator muscle these fibrils are usually lost, but sometimes a few of them may be traced down to the lower hip.

The Third Branch in the order of its being given off, is *the dental nerve*. This is generally considered as the continuation of the main trunk of the nerve. It passes across the pterigoideus, and enters a canal, *the dental canal* on the inner face of the lower jaw-bone, near the upper edge, and at the bending or angle of the jaw. It takes its course along the interior of the bone, close to the roots of the teeth, and giving branches to each of them as it passes. Emerging through the lower maxillary foramen, it divides into two branches, one of which is spent in numerous ramifications on the outside of the lower lip, and the other in fewer ramifications on the inside of the lip. These are evidently sensitive fibrils, and the power of motion is derived from the seventh pair.

The Fourth Branch in point of order, but which does not enter into the dental canal, is the *gustatory or lingual nerve*, and which is the largest of the four. It is singularly flat, like a little riband. It runs along the inside of the lower jaw, and a portion of it enters a foramen in the jaw to supply the roots of the incisor teeth with nervous influence, but the main nerve, proceeding obliquely downward, gives fibrils to the submaxillary glands, and to the glands and muscles at the base of the mouth generally; these fibres form true plexuses about the salivary glands and the muscles of the tongue; they anastomose freely with the twelfth pair (the *linguales* or motor nerve of the tongue), as this nerve had previously done with the seventh pair (the portio dura). The gustatory branch penetrates the substance of the tongue between the stylo and genio-glossal muscles, passes obliquely to the surface of the tongue, and terminates in the papillæ, and these papillæ thus endowed with nervous influence are the seat of the sense of taste.

The Fifth a Compound Nerve.—It appears then that the fifth nerve has *a double origin, and a double function*—the larger portion, and *that which passes through a ganglion, is derived from the superior surface* of the supposed prolongation of the spinal chord, and the smaller, and *the ganglionless, springs from the inferior surface.* The portion *with the ganglion* is devoted to *sensation; that which afterwards joins it* is connected with *motion;* and thus the same sheath contains sensitive and motor fibrils, and *the nerve is devoted to the double purpose of sensation and voluntary motion.* The sensitive root, however, is so much larger than the other, and the sensitive fibrils so much more numerous, that it may still be called the sensitive nerve of the face; and upon mature examination we are assured that it is the only nerve of the brain whose fibrils bestow sensibility on the face, except a few fibrils from the cervicals which may be traced to the lower part of it.

THE SPINAL MARROW.

Now then, Gentlemen, we shall, perhaps better understand our subject, if, for a little while, we leave the brain, and inquire into the construction of the nerves that supply the frame generally, and direct the voluntary motions of the muscles of the body, and bestow sensibility on every part; and these, as every tyro knows, can be traced on the trunk directly to the *spinal marrow*, and indirectly to it in the limbs, through the medium of the humeral and femoral plexuses.

Commencement of the Spinal Marrow.—I have already said, that I cannot point out the particular spot where the medulla oblongata terminates, and the spinal marrow commences: but I observe the medulla oblongata decreasing in width; the corpora olivaria in the human being subsiding into the general level, and in the subjects with which we have to do, the comparatively flattened and extended portions at the side of the medulla oblongata (answering to or comprising both the corpora olivaria and restiformia of the human being) also rapidly narrowing, and the diminution of width proceeding until I arrive at the spot, or nearly to it, where this prolongation of the brain quits the cranium and enters the spinal cavity:—this spot, the mouth of the foramen magnum, I consider to be the place where the medulla oblongata ends, and the medulla spinalis commences.

Form and Appearance of the Spinal Marrow.—When exposed to view the spinal marrow has the appearance of a flattened cylinder, somewhat more convex superiorly than inferiorly. It is small compared with the canal through which it runs. The brain completely fills the cranium. It does so in sickness and in health, for however emaciated the frame generally may become, the absorbents spare the brain; or if from local disease a part of it wastes, the vacuity is occupied by some fluid. It was necessary that concussion should be avoided in so important an organ:—there could be no joints in the cranium, they would have interfered with its requisite strength, and concussion and injury were obviated by the cranium being always full. But the spine is composed of a series of joints;—the dorsal vertebræ admit of as much motion as is consistent with strength, and the cervical necessarily possess considerable motion. If the canal was perfectly occupied by the marrow, the chord would be seriously compressed and injured by the various, and rapid, and extensive flexures of the vertebral column, and particularly of the neck.

Comparative Size of the Marrow.—It varies, however, in bulk in our domesticated animals. It is comparatively larger in the quadruped than in the biped, and that with evident reference to the different muscular power of each. It is more developed in the horse than in the ox or sheep. Notwithstanding the peculiar width of the medulla oblongata in the latter, the spinal marrow itself shrinks to a small chord. It is comparatively larger in the little horse than in the bulky dray-horse, and evidently more developed in the blood-horse than in him who has no pretension to foreign admixture. It seems to have intimate connexion with muscular strength and endurance. There is much in the bulk of the muscle, and more in its situation or the mechanical advantage with which it acts; but there is more yet in the degree of nervous influence which causes the muscle to contract. The conformation of the horse is a very important and sadly-neglected branch of study. I am sorry to say that there are many among us who have no definite or philosophical idea of the connexion between the proportion and the position of certain parts, and muscular action. Those of us, however, who know or fancy we know the most about this, are often egregiously deceived; and the horse, beautifully proportioned, according to our notions, has neither spirit, strength, nor endurance. If we could look into the spinal canal, and especially if we could comprehend the mechanism of the brain, the mystery would be at once unfolded. To the development of the spinal chord in carnivorous animals we must chiefly trace their immense strength. Neither the bulk nor the position of the muscles of the jaws and neck of the tiger will satisfactorily account for the ease with which he will bear away, at full speed, an animal twice as heavy as himself. Mr. Neilson gives an account of a bear steadily carrying a dead horse over a small tree that had fallen across a river. The explanation is to be found in the excess of stimulus with which the muscles are supplied, and that connected with the bulk of the spinal chord, and the corresponding development of the nerves.

Size of the Spinal Chord in different Parts of the Canal.—The varying bulk of the chord in its passage through the spinal cavity will afford us some illustration of this. It nowhere fills the cavity, for the position of the vertebral column, and the form of the canal, are so continually changing, that the marrow would be exposed to compression and injury. In the upper part of the cervical division of the canal it is of considerable size, for it has to supply with nervous influence the numerous and powerful muscles which support and rotate the head. As it proceeds down the neck it evidently diminishes; but when it approaches the lower cervical vertebræ it swells out again, for it has to furnish these plexuses of nerves which bestow sensibility and the power of voluntary motion on the fore extremities. After it has passed the first dorsal vertebra, and the muscles of the spine and the ribs alone are to be fed, it contracts again, and to less than its former dimensions; it loses as it were the lateral column whose function has been discharged; and it becomes diminished to a mere cylindrical chord; but, arriving at the lumbar vertebræ, where the femoral plexuses begin to be formed in order to call into action the muscles of that part of the frame in which the power of the horse seems peculiarly to reside, it once more thickens and widens, and attains a bulk to which it had not reached before:—and, finally, the lumbar vertebræ being traversed, it rapidly and strangely shrinks, until it becomes a mere point, for it has the tail alone to feed. Before the spinal marrow quite disappears, it divides into a great number of chords, some of which soon terminate, while others extend into the coccyx; this portion of the marrow is called the *cauda equina*, from its supposed resemblance to a horse's tail. In every part of the canal the bulk of the canal has reference to the size or the function of the neighbouring muscles; and so the animal principle preponderates in the inferior part of the creation while the extent of intellectual power in the human being more than compensates for the deficiency of brute strength.

The Columns of the Spinal Chord.—It does not require any very minute inspection of the spinal marrow, to observe that there is a mesian furrow running through its whole extent. It is more evident on the inferior than the superior surface, but sufficiently plain on both. It is a very convenient sulcus for the transmission and defence of the blood-vessels of the chord; but it is more than this—it constitutes a longitudinal division of the chord into two equal parts. Observe how readily, in this hardened brain, the chord separates in the direction of this furrow. You see the lacerated portions of minute medullary fibres running across, and, quite as plainly, an exceedingly fine cellular membrane. In fact there are two nervous chords placed in apposition with each other through the whole extent of the spinal canal; connected with, yet distinct from each other, and each discharging its proper function with regard to the side of the body on which it is placed. Some of the paralytic affections of the human frame are illustrations of this; hemiplegia however is a disease that rarely occurs in the quadruped. The medullary bands between the two columns are more evident in the human being, and preserve perhaps a greater sympathy between the two divisions of the chord.

The Lateral Furrows.—Running parallel with the mesian furrow is another on each side, and on both surfaces. It is very distinct on the superior surface of the chord, and may be satisfactorily traced on the inferior one.

It is sometimes peculiarly interesting to observe the slow progress of the most important discoveries. Sir Charles Bell, to whom we are indebted for almost all we know of this portion of the nervous system, was long in detecting these subdivisions; indeed, in the early editions of his admirable system of anatomy, he speaks of a distinction, convenient for the purpose of illustration, but which has no structural existence. He says that "the two lateral portions are divided into an anterior (inferior) and posterior (superior) portion, so that this prolongation has *four* distinct portions." He has since discovered, and it is the key-stone to our knowledge of the structure and function of the spinal chord, that there are *six* distinct portions. Observe this transverse section of the spinal marrow. Can you not trace three distinct chords on each side—a central one superiorly and inferiorly, and a lateral one?

The Central Canal.—The perfect distinction or separation between the central columns, superiorly and inferiorly, is a most important thing. There is a cavity interposed between them—a central canal along the whole course of the spinal marrow. I must claim the discovery of this for a veterinary surgeon. Mr. Sewell, the assistant professor at the Veterinary College, so long ago as the year 1808, in a letter to the Royal Society, describes this canal running from the fourth ventricle through the whole length of the spinal marrow, containing a limpid colourless fluid, and lined by an arachnoid membrane. How near did this gentleman approach to one of the most interesting discoveries in physiology that was ever made! An error with regard to the *commencement* of the canal possibly prevented the discovery of the true structure of the spinal chord from becoming the proud boast of a veterinarian. I will not enter into the disputed question who was the actual discoverer of these six columns, let the honour be awarded, if due, to M. Bellingeri of Turin; but the conclusion, the all-important physiological and pathological deduction, from this structure of the spinal chord, I will claim for an English physiologist, the veterinary students' patron and friend. Respect and gratitude give me a double interest in his well-earned fame, although I now see him, with some others of his brethren, and to the exclusion of the only legitimate claimants—veterinary practitioners — sustaining the strangely anomalous character of *a medical examiner of veterinary pupils*; and for which, in truth, even *he* is, and can be, no more qualified than I am to occupy a place at the board of Apothecaries' or Surgeons' Hall.

Sir Charles Bell's Important Discovery of the Functions of the different Columns.—Now, Gentlemen, examine once more these lateral furrows. Do you find some difficulty in tracing the inferior one? You will see on both surfaces at certain distances, and from one end to the other of the canal, little nervous fibrils coming out in a line, and then approximating and forming a nervous chord. On each surface they proceed from this furrow—from the superior surface plainly enough—and, after close observation, there can be no doubt that they spring from the same furrow on the inferior surface. It is evident, from the manner in which they spring from the chord, and from dissection of the hardened spinal marrow, that these fibres come from the central column above and below. Now, Gentlemen, please to recollect what has been said of the cerebral nerves of pure sensation and pure motion, and the double root and double function of the fifth pair.

The Manner in which the Spinal Nerves arise.—Permit me to refer you to, and to recommend to your careful, and I am sure profitable perusal, Sir Charles Bell's own account of this, in his admirable "Exposition of the Natural System of the Nerves." The short sketch which I am about to attempt will somewhat prepare you for that perusal, and will chiefly consist, although, in other words, of the sum and substance of what he has stated. I observe, first, the nerves from the superior surface, and I have already seen that that superior surface answers to the portion of the brain which gives origin to nerves of sensation—I trace these fibrils from the *centre* of this surface—I observe that they come out as it were *bodily* from the chord, that they approximate, unite, and *run into and form, a ganglion.* I turn to the inferior surface, which more plainly answers to that part of the brain whence I traced the origin of the *motor* nerves—I there observe these fibrils—they arise from the *centre*—they are *smaller* and more *superficially attached* to the chord—they also approximate—they unite—they form a nervous chord: — *they have no ganglion*, but they join the chord from the superior surface, immediately beyond its ganglion, and, as they are penetrating the dura mater, and emerging from that membrane, they coalesce and become one nerve. "Is it then," inquires the discoverer of the function of the spinal chord, "is it by means of these separate and essentially different origins, that the spinal nerve is enabled to discharge its apparently double office?" A less cautious physiologist would have jumped at once to a conclusion. He has recourse to the infallible test of experiment, and in the conducting of the necessary experiments, the humanity of the inquirer reflects on him as much credit as his physiological acumen, and forms a highly honourable contrast with the recklessly brutal system of torture adopted by some other inquirers.

Bell's Experiments.—He opened the spinal canal of a rabbit, and cut the superior roots of the nerves of the hinder extremity. The creature crawled. The protracted cruelty of the dissection deterred him, he says, from ever repeating this experiment, and he reflected that it would be quite as satisfactory if performed on an animal recently knocked on the head and rendered insensible. He struck a rabbit behind the ear, and exposed the spinal marrow. On irritating the superior roots of the nerves he could perceive no motion on any part of the frame; but on irritating the inferior roots, at each touch of the forceps, there was a corresponding motion of the muscles to which the nerve was distributed. This was perfectly satisfactory, and he now felt himself justified in coming to the conclusion, that "the inferior roots of the spinal nerves bestowed the power of muscular motion, and the posterior roots sensibility."

The true Structure of the Spinal Nerves.—If you will now recollect what I have stated of the fibrous structure of the nerves, that not only each fasciculus, but each little fibril, was surrounded by its own neurilema, you will easily conceive the possibility of these nerves of different function pursuing their course independent of, and uninfluenced by, each other; or, in the words of my great master, if I may dare so to call him, and with which I will dismiss you—"each filament or track of nervous matter has its peculiar endowment, independently of the others which are bound up along with it, and continues to have the same endowment throughout its whole length."

DESCRIPTION OF A

FOSSIL FOX.

By Mr. Murchison *and* Mr. Mantell.

Condensed from the "Geological Transactions."

Amongst other interesting matters contained in the recent volume of Transactions, published by the Geological Society, is the description by Mr. Mantell of a fossil fox, found in quarries of Œningen, near Constance. These quaries are known to have been worked as early as the year 1680, and have excited the curiosity and stimulated the exertions of many naturalists. The variety of animal and vegetable remains found at different times in these situations would astonish any geologist unacquainted with the wonderful fertility of these quarries. Several quadrupeds and birds are mentioned, a vast number of fishes, reptiles, insects, and innumerable plants. In 1829, the fossil fox was discovered by Mr. Murchison, in a bed of whitish-grey marlstone. The fossil nature of this stone has occasioned it to split into two slabs, with such uneven surfaces, that characteristic portions of the bones or impressions of them are seen in each. The slab containing the more perfect portion of the animal has been selected by Mr. Mantell for his operations of clearing away the surrounding rock. Mr. Mantell in his letter to Mr. Murchison proceeds as follows:—

"In compliance with your request, I beg to offer a few remarks on the osteological characters of the extraordinary fossil discovered by you in the Œningen limestone, a fossil which far surpasses in interest any of the wonderful remains which had been found in that deposit.

"When you first submitted this matchless specimen to my chisel, you expressed your conviction that it would prove to be a species of fox; and as I proceeded in the interesting task of removing the stone from the skeleton, I found myself warranted in agreeing with you in that opinion. When the specimen was entirely exposed, I procured a recent fox, and dissected the skull, extremities, &c., and upon comparing them with the fossil, could detect no essential difference. Through the kindness of Mr. Clift, I have since been able to examine the skeletons of several varieties and species of the genus vulpes in the museum of the College of Surgeons, namely, five or six individuals of the vulpes communis; the black fox, the crop fox, and the isatis or white fox. The skull of the last mentioned species is more obtuse than that of the fossil; but the skull of the common fox and its varieties do not differ more from the fossil than from each other; in fact, there was not a greater difference observable than between the skull and teeth of individuals of the recent animals of various ages and countries. In making this observation I would, however, particularly remark, that the skull of the fossil is unfortunately so much broken and defaced, that the true form of the frontal bone, post-orbital apophyses, &c., cannot be correctly determined, and that these alone might be expected to afford essential specific characters.

"In the other parts of the skeleton, the only differences I noticed were the following, viz., the spinous process of the *dentatus* appeared to be more expanded; the *radius* more cylindrical and elegant near its brachial extremity; and the *fibulæ* larger and more rounded than the corresponding parts of the skeletons of the *vulpes communis*, which I had an opportunity of examining. I do not, however, think that these slight variations in osteological structure are alone sufficient to establish a variety, much less a species. Near the angle of the lower jaw of the fossil there is a process of bone so like the *styloid* in form, and occupying the situation it might be supposed to occupy if these animals possessed such a process, that its appearance was at first very puzzling. Upon minute examination I am inclined to believe, that it is a spiculum of bone, displaced by the fracture and compression which the skull has sustained, and that its situation is accidental; it is, however, too remarkable an appearance to be passed by without comment.

"I very much regret that from my limited means of observation I cannot offer more satisfactory results. The remarks of Baron Cuvier on the bones of the animals of this genus found in caverns, '*that they may with almost equal propriety be assumed to belong to one recent species as to another*,' will apply to the splendid specimen before us. If the skull were perfect more rigid conclusions might be obtained; but as this is not the case, I beg to submit that the only inferences we are warranted in deducing are the following:

"1. That the fossil animal belonged to the genus *vulpes*.

"2. That it bears a closer analogy to the *vulpes communis* than to any other with which it has been compared.

"3. That notwithstanding its resemblance to the recent, it may possibly belong to an extinct species, since specific differences cannot always be detected in the skeleton."

In addition to Mr. Mantell's statement, Mr. Murchison observes, that the occurrence of a black-coloured matter in the place of the abdomen, immediately beneath the lumbar vertebræ, induced him, under the supposition of its being the fossil fæces of the animal to submit a portion to the examination of Dr. Prout, who after analysis pronounced it to be of the same nature as the balls of *album græcum*, and coprolites of Dr. Buckland. This is the first instance in which the fossil substance has been found in the body of a land quadruped, although Dr. Buckland has observed, that it occurred in the abdominal regions of saurians. In this case, however, it is not the coprolite alone which contains phosphate of lime; for, by other experiments of Dr. Prout, it appears that the entire black of the surrounding marlstone is also impregnated with the same mineral. There seems, therefore, to be every reason to conclude, that the bitumization of this mass of rock is due, in a great measure, to the destruction of the large quantity of animal matter contained in it; for the coprolite itself only differs from the surrounding matrix in containing a greater proportion of lime.

IMPORTANT CASE OF

INFLAMMATION OF THE LUNGS IN A NEW-BORN INFANT,

GIVING RISE TO A SUSPICION OF

MURDER BY SUFFOCATION.

By A. A. Lecadre, M.D., *&c., &c.*[*]

Of all the branches of the medical art that which assists the legislator in his decisions on the guilt or innocence of accused individuals, deserves the most minute and scrupulous attention. The most trifling observation, the least remark, even a solitary word, is sufficient on some occasions either for the detection of the guilty or the preservation of the innocent. The sagacity essential to the medical jurist, is pre-eminently the fruit of experience in medico-legal cases. In that which I am about to relate, it will be seen that in case of much suspicion, some very simple circumstances decided the opinion which I advanced, and which the judges adopted.

On the 3rd December 1851, I was required by the solicitor general of the tribunal of Havre to proceed to the exhumation and dissection of a new-born child which had been interred the evening before, and whose death was suspected to have been the result of suffocation. The following circumstances led to these suspicions:—the mother of the infant had been separated for more than fifteen months from her husband, a sailor. She had, however, met him six months before, during a brief period of two hours for which the vessel touched at Havre. She subsequently declared publicly that she was pregnant, and immediately before the birth of the child, said she was but six months gone. On the 24th of November she was confined, and the child was readily recognised to be of the full term. In six days the child died suddenly. The midwife, who had left the woman and child well on the third day after delivery (November 27th), returning on the sixth day, to her astonishment learned of the infant's death: finding the lips and body of a violet colour, she gave information to the police. An inquiry was consequently directed.

The body having been exhumed, was carried to an adjoining house. Its length was eighteen inches, and the weight between six and seven pounds. The umbilical chord was withered and almost separated; the head was covered with hair more than half an inch long; the nails were so long as to pass beyond the extremities of the fingers. Everything, in short, indicated that the infant had attained its full term. The right side of the face, the chest, and back, were covered with livid cadaveric spots, and several spots of the same kind existed over the limbs. The abdominal integuments were of a greenish colour, and in a state of incipient putrefaction. There was no trace of bruise, ecchymosis, fracture, dislocation, or wound, on or about the head, trunk, or limbs.

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[*] Annales d'Hygeine publique, &c, &c.

On opening the body we found.—1st, *Head*. Nothing abnormal. The venous vessels of the brain gorged with black blood. The bones touching, except where separated by the fontanelles.—*Mouth*. Slightly red internally and posteriorly. The frænum linguæ entire.—*Larynx* and *trachea* natural and free from mucous obstruction.—2nd, *Thorax*. *Lungs* apparently healthy, of a red colour, considerably gorged with blood—floating completely on water, as well while entire as in separate sections; they were not, however, sufficiently buoyant to float the heart and liver. The heart, which was well developed, had its right cavities filled with black liquid blood. All the veins terminating in the heart were similarly turgid. The left cavities of the heart, with the vessels thence issuing, were void of blood and collapsed. The foramen ovale was almost obliterated.—*Abdomen*. The *stomach* and *intestines* void of food, and presenting occasional patches of arborescent vascularity, and containing no feculent matters. The *bladder* was full of urine. The *liver*, spleen, and kidneys, on incision, gave out a large quantity of black liquid blood.

The death of this infant then resulted from asphyxia, but of what kind? and what was its cause? The day before his death, the thermometer having fallen remarkably, I inquired if he had been exposed to the cold. A negative answer was returned, and questions relative to the possibility of death by overlaying were replied to in a similar manner. In my report, therefore, I pointed out asphyxia as the cause of death, but I abstained from assigning any cause for the asphyxia. In a few days I was cited before the *judge d'instruction*, and then the mode of death of this infant became perfectly intelligible. Witnesses were then examined, who deposed that, from the first hour of its existence, the child showed symptoms of pulmonary disease. He sucked with difficulty, was oppressed, had a slight cough, with mucous expectoration. An apothecary deposed that a messenger came from the mother for "something good for a baby with a sick chest." He gave some syrup of capillaire, but the child's oppression increased, and he died on the 30th, after experiencing great difficulty of respiration.

It was thus rendered evident that the child's death was occasioned by pneumonia, a disease so prevalent among new born infants, that of twenty deaths fifteen generally proceed from this cause. If, on the inspection of the lungs, no traces of this disease were found this depended on the fact that it is impossible to distinguish the pulmonary congestion of the first stage of pneumonia from the congestion dependent on simple asphyxia. Again, this kind of pneumonic congestion only existing in the early days of the disease, it is not surprising that the midwife who left the child well on the 27th, found it dead on the 30th. Indeed, infantile pneumonia is frequently fatal in even a shorter period.

The preceding explanations being deemed sufficient, the accused was acquitted.

CASES OF

MALIGNANT CHOLERA,

TREATED WITH LARGE DOSES OF

CALOMEL, &c.

By D. TOD, Esq., *Surgeon, Hampstead Road.*

CASE 1.—On the 15th August last, a gentleman residing in the Minories, was taken suddenly ill in the afternoon whilst walking along the street, and was obliged to return home. After his arrival he gradually became worse, and about midnight was seized with vomiting and purging, and cramps in the stomach and extremities, with a deadly coldness over his whole body; and in this state I found him in the following morning at eight o'clock. In addition to these symptoms, I learnt that he was exceedingly thirsty, had passed no urine, and his pulse could not be felt at the wrist. The usual remedies to allay the vomiting, and to restore the heat to the surface of the body were immediately resorted to, and repeated at short intervals for nearly thirty hours without giving the least relief. At last I had recourse to mercury (for to all appearance he was rapidly sinking), and began by giving ℈j of calomel, and by rubbing in ʒij of ung. hydr. fortius every hour, until his mouth or breath became affected. Next day, on my arrival, I learnt that in about six hours after the commencement of the mercurial plan the vomiting and purging had entirely ceased, and that the heat gradually returned to the surface; shortly afterwards he fell into a sound sleep, bathed in perspiration.

In three weeks he was quite well.

CASE 2.—On the 30th March last, Mr. S., aged 60, residing in this neighbourhood, was suddenly seized at midnight with cramps in the extremities and abdomen, and afterwards with vomiting and purging. In this state I found him on the following morning at six o'clock. On investigation, I found that his whole body was as cold as marble, that he had no pulsation at the wrist, had passed no urine, and complained of great oppression in breathing. According to the treatment then recommended, I immediately gave him an emetic, which cleared his stomach in a few minutes; afterwards ℈j of calomel and a wine-glassful of a strong stimulating mixture, composed of opium, ether, camphor, alcohol, &c., and repeated the latter dose every fifteen minutes. I also had him rubbed before a large fire with flannels, and afterwards put into a warm-bath, as soon as it could be procured.

After remaining in the bath about twenty minutes he became drowsy, and was immediately removed, rolled in blankets, and put into bed. In the course of an hour he fell asleep, bathed in a most profuse, warm perspiration, and died in four hours afterwards.

CASE 3.—J. English, residing at 17, Fitzroy-market, aged 21, was taken suddenly ill about one o'clock in the morning of the 30th March last, with vomiting and purging, cramps in the legs, a coldness over the whole body, great thirst, and oppression in breathing; no secretion of urine; in this state I saw him at eight o'clock. He had also no pulsation at the wrist. Ordered him an emetic; and after the stomach was cleared, to take ℈j of calomel every hour, and a dose of a very gentle stimulating mixture every four hours. After having taken five doses of the calomel, I ordered him to be sent to the hospital in consequence of being destitute of the common necessaries of life. In three weeks he quite recovered.

Before he was removed from his lodgings, all his symptoms were much abated, and I have no doubt that his recovery was owing to the mercury.

CASE 3.—On the 8th April last, J. Atfield, aged 26, residing at 12, Frederick Street, Hampstead Road, was taken ill with vomiting and purging, cramps in the stomach, and coldness over the whole body, constant thirst, and no secretion of urine. In this condition I saw him six hours after the beginning of the attack. He had also no pulsation at the wrist. I immediately gave him ℈j of calomel, and desired it to be repeated every hour until he had taken a drachm. Shortly after the first dose of calomel, the vomiting and purging appeared to be much abated, seldom occurring oftener than once every twenty or thirty minutes.

During the evening a physician was called in, who ordered him to take opium in pills, which he fortunately always threw up in less than a minute after they were taken; also a stimulating mixture, combined with opium and liquor potassæ, to be taken in doses every 2 hours. Next day he was much worse, and apparently sinking. In the evening I took about fourteen ounces of blood from the arm in a full stream, which caused syncope, and I left him for the night. On the following morning I learnt that his mouth was affected with the mercury, that he had slept for several hours during the night, that the vomiting and purging had ceased, and he was warm over the whole body, but had not made any urine.

After this period he progressively recovered, but in consequence of being removed into the country by his family, he had a relapse, and in two days died, apparently from the effects of the journey.

CASE 4.—On the 17th April last, Mrs. Atfield, the wife of the former patient, was taken suddenly ill with the usual symptoms of cholera, and in two hours afterwards, on being called, I gave her ʒss of calomel, which, to my agreeable surprise, put a stop to all her symptoms, and on the following day she appeared quite well.

CASE 5.—On the 22d June last, a gentleman in my neighbourhood was taken suddenly ill with the usual symptoms of cholera while in the city transacting business. Twelve hours afterwards I was desired to see him. I immediately gave him ʒss of calomel, and repeated the dose in the course of an hour; also desired half an ounce of ung. hydr. fort. to be rubbed in every hour for four hours. Shortly afterwards the mercurial action was apparent, when all his symptoms gradually ceased, and in ten days, with the exception of his sore mouth, he was quite well.

CASE 6.—On the 23d June, William Cannon, aged 36 residing at 40, Charles Street, Hampstead Road, was taken suddenly ill with all the symptoms of cholera, and six hours afterwards I was desired to see him. The same mode of treatment was pursued as in the former case, and was attended with the same result.

In this case a relapse took place, in consequence of having begun too soon to take nourishing food, but with gentle care he soon recovered.

CASE 7.—On the 12th July last, William Morris, aged 50, residing at 1, Frederick Place, Hampstead Road, was taken suddenly with the usual symptoms of cholera, and had had administered to him sixpennyworth of brandy before I was called in.

Five hours after the commencement of the attack I saw him, and immediately resorted to the same mode of treatment which I pursued in the two former cases, but the mercury did not take effect, and he died in fourteen hours.

At my first visit the symptoms were not of that violent character which would have led any one to think he would die, but as soon as his wife acquainted me with the brandy which he had taken, I immediately pronounced the case to be fatal.

Case 8.—On the 14th of July last, Mary Shipley, residing at 12, Henry Street, Hampstead Road, was suddenly seized with the usual symptoms of cholera, and had taken a small quantity of brandy, a small dose of calomel and opium, and the mixture recommended by Dr. Stevens, at the desire of a medical gentleman before I saw her. On my arrival, three hours afterwards, I immediately gave her ʒss of calomel, and ordered ℥iss of ung. hydr. fort. to be rubbed in. Next day all the symptoms, except the purging, were considerably relieved, and with very gentle treatment she was in nine days pronounced convalescent.

N.B. Three other cases have been under my care since the last mentioned, which have undergone the same mode of treatment, and are now convalescent. I have also recommended several medical friends to pursue the same plan in the treatment of this disease.

Remarks.

On perusing the above cases of spasmodic or malignant cholera, it will be perceived that the first fatal case was treated according to the stimulating plan, and died in eight hours afterwards; and that the second took a large quantity of brandy before I saw him, and died in sixteen hours.

In the treatment of this disease I have, except in the two first cases, paid no attention to symptoms, because they are not the cause, but merely the effects of the malady, and a favourable issue has invariably been the result. In disease a certain action always exists, which, if not superseded by another action, will soon pervert the harmony of the animal economy. In cholera such a state of things invariably exists, and what agent do we possess that can produce a specific effect on the vital system so speedily and so beneficially as mercury? In the treatment of syphilis do we attend to symptoms when we salivate? The same question is applicable to iritis, peritonitis, and every other acute inflammatory disease which has its seat in a vital part, and why should not cholera come under the same rule?

That salivation of cholera patients is attended with the happiest effects will soon appear evident to every one who may adopt it. From experience I find that ʒss doses of calomel act in nine cases in ten like a charm. They allay the vomiting and purging, cause a return of heat to the skin, then a profuse warm perspiration, and, subsequently, a gentle and refreshing sleep. In very violent cases I have given 120 grains of calomel in three hours, as well as had ℥ij of the *ung. hydr. fortius*, rubbed in without any ill effects appearing at a subsequent period, such as violent ptyalism. I state this, in order that the numerous readers of The Lancet may not be alarmed in pushing the mercurial plan to a considerable extent in the treatment of cholera, for unless they put a speedy stop to its action, the system will be soon worn out, and death be the result.

Sols Row, Hampstead Road,
Aug. 1, 1832.

TREATMENT OF
MALIGNANT CHOLERA.

Stimulants injurious.—Efficacy of Calomel and Counter Irritants.—Danger of Bleeding; &c.

To the Editor of The Lancet.

Sir,—Having seen a good deal of cholera, and attended to the post-mortem appearances presented in about twenty cases, I am induced to make the following remarks, which you may perhaps deem worthy of a place in your journal. I would particularly caution against the use of stimulants, those who may not have seen their effects in this dreadful malady. The great prostration of strength, the small, frequent, feeble pulse, the short breathing, and the paralised state of the vital powers, indicate the use of stimulants, but their extensive exhibition has only shown their total inefficiency; nay, I would even say their injurious effects, for I have seen brandy and carb. ammoniæ given in very large doses, and after each dose there was a decided depression of the vital powers. However much this may be opposed to generally-received opinions, I have no hesitation in stating it as a fact, calculated to prevent a mode of treatment plausible enough in theory, but practically bad. On the commencement of an attack of cholera, while the pulse is pretty strong, the cramps not severe, and oppression not great, a few doses of calomel and ext. hyosciami, followed by a little rhubarb, or a small bleeding, will very much mitigate or cut short the disease; but when collapse has existed for six or eight hours, and the pulse cannot be felt at the wrist, when the breath and tip of the tongue are cold, and when the surface of the body has a cold inanimate feel, and the eyes are much sunken and have a glassy appearance, medical treatment becomes useless, for the patient's fate is sealed in a very few hours. Cases neither so slight as the former, nor so hopeless as the latter, very frequently occur; in such cases, the treatment I should recommend is the following, and if actively and assiduously enforced, it will be productive of the happiest results:—The body should be enveloped in warm blankets, heated air applied to the surface of the body, and if the dejections be frequent, an enema containing tinct. opii and tinct. catachu, each a drachm, should be given, while the irritable state of the stomach is best allayed by effervescing draughts, containing an excess of alkali; but these are only auxiliaries to counter irritation, and calomel and ext. hyosciami, given in an almost unlimited quantity; 8 grs. of calomel, with the same quantity of ext. hyosciami, should be given every two hours, gradually diminishing the dose, or extending the period between each dose; at the same time extensive irritation must be produced on the surface of the body: this may be difficult to obtain, but no means should be untried, for the patient's safety very much depends upon it. Common blisters are not sufficiently active. Sinapisms, heated almost to scalding, may fail; even hot spirit of turpentine may not produce the desired effect, but a compound, containing equal parts of spt. cornu. cervi, and ol. terebinthinæ, with a sufficient quantity of pulv. cantharidis to form a semi fluid, will scarcely fail to produce redness, heat, and vesication, if the parts be well rubbed with a piece of flannel dipped in it; the irritation should be kept up by occasional rubbing, until the constitution begins to rally. The legs are the most convenient seat for irritation, and may be the most useful, from their distance from the seat of oppression. Other parts of the body should be rubbed occasionally, with spt. camphoræ, containing a drachm of camphor to an ounce of spirit; so soon as reaction commences, calomel and antimony must be given in small doses every four or five hours, and bleeding to a considerable extent will be almost always necessary. If local determinations take place, leeches must be applied in large numbers, with blisters and cold lotions.

Cholera appears to me a *congestive* disease, and in the early stage as much subject to proper remedies as any other severe complaint. Whether the diarrhœa, which, with scarcely an exception, precedes the symptoms of collapse, be considered the first stage of cholera, or as a distinct affection producing this disease by the excessive draining of fluids from the body, I am fully convinced that no bad symptoms follow when it is treated in the manner noticed above. The symptoms during collapse, and appearances on dissection, prove the unequal distribution of the blood, and congested state of internal organs; the quality of the blood is also depraved, but it seems to me that no change in its properties can materially benefit the patient, until the balance of the circulation be restored. This, then, should be the great object of our remedial means, but it cannot be accomplished by the means usually had recourse to. When a vein is opened, or a stimulant given, in common congestive fever, or the cold stage of intermittent fever, determination takes place to the surface, and the action of the heart is increased; but in the collapsed stage of cholera, even a small bleeding cuts short the thread of life. From a bleeding to the extent of four ounces, I have known the small feeble pulse become imperceptible, the features shrink fearfully, the patient gradually sink under the use of powerful stimulants, and death follow a few hours after the bleeding.

The severity of the stage of excitement is proportionate to that of the stage of collapse, and is much more fatal in young than in elderly subjects, probably from the greater activity in the vascular system of the former. Sedative means must be very freely had recourse to as soon as reaction appears. Tart. antimonii will then be found a valuable remedy, but not sufficient of itself, for I have never seen a case without determination either to the head, chest, or abdomen. The head is the most frequent seat of increased action, which can be reduced only by bleeding almost to death's door, cold lotions, and counter irritants; these last should never be applied to the head, but to the neck, chest, or extremities. Two or three dozen leeches will be found most useful in removing abdominal congestion. Although the successful result and appearances on dissection fully justify the above treatment, the pulse will seldom indicate bleeding, for it is generally small and weak; indeed a moderately strong pulse should be considered a favourable symptom. In conclusion I may state, that the treatment recommended above is founded on observations made in an ample field, and on the *post-mortem* appearances presented in a great many cases. I am, Sir,

Your obedient servant,
An Assistant-Surgeon, R. N.*
Woolwich, 24th July, 1832.

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* Our correspondent has forwarded us his name and that of his ship.—Ed. L.

CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE

IN THE UNIVERSITY OF LONDON,

Delivered April 16, 1832.

PURPURA HÆMORRHAGICA.

THE first case, Gentlemen, of which I will speak, is that of a woman who laboured under purpura hemorrhagica, who was admitted on the 22d of March, and of whose case I spoke in the first clinical lecture after her admission. I stated that the discoloration was particularly great upon her legs, so that they were stained in large patches, and had just the appearance of what have been called *claret marks*.

Treatment.—I mentioned that the treatment would be antiphlogistic, that she had been bled to eight ounces, that colchicum had been given, half a drachm of the wine three times a day, and that she had been put on milk diet. Under this treatment she got perfectly well. The effect of the treatment was I think decided; there can be no doubt that the amendment was owing to it. She had been in this way some time, and had not been mending, but on the contrary getting worse every day up to the time of her admission; yet in a few days, at least on the 27th, having been admitted on the 22d, the spots began to fade, and on the 30th disappeared entirely from the body, and a brown mark only remained in the legs where the patches had been considerable, so that she almost instantly began to mend, and was presently perfectly free from the disease. She now appeared pale, just as persons are who do not make sufficient blood, or blood of a sufficiently rich quality, or who have lost a great deal of blood. I then ordered her the tartrate of iron, half a drachm three times a day, but it rather purged her, and on that account the carbonate of iron was substituted for it, and she soon left the hospital perfectly well.

It is an interesting case on account of the nature of the disease, which is not one seen every day, although it is frequently seen, and it is one the pathology of which is obscure. I do not pretend to know anything about it, but it is an affection in which antiphlogistic treatment is generally required, though, occasionally, treatment of an opposite kind is demanded.

INFLAMMATORY LEUCORRHŒA.

The next case of which I will speak is one of inflammatory leucorrhœa. This was a single woman who had been confined seven months since—a thing very possible, although perhaps not much to her credit.

Subsequently to her confinement she was attacked with pain in the right hip, the abdomen was tender on pressure, she had a copious discharge from the vagina, pain in making water, and so on. It appeared to be inflammatory leucorrhœa or gonorrhœa, I do not pretend to know which.

Treatment.—She was bled to twelve ounces at first, and purged every day, and then twelve leeches were applied, and so on, till nothing remained but a considerable discharge, and the time had arrived for applying astringents. The treatment of a discharge from one mucous membrane is exactly the same as the treatment of discharge from another, so far as circumstances will allow of it. If a discharge from a mucous membrane be accompanied with heat and pain, it would be absurd for the most part to apply astringents, for they would do no good. If you could check the discharge of fluid from the vessels, but did not remove their inflammatory condition, you would stop the discharge for a time, and increase the inflammation by increasing the congestion. The proper treatment, where there is inflammation, is not to attend to the discharge, but to use antiphlogistic means; and when the inflammatory symptoms have gone off, the discharge will frequently cease spontaneously: but if it remain afterwards, then it is right to apply astringents.

Now, in cases of discharge from the vagina, I do not know so good an astringent generally as the nitrate of silver. It was commended much by a surgeon of the name of Jewell, and in consequence of his recommendation I have frequently employed it. In this case the injection consisted of one grain to three ounces of water, and the effect was speedy. Although a discharge continued some time after the antiphlogistic measures were employed, yet it presently diminished by means of the nitrate of silver, and so rapidly, that there was no occasion to increase the strength of the injection, and the patient went away perfectly well, supplied however with a quantity of the injection, because, if the medicine be not used some time after the disease has ceased, in many instances this will return. I sometimes employ the nitrate of silver in as large a quantity as a grain to an ounce, but it is better to begin with a quarter of a grain to the ounce, and it is very easy to increase it. Although it is easy to decrease it when it is too strong, yet you have an unpleasant effect produced, which may not cease immediately. You may increase it without inconvenience, but an excessive strength is attended frequently with considerable inconvenience; for notwithstanding you decrease the quantity, the effect may continue. You will find this an excellent injection, one which is frequently far more efficacious than any other—at least in the case of the vagina.

CHLOROSIS.

The next case to which I will advert is one of chlorosis, and is very interesting, both on account of the severity of the symptoms, and the cure which was effected.

This girl, labouring under chlorosis, was almost as white as snow, but, strange enough, her Christian name was Rosa—Rosa Barclay. It ought rather to have been Lily—Lily Barclay. She was eighteen years of age; her countenance was pale and sallow, chlorotic, and her eyes were pearly, with a dark circle around them. She was so weak that she was scarcely able to support herself, and she had had no catamenia for nine months. During that time her health had been declining, and she complained of pain of the chest, pain of the abdomen, pain of the loins and legs. You know that the character of chlorosis is anæmia, want of blood in the system, blood of an imperfect quality, with few red particles and little fibrin. There is very frequently œdema of the limbs, sometimes of the face, and all the symptoms of anæmia, such as palpitation of the heart (the heart having so small a quantity of blood to circulate, and that of so light a quality, it acts easily and violently, and therefore you have palpitation on the slightest causes), dyspepsia, frequently costiveness, extreme debility, and a want of the menses; the menses may never appear, or, if they appear, it is only in a slight degree.

Now, with the exception of menstruation, this is a state that will occur in males. You sometimes see young men fall into the same condition—pale, bloodless, and weak, with every symptom except those that depend on the uterus, so that we are not justified in considering this a disease of the uterus. I believe we ought rather to say the uterus cannot perform its functions, on account of the deficiency of power in the whole of the system at large, the deficiency of blood.

Treatment.—Now this girl had been under treatment, but the usual remedies proper in these cases had not been adopted, and she was no better. I mention this, because she got well under the treatment employed here; and as another mode had been had recourse to without benefit, one may fairly ascribe her cure to what was done here. Her pulse was quick and irregular, and I ordered her a drachm of ferrum tartarizatum three times a day, and found her improving fast. On account of her bowels being confined she had a dose of calomel ordered her by somebody, with hydrarg. c. creta, and effervescing draughts; but I did not find this answer, for when I saw her again on the 24th she was nobetter, and a degree of bronchitis came on, in addition to which she talked incoherently, so that it was thought advisable to put a blister on her chest, a blister on her head, to take off meat diet and substitute milk in its stead, and treat her as labouring under inflammation without power: for she appeared to be suffering under bronchitis, with a degree of inflammation in the head. At last she had a hectic look, and such a change took place that she appeared sinking. I therefore ventured to give her half an ounce of wine every eight hours, sulphate of iron, and mutton chops, every day. Some tenderness of the abdomen came on, for which a blister was applied, and then purging ensued, so that it was necessary to give her catechu. She sweated profusely at night, her pulse was 160, and she took half a grain of muriate of morphia every night, persevering with the iron and mutton chops. On the 15th she was stronger, and on the 29th, as she proceeded too slowly, the iron was increased to three times a day, and the wine to four ounces a day, and she then improved rapidly, and got completely well.

She was presented before I wished, because, although she was well, I was anxious that she should remain in the hospital some time, that her strength might be quite re-established. But she desired to go out for the purpose of presenting herself to the committee of the Magdalen, having been a naughty girl. Our pity, however, was greatly excited by hearing that an old woman in whose establishment she had been, came to the hospital, pounced upon her, and took her back to her house to engage her again. From the pity excited on her behalf we were going to endeavour to trace her out, when it appeared that she had sent for the old lady herself, and entered her former line of life voluntarily. Such an occurrence as this is almost enough to make one weary of well doing.

BRONCHITIS — DISEASE OF THE LEFT VENTRICLE OF THE HEART.

The next case among the women, was not one that terminated so favourably as the three I have now mentioned. It was a case of sudden death in an old woman, who came in with anasarca. She was admitted on the 29th of March, ætat. 66.

The symptoms were, anasarca, especially of the lower extremities, which were distended to an immense size. She stated that she first observed this swelling five weeks ago. Before its appearance she was troubled with pain in her legs, but she made no complaint, and said her health had always been good. On examining the chest, which one ought always to do as well as the abdomen, in cases of dropsy, there was discovered sibilous rattle. She had bronchitis, and a loud bellows sound was heard in the region of the left ventricle, at the moment of the impulse of the heart, at the contraction of the ventricle; it was therefore clear, that there was an obstruction to the course of the blood from the left ventricle.

Treatment.—I ordered her to lose a little blood, eight ounces, and to be purged well with elaterium. I gave her half a grain every day, but it purged her so little, that it was increased to a grain. The swelling of the legs became less, but it would have taken some weeks to get her into a condition fit to leave the house, and then she never could have been cured. About twelve o'clock on the 8th of April, she complained of pain in her limbs, and in the morning she suddenly became speechless, and remained in that state till half past twelve the following day, and died before I arrived. I presume she died through some affection of the head, but I do not know. I should have supposed it was syncope, and that it arose from disease of the heart, but from the length of time she lay in the state I have described, I imagine that she died from something wrong in the head. As I found considerable disease at the beginning of the aorta, it is very likely that similar disease had taken place in the arteries of the head, and caused an eff sion into that organ; but not knowing at the *post mortem* examination what had occurred, the head was not examined.

There was found some slight inflammation in the small ramifications of the bronchial tubes; they were filled with mucus. But on examining the heart, the aortic opening was found contracted, a circumstance which I expected on account of the difficulty in the transmission of the blood, shown by the loud bellows sound at the moment of the pulse,—at the moment the heart struck against the side. There was a bony formation below the valves, the valves themselves were sound; the arch of the aorta, and indeed the whole of the aorta, was in a very diseased state, the inner membrane being partially destroyed, and the surface studded with bony deposits. The valves themselves were sound enough to play: they could prevent the blood from rushing back into the ventricle. If they had been so diseased as to have allowed the blood to rush back, no doubt the bellows sound would have been heard also after the pulse when the ventricle dilates again. There was a bony ring all around their base. Though she fancied she had been ill only so a short time, yet this disease must have existed for a long period. The whole of the aorta is diseased. You see another exemplification of the fact, that the left side of the heart is generally diseased, and not the right. Here are the pulmonary valves and the pulmonary artery all in a state of disease; but the pulmonary artery is affected in the slightest possible way compared with the aorta. The branches coming from the aorta are diseased. Here is the innominata also in a state of ossification. Here is the thoracic aorta and the abdominal ossified down to the bifurcation. The iliacs are all ossified also.

There is nothing important in the uterine system, — nothing that could have done harm; but there was a disease there which is very common, namely, a white fibrous tumour. It is very common to see white firm tumours on the outside of the womb under the peritoneum, likewise in its substance; sometimes indeed within the cavity: that, however, is not so common. But here is a tumour outside the womb, under the peritoneum, at the top of the fundus. You may see the white fibres in it now. It was quite white when the parts were first removed, not being dyed with blood, but you may still see it to be white. Sometimes you see several such tumours, and occasionally they make the womb very large. True scirrhus rarely makes the womb large; you may feel it sometimes above the pubes, but in general it is not very large, whereas these tumours may make it of an immense size, but that will be attended with no pain, and no disturbance except from the bulk. There is something pretty here—a young polypus, no larger than one of the fringes under the tongue. You know that polypi most frequently come from the neck about the os uteri, and here is one immediately above the os uteri, a little growth of the inner membrane, and which, had the patient lived long enough, would have grown and become large. As a great tree will come from a small shoot, so this in time might have been a source of great trouble; it might have dragged down and required a ligature. It is at present in embryo, an innocent little polypus.

The liver was of singular form; I never saw anything like it before. For about two inches in breadth the whole margin was turned completely up, and there was a sharp line between these two inches and the rest of the liver, so that really a long channel was formed between the convex surface of the liver and the two turned-up edges.

Such an obstruction to the course of the blood in the aorta at large, and particularly at the mouth, of course, threw too much blood on the lungs, and caused a difficulty in its return to the rest of the body, and therefore it is no wonder that œdema came on; but notwithstanding all this, there is no hypertrophy of the left ventricle: it is not thicker than it should be, nor dilated.

INFLAMMATORY HEADACHE—INFLAMMATORY EPISTAXIS.

In regard to the men, one was presented who laboured under inflammatory headache, and another under inflammatory epistaxis.

Case 1.—The symptoms in both these people were of course very much alike. There were inflammatory symptoms in the head in both cases, but with other symptoms additional in both. With respect to one, besides the inflammatory headache, there were signs of paralysis; both the hands and the feet were benumbed. I believe that when the feet only or the legs only are benumbed, that in the greater number of cases the cause resides in the spinal marrow. In almost all the cases of paraplegia that I see the cause is in the spinal marrow. But when I see the hands and feet affected, not the whole of the upper extremities and the lower, but merely the hands and feet, I think in the greater number of cases you will find it depend upon some disease in the head, generally by distended vessels, compression; and these are cases that generally depend on simple compression, without organic disease, and are relieved very much by bleeding. That was the case in this man. This man, who was a barge builder, had been ill for six weeks with numbness of the hands and legs, frequent vertigo, dimness of sight, and pain in the head.

Treatment.—He was bled to 20 ounces, put on slops, and had five grains of calomel every night worked off with salts and senna in the morning. This was on the 9th. On the 14th he was bled again to 20 ounces; on the 16th to 20 more. The calomel by this time had made his mouth tender; and as there was still some pain of the head and some giddiness, 20 leeches were applied to the head and repeated every other day. Under this treatment he got well; the symptoms in the head ceased; the giddiness, pain of the head, and numbness of the hands and feet, also disappeared. The latter is an occurrence commonly seen. A great number of cases of numbness of the hands and feet depend upon mere fulness of the head, and may be removed by such means as these. The treatment consisted of low living, starvation; 60 ounces of blood were abstracted in a few days, and then leeches were applied repeatedly.

Case 2.—In the man, however, in Jacob's Ward, there was no numbness, but a discharge of blood. The other was a case of inflammatory paralysis, but this was a case of inflammatory hemorrhage. Both these were instances of an inflammatory state of the head; but the one was attended by paralysis, the other by hemorrhage.

This man was twenty-eight years of age, and was a coal whipper. He was a stout athletic Irishman, of florid complexion and sanguineous temperament. He said that ten years before he had been struck on the nose. The former case arose from an excess of blood in the head, but this owed its origin to mechanical injury, and it shows how long, after it has been inflicted, mechanical injury of the head produces its ill effects. Ten years before, this man was struck on the nose, and profuse hemorrhage—epistaxis—followed. He supposes that he lost a pint of blood, and says that the bleeding returned on three successive nights following the injury, so that he lost altogether a large quantity of blood. On the Friday preceding his admission (he was admitted on the 14th) he fell on his nose again, and profuse bleeding ensued, which returned each night till he was admitted. He says, that ever since his first attack, now ten years ago, he has been subject to headache and swimming of the head. He says that he has been much addicted to the use of spirituous liquors, and that he has likewise drunk a large quantity of porter. This is common with all coal-whippers. He says that the nature of his occupation rendered it necessary; that the labour is so severe that he was obliged to drink a great deal, and that he was obliged to spend not less than 14s. or 15s. a week in the purchase of drink. He has, however, been in the habit, he stated, of taking much more than this sum would afford him, and sometimes he has taken in a day three gallons of porter, and at least half a pint of gin. He says that his employers forced him to take that quantity for the purpose of supporting their connexion with different public houses, the masters of which in return procure them customers. The labour that these men undergo, requires them to eat a great deal and to drink a large quantity, but such malt liquor as porter is very good for them, — infinitely better than spirits. These men have no stated wages, but are paid in proportion to the work they do, which excites them to do a very large quantity, and they necessarily drink in proportion. But besides the quantity of drink which this labour would require, and the additional quantity which the extraordinary labour to which they are stimulated requires, they are absolutely obliged to drink in order to get employment, because the persons who send coals to London, or who bring them for the purpose of sale, are usually patronised by public-houses, which find them customers, and enable them to dispose quickly of their cargoes; and then to oblige these public-house keepers, the coal-sellers will employ no poor men as coal-whippers who do not spend freely in public-houses. This is certainly a very cruel system, and a very great disgrace. This was as fine a man as you ever saw, but if he continue in this line of life, he will inevitably fall a victim to it, and will die of dropsy or liver disease, or some form of structural disease of the head. He said that the system of the coal-sellers and public-house keepers was abominable, and the destruction of poor men like him.

Treatment.—This patient was bled to 16 ounces, and salts and senna were given him every day; a cold lotion was applied to his nose also, and to the whole of his head. Some pain still continued, and on that account a blister was subsequently applied to his head: he was kept of course on low diet. No return of the hemorrhage took place; the headache and giddiness greatly diminished, and he went away very well, excepting some little headache and giddiness, which will doubtless remain. It was lamentable to think, that on returning to his occupation he would to a certainty fall into the same condition that he was in before.

HYPERTROPHY OF THE LEFT VENTRICLE OF THE HEART.

There was likewise presented a boy who came in with hypertrophy of the left ventricle of the heart. It was the case of which I spoke in a former clinical lecture.

Treatment.—By persevering in the use of colchicum, and the application of leeches from time to time to the region of the heart, keeping him still, and restricting him to, low diet, the symptoms yielded so much that he was anxious to go out. Of course he was not well; but if he continue such treatment as was adopted here, and remain still for a year or two, it is possible that the condition of the heart may become much more improved than it is now.

PHTHISIS.—INHALATION OF TANNER'S LIQUID.

There was likewise a man presented with phthisis, to whom no medicine was given, but in whom a trial was made of the inhalation of tanner's liquid. I have perseveringly tried for a year and a half, or more, the inhalation of iodine and the inhalation of chlorine, but I have never seen a case of phthisis cured by either. I have seen great temporary relief afforded, but I have never seen a single cure effected by them any more than by other measures which have been adopted. I have heard of the atmosphere in the neighbourhood of tanners being considered favourable to phthisical patients, and as there are such simple modes now of making patients inhale air impregnated with different things, I thought if a patient were made to breathe through tanner's liquid, and any benefit could be derived from it, that would be a way in which it might be obtained. This man inhaled it, and found a great mitigation of his symptoms—quite as much as, nay more than, in cases where I have employed chlorine or iodine; but unfortunately he caught cold, through the windows being open, and it was necessary to treat him antiphlogistically and get rid of the bronchitis. He then inhaled again, became better, and felt a desire to go out.

I have never seen a cure effected by this tanner's liquid, but I have seen a great mitigation of the symptoms, and in chronic bronchitis more than in phthisis. In bronchitis, where the inflammation is not violent, where there is more irritability of the membrane than anything else, and profuse discharge, I have seen great good done by it. It can do no good where the inflammation is severe. There is a man now in William's Ward with chronic bronchitis, who has experienced great relief from breathing for twenty minutes, three or four times a day, through a hot solution of tanner's liquid. Whether a decoction of oak bark alone would answer an equally good purpose, I do not know: for as the air in the neighbourhood of tanners has been so much praised, I thought it better to order tanner's liquid. It is, however, so offensive in its nature, that some people will not use it at all, especially ladies, and some are made sick by it; but as a means of mitigating phthisis it is likely to be useful; beyond saying this, I dare not speak. I intend, however, to ascertain whether breathing through mere decoction of oak bark, or Peruvian bark, may not be as serviceable.

Concluding Remarks.

If I deliver any further clinical lectures, you shall have due notice, but I have now given them for six months, and as there are three physicians attached to the hospital, I have contributed my share of clinical instruction. I know, that when the fine weather has set in, it is tiresome to remain in town and hear clinical lectures, but if any cases should occur in the course of the summer on which gentlemen wish to have a clinical lecture, I shall feel great pleasure in complying with their desire. (*Loud applause.*)

CASES OF
MALIGNANT CHOLERA
TREATED BY
VENOUS INJECTION.

By G. F. Girdwood, M.D., *Islington.*

Sir David Barry having expressed strongly his wishes that I should communicate to the Central Board of Health the result of my experience in the treatment of cholera by venous saline injections, I have made out a list of the patients thus treated, their residence, their ages, the date when done, and the quantity of saline fluid used. My own sense of duty towards Dr. O'Shaughnessy and Dr. Latta would have induced me, of my own accord, to make public my success, and I only waited till further experience enabled me to make my communication more worthy of the authors of this new mode of treatment than it is at present.

The first five cases in the following list were all of them under my own care.

Taken ill.	Name.	Age.	Residence.	Saline Injection used.
June 25 ..	Thomas Jones..	45	69, Harrow Road	4½ Pints
July 7	B. Conolly	35	27, Princess Street	2½ ditto
— 13 ...	Pat. Kelly	30	Workhouse	4½ ditto
— 13 ...	Mrs. Kemp	63	5, South Wharf Road	3½ ditto
— 24 ...	Miss ——— ..	65	——	4 ditto
June 30 ..	Mrs. ——— ..	32	39, Bell Street	5½ ditto
July 19 ..	Mrs. Moore....	42	8, North Wharf Road....	6 ditto

Case 1.—Was a man of very dissolute habits. He was employed on the roads. For a week he laboured under premonitory symptoms. I entertained no very sanguine hope of success, but had recourse to the venous injection as a last resource. His pulse was nearly gone. He was voiceless. The cramps were most violent. The vomiting and purging incessant. The breath and tongue were cold, and the surface of the body was cold and clammy. The operation was performed at eight in the evening. It occupied forty minutes. The violence of the symptoms subsided as by magic. The cramps instantly ceased. There were but two very slight attempts at vomiting after the operation. There was no longer any purging. The patient described the operation, in his own words, as being " very nice." It was as if something was thrilling all over him. After four hours great *heat* and *restlessness* he dropped asleep for two hours, and during the night had some refreshing slumber. He complained in the morning of nothing but weakness.

He returned to his usual occupation in two days after. Four days after he had been at work, he came to me. His legs were slightly œdematous, but the swelling speedily subsided under the administration of sweet spirits of nitre and digitalis.

Case 2.—Is that of an Irishman who had partaken freely, the night before I saw him, of bacon and peas and porter for supper. At three in the morning he was attacked violently with the usual symptoms of cholera. I was called to him at eleven. He was then not so far gone in collapse as Jones, but as I considered the benefit so decidedly advantageous in Jones's case, I did not hesitate a moment in making use of the venous injection in this case, before the collapse proceeded further. It was used. The instant the operation was finished, *he had a most violent rigor*. His features sank, his limbs shook, and his teeth chattered violently. The surface meanwhile became a deeper and a deeper blue. The friends were frightfully alarmed, and I confess I was not without serious apprehension for my patient, and some misgivings in my own mind respecting the propriety of my practice. However, the cramp, sickness, and vomiting, were gone. His hands and legs soon became warm, and in two hours he felt hot all over the body. He remained *hot and restless*, with occasionally a little sleep throughout the day. He complained much of pain in the loins; this pain increased so much, that at midnight I was awoke to relieve it. The camphor liniment, with laudanum, was freely applied to the back. In half an hour afterwards he made water—the first he passed from the commencement of the disease, and the pain in the back left him entirely. He remained weak for two days, but required no further medical treatment, excepting a small poultice for three days to the arm where the vein was slightly inflamed. (This is the only occasion on which I have had any annoyance of this kind after this operation.)

Case 3. — Patrick Kelly, was a poor fellow who had been sleeping for some nights in the fields, and had been half starved. He was much in the same state of collapse as Conolly. After the operation *he had a rigor*, though not so strongly marked as in Conolly's case. I now recalled to memory that Jones had, after the operation, felt cold. This circumstance escaped my notice at the time. Kelly was *hot and restless* for several hours. Nothing worth noticing occurred afterwards. I kept him till Sunday last in the workhouse. He then left perfectly well.

Case 4.—Mrs. Kemp, a poor woman who had been long an invalid. She was a cripple, and had been often under my care on account of chronic disease of the liver. She was attacked with the epidemic in a mild form on the 7th, and under the use of the saline treatment she had so far recovered, that I was visiting the patient only once a day. It seems that shortly after my visit on the 12th, she fell into a state of stupor. The surface became cold and clammy, and coloured of a dingy yellow. I was not informed of this state of collapse, and when I called next day, I found my patient dying. She was pulseless at the wrist. She could be roused for an instant to open her eyes. She would talk sensibly for a moment, and then drop into a state of lethargy. Without the slightest hope of advantage I used the injection. It brought back the pulse at the wrist, and heat to the surface. No other favourable symptom followed. She died fifteen hours after. I may remark that the collapse came on without any return of the vomiting or purging.

Continual vomiting and purging, highly characteristic of the disease, existed from the commencement of the attack up to the hour when the operation was performed.

Case 5.—Miss B., the last I have had in my own practice. She is 65, of a spare and delicate habit. She came in the morning of the 24th, from Brompton, to spend the day with a friend in my neighbourhood, who had just arrived from the country. She felt qualmish on her arrival. As she had occasionally felt so, her feelings did not alarm her, and her friends, whom she had not seen for years and to whom she was tenderly attached, attributed her uneasiness to the excitement of mind she felt on meeting them after so long a separation. Miss —— retired to bed for a few hours, meanwhile the symptoms became more and more declared. When I saw her at four, she was sinking into the state of collapse. Dr. Daun was sent for, and as soon as he arrived the operation was performed. This took place at nine in the evening; by this time the collapse was very great. The same relief as in the other cases was experienced. The cessation of vomiting and purging, the subsidence of the cramps, the restoration of the voice and looks, the return of the pulse, the rigor and the heat, and the restless following the shivering fit—the symptoms as already detailed in the other cases—occurred in this one.

It may be remarked, that no secondary fever occurred in the cases already detailed. Miss —— was the only patient who had it after venous injection. On the morrow after the operation the tongue was warm and dry, the pulse was not full, as it was immediately after the injection; great debility existed; she was much inclined to sleep. Several bilious stools were passed throughout the day and succeeding night. During the next four days this lady gradually retrograded; the diarrhœa continued, and gradually became of the colour of water, and she wasted in flesh; four or five stools on an average during the 24 hours. No urine was passed all the day after the bleeding; the sleep became more lethargic; the pupil contracted; the sensorium somewhat affected. She could sit up in bed and help herself to food, but would sit for a few minutes in a reverie, with the spoon filled and lifted half-way to the lips. She complained of flashes of light in the eyes; there was but little increase of heat over the head. On the morning of the 29th, a dozen leeches were applied to the head. The pulse this morning was much fuller than for the last four days. Miss — was relieved; ice was continued throughout the day to the head; a dozen and a half of leeches were applied in the afternoon; and finding considerable benefit from these two local bleedings, a vein in the arm was opened at midnight, and ten ounces more blood removed. This last bleeding gave the patient decided relief. She had an excellent sleep after, and was much refreshed in the morning. The pupil was now its natural size; the lethargy gone, and mind quite collected. She is now out of danger. This last case was one of great interest and much anxiety to me. I considered myself extremely fortunate in having Dr. Daun's great experience as a guide in the treatment of the secondary fever. As in all the cases of cholera that have come under my care (now upwards of thirty) I had not till now seen a well-marked case of it.

And now with regard to the internal treatment. In all these cases, as well as in all others I have had latterly under my care, I have used the saline treatment. The preparation I have found to rest most readily on the stomach, is the *subcarbonate of soda*. Of this salt I keep a solution prepared. The recipe is

Sodæ subcarb. ℥j;
Liq. opii sedativ. ℥ss;
Sp. myrist. ℥ss;
Aq. cinnam, Oiij. M.

Of this mixture a tablespoonful is given every half hour; and between each dose after the venous injection, a tablespoonful of brandy in a *little* hot water, until reaction be well established. Miss — took a pint and a half in this manner; the three others above half a pint. I consider it to be of the utmost moment to give these things regularly, and in these *small quantities.*

Occasionally the powder for the chalk mixture has been added to the above mixture in small doses. Occasionally the carb. ammoniæ in doses of five grains at a time.

Cases 6 & 7.—The two last on the list were patients of my friend and neighbour Mr. Stott. They had both been bled and treated with calomel and opium. Mr. Jones, my zealous pupil, saw one case. I saw the other. Both pulseless—both were rapidly sinking. Mrs. —— was so roused by the operation as to call for her baby, four months old; she sat up in bed and suckled it. For several hours she continued well, but gradually sank again into collapse, and died twenty-four hours after the operation. This patient I did not see, as I happened to be from home when Mr. Stott called. I performed the operation in Mr. Stott's presence on Mrs. Moore. The patient rallied amazingly for a few hours, but she too sank again into collapse, and died ten hours after the operation. She too had the *rigor* I mention as following the injection in the other cases. It was not followed by the heat so apparently as in the others. These two cases not having occurred in my own practice, I have requested Mr. Stott's permission to publish. I do not consider that the unsuccessful issue of either of them militates in the smallest degree against the operation. I relate them that they may act as a warning to those who wish to give this treatment any chance of success, not to allow the collapse to go on too far.

Remarks.

On comparing these cases together, I wish to draw the attention of the profession to two or three important facts.

Three of these cases recovered without secondary fever. At least the reaction that took place for a few hours scarcely deserves the appellation.

The case in which secondary fever came on was bled with the most decided advantage. The blood had the indications of inflammation to a high degree. This patient, it will be observed, was in years. How far this circumstance had to do with the induction of the fever in a *weak constitution* must be proved by future experience.

In all of them there were indications, after the operation, of the system having received a severe shock. In all more or less existed the rigor and the consequent heat. In those who recovered, this was most apparent.

Two of the unsuccessful had the *greatest* quantity of fluid injected. These two, however, were in a state different from the others; both had *taken opium*—both had been *bled.*

In three of the successful cases, no purgatives of any kind were administered; they were not required. The fluid used was of the nature and temperature adopted by Dr. Latta.

The apparatus was made on the spur of the moment. The brass nob of one end of my enema apparatus was cut off, and a silver blow-pipe introduced into the tube, air-tight. All that is necessary to secure the instrument from having air in it, is to force the fluid several times through it before it is used. Care must be taken not to lift the nose of the syringe out of the fluid when once the operation is commenced. As phlebitis has occasionally followed the operation, it is of great importance to handle the vein as delicately as possible. The neatest way of performing the operation, is to use the saphena vein immediately above the inner ankle. The tract of the vein being remarked by the eye, let an assistant push the blood upwards in the vein, whilst the operator's left thumb is on the vein, and below where the operation is to be performed. Cut with the point of the lancet a slit less than the fourth of an inch along the course of the vein. One or two scratches more with the point of the lancet will cut through the cellular membrane and lay the vein bare; the smallest puncture now of the vein to admit the end of the tube is necessary. No blood has flowed, and the end of the apparatus, all ready for action, is readily introduced into the orifice by the right-hand of the operator.

Of the four successful cases I have already mentioned, no inconvenience arose except in Conolly's case, where a little hardness existed for two or three days, and this I attribute to my own gaucherie. I opened the median basilic, and was obliged to cut twice.

From all I have said, I trust many may feel inclined to give this remedy a fair trial. It is a practice whose *immediate* advantages are as decided as the extraction of the tooth in toothache, the administration of opium in delirium tremens, or the free use of the lancet in peritonitis or pneumonia. In the meanwhile, until we know more about it, let it be considered merely as a temporary stimulus which, used with propriety, at least *does no harm.*

August, 1832.

TREATMENT OF THE
MALIGNANT CHOLERA
AMONGST THE SOLDIERS OF THE
2ND BATTALION GRENADIER GUARDS.

By J. HARRISON, *Esq., Surgeon 2nd Batt. Grenadier Guards.*

DIARRHŒA, with more or less of febrile excitement and gastric irritation, showed itself the latter end of April in the 2nd battalion Grenadier Guards; from this period till the beginning of June, when the regiment embarked for England, twenty-seven cases were treated in hospital, three of which number were distinguished by the symptoms of malignant cholera at that time prevalent in Dublin, characterised by vomiting and purging of a fluid resembling rice-water; great prostration of strength; a small, weak pulse, or one not distinctly to be felt; extreme coldness and livid hue of the surface of the body; contracted countenance; the sunken, dull, suffused eye; cold moist tongue; the peculiar voice; the shrivelled skin so strikingly observed in the hands. These symptoms were generally preceded or accompanied by vertigo and pain in the head; a total cessation of the secretions, cramps, and spasms; great anxiety, restlessness, thirst, sense of oppression, and burning heat in the chest.

Since our arrival in England, and being quartered in the Tower of London, 31 cases have been admitted into hospital, ten of which number were returned as well-marked cases of cholera; in the whole, up to this period, thirteen.

The plan of treatment adopted was, in the first instance, to administer a mustard emetic, which usually produced some re-action. If it failed to do this, and the pulse continued to sink, three or four ounces of blood taken from the arm seemed to relieve the circulation. Ten grains of calomel, with one or two of camphor, and one of opium, were now given every hour till the fourth dose, or if the stomach was very irritable, two-grain pills were preferred every ten minutes to the same extent of quantity. Camphorated mercurial ointment, with a small quantity of oil of turpentine, was assiduously rubbed into the abdomen, insides of the thighs, legs, and arms; heated sand-bags were applied to the extremities and sides of the body; mustard poultices and blisters to the pit of the stomach to allay the continued irritability of this organ. But the great object to be obtained was getting the system as speedily as possible under the influence of mercury, for it was observed that the least tinge of bile in the evacuations was a sure indication of the disease becoming subdued. Aperients were now given; rhubarb in peppermint water, or others of that class; cold, weak brandy-and-water given frequently in a tablespoon, to prevent a large quantity being taken at one time,—this for common drink; but when the thirst was very urgent, accompanied by that distressing sensation of burning heat in the chest, nothing appeared to be so grateful as iced water, or even a small lump of ice laid on the tongue.

Of these thirteen cases one proved fatal. In this instance the stage of collapse was fully formed when he was brought to the hospital, and although heat of the surface was partially renovated, re-action was never established. This man sank rapidly. Notwithstanding the large and repeated doses of calomel given (although not much of it might be retained), still the mercurial frictions were perseveringly continued, and chiefly depended upon, yet ptyalism to any great degree was in no one instance induced. Every case, however, was more or less affected. This mercurial affection of the sytem seemed to shorten and render milder the consecutive fever.

If this practice is deemed successful, there is a consideration to be borne in mind, which is, that soldiers, from their regular habits of living, and being well fed and clothed, are not a class of people one would expect to be great sufferers from cholera; and, moreover, the medical officer has a very great advantage over the private practitioner in the opportunity of treating the disease before it is much advanced, and also having all his remedies at hand, and all the assistance he can desire in their administration.

The above relation is intended to convey a well-grounded belief in the efficacy of the mercurial plan of treatment in cholera, and more particularly the application of mercury in the form of inunction.

Tower, August 7, 1832.

LETTER FROM DR. ANDERSON.

To the Editor of THE LANCET.

SIR,—I feel it necessary to set myself right with the profession and the public, as to some remarks made in a letter from Mr. Robertson, surgeon of the *Cumberland*, Convict Ship, at Chatham, published in your last LANCET; from which it might be inferred that in a communication I made to the Central Board of Health, and printed without my knowledge in a preceding LANCET,* I had intentionally suppressed or mis-stated the result of the three cases reported as convalescent. My letter was one of daily communication to the Central Board of Health, and not written with the view of publication, but to give an account to the Board of the progress of the disorder, and of the daily alteration we saw necessary to adopt in the treatment.

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* The letter of Dr. Anderson was forwarded to us from the Central Board of Health, with the impression, undoubtedly, that it was a public document, and adapted for insertion.—ED. L.

"Three additional cases were selected yesterday, and this plan of treatment adopted, and I am happy to add with most decided benefit. They are all *at this moment convalescent.*"—Lancet, June 23.

In the three cases that I mentioned, there was, when I wrote, such a complete remission of the choleric symptoms, that I felt myself justified in considering them convalescents. One of the cases, Eccles, lived nine days, Longbottom two days, and Cooper is still alive. As the after treatment did not devolve upon me, I am unacquainted with the circumstances of the death of the two former; but had I written for publication, and after I was acquainted with the want of success which attended the practice of venous injection by Mr. Robertson, I should perhaps have written less sanguinely.

On again perusing my letter, I can perceive no more of the *exultation* of which he accuses me, than the gratification which every humane man would feel at the apparent confirmation of Dr. Latta's discovery of a remedy for this dreadful disorder.

It is to be remembered that the venous injection was employed in *those cases only* in which a fatal termination seemed almost inevitable; if, therefore, life is preserved in a single one of these cases, the medical attendant may be forgiven for a little *exultation.*

Having made the above observation, in reply to Mr. Robertson's remarks I have to state, that I am still of opinion that venous injection is a powerful means of cure, but I shall be inclined in future to bleed freely in the opposite arm, during, or immediately after, the process of injection, as the death of all the cases I witnessed after its use seemed to arise from cerebral congestion. I am, Sir,

Your obedient servant,

John Anderson, M.D.,
Surgeon, R.N.

Rochester, Aug. 5, 1832.

LITHOTRIPSY.

REPLIES OF MR. BIGGS AND MR. GOODWIN TO THE LETTER OF MR. COSTELLO.

To the Editor of The Lancet.

Sir,—The nature of Mr. Costello's last letter is such, that by publishing it I am afraid he will only add to the unfavourable impressions which his former ones must have left to his own disadvantage. The task of proving to Mr. Costello that he did not, quite so strictly as he should have done, adhere to truth, was so unpleasant, that I shall not enter into the contents of his last publication, or attempt to pursue the very handsome mode of reasoning which is there so eminently displayed.

I will merely make one observation respecting the letter which M. Arago, secretary to the Institute of France, addressed to Mr. Costello, announcing to him that his letter had been read in open Academy. M. Arago, without doubt, did write to Mr. Costello, but, in doing so, he merely discharged an official duty devolving upon him, which is nothing more than a well-known academical form, for it is impossible to tell the author of a letter, whoever he may be, that his letter was received, and, at the same time, that it was not read. The letter, as I before said (and I was well informed), was not read in open Academy; and Mr. Costello well knows that the *secretary* never reads letters of such length, even if the matter they contain be of the most interesting nature. The real fact is, that instead of being read, they were glanced into, and their contents being soon ascertained, they were sent to be perused by the committee selected by the Academy to examine Baron Heurteloup's labours in *lithotripsy.* It is impossible to say whether they have been or will be read by the gentlemen who form this committee, but I do not suppose that they require the assistance or advice of Mr. Costello to form an opinion on what is submitted to their inspection. With respect to the paper (the *Messager*) which Mr. Costello brings forward to prove that his letter was not only *read*, but listened to with the most *profound attention*; it merely proves that it was read by the journalist, who always, after a sitting of the Academy, peruses all the papers introduced, and publishes extracts from them. This is all that requires an answer in Mr. Costello's last letter. That gentleman would do far better to publish and substantiate by authentic proofs his numerous successful cases, than write long and unmeaning letters in which he so often contradicts himself, and which, as I before remarked, are evidently not written "in a scientific point of view."

Before I conclude, I cannot omit complimenting Mr. Costello most sincerely on having *seen* the Baron Heurteloup's curved instrument (which unfortunately is not the case), on *possessing* it, on having *operated* with it, and, above all, on having so far *improved*, *modified*, and *strengthened* it, as to render it a safe and valuable instrument, and so all at once to have cleared it from the numerous reproaches and serious inconveniences, which but a fortnight or three weeks before he so strenuously heaped upon it in his letters to the Academy.

With respect to my youth, with which Mr. Costello so frequently finds fault, I am sorry that he should consider it a cause of offence. I will merely remark, that my youth can be no admissible reason why I should not write as well as speak the truth, even to my superiors in age and acquirements, should they place themselves in such a position as to require explanations and statements of facts to clear up their *ambiguities*, not to say more.

I have the honour to be, Sir,
Your most obedient servant,
THOMAS T. BIGGS.
Vere Street, Cavendish Square,
July 30, 1832.

NOTE FROM MR. GOODWIN.

To the Editor of THE LANCET.

SIR,—Mr. Costello has written in last week's LANCET a letter, in which he alludes to the statements that I have before given, and asserts, that my testimony and that of Mr. Hodges were offered in support of facts, which he for the second time treats as incorrect.

What other motive could I have had in giving that statement than the establishment of truth? And with the same object in view, I once more assert, that what I stated in my former letter was *scrupulously true*.

With respect to the letter bearing my signature, which Mr. Costello has inserted, I have not sufficient recollection of what I wrote to venture to speak of its authenticity; but it matters little, the fact subsists. Mr. Costello, I again repeat (and Mr. Hodges will feel pleasure in again certifying), operated upon me with the *lithotrite three different times, as stated in my former letter*.

With respect to any other observations in my letter, my object was merely to present facts as they occurred, and this I have done faithfully.

I regret much that it should be necessary for me again to appear before the public, in a controversy from which I would willingly have refrained. But my obligations to the Baron Heurteloup, and my sense of justice called upon me to take this step.

I have now spoken so decisively upon this subject, that I shall think it unnecessary, whatever answers may appear from Mr. Costello, to give any further reply to a subject on which your readers must, I am sure, be able to decide correctly.

I have the honour to be, Sir,
Your most obedient humble servant,
GEORGE GOODWIN.
Chapel-street, Grosvenor-place,
July 29, 1832.

[As it is quite time that this controversy should terminate, any other letters on the subject can only appear as advertisements on the cover.—ED. L.]

NEW MODE OF PREPARING

PURE SUGAR.

To the Editor of THE LANCET.

SIR,—I avail myself of the opportunity which your Journal affords, in occasionally inserting notices of scientific interest and inquiry, although not immediately connected with medical science, to announce to your readers a fact important both in a philosophical and commercial point of view, the preparation of pure sugar direct from the cane juice, without any subsequent process of decolorisation, solution, or refining.

Whilst this most important desideratum to the planter is effected, it must be considered a proof that molasses were but a product of the former mode of operation, from the intense and long-continued degree of heat employed in the processes, rather than an educt from the sugar cane, in which light it has been uniformly viewed by former chemists. The improvement of the present over the ordinary mode of operation, consists in effecting the last stages of concentration in a vacuum pan deprived of atmospheric air by means of an air-pump, the process which was first proposed by the Hon. Edward Charles Howard, and most successfully introduced into operation in the sugar-refineries of Great Britain, and by which the heat that is employed is not sufficient to produce any change in the constitution of the elements of the sugar. A concentration of the juice at the reduced temperature, required in the operation, effectually prevents the formation of molasses, and the sugar is preserved from undergoing that extensive partial change and decomposition in which it has ever uniformly before been transmitted to our hands.

The sugar thus obtained is in perfect, pure, transparent, granular crystals, developing the form of the crystal of the sugar, and being entirely free from the least portion of uncrystallisable sugar, molasses, or colouring matter. Its taste is very mellifluous, and is not approached even by best refined sugar, than which it is more valuable for all purposes of solution or for domestic economy.

I have forwarded a sample for your notice and inspection, and for the information of your readers may state that samples may be had of Messrs. Oaks and Son, 97, Houndsditch, manufacturers of the apparatus, to which gentlemen, science is indebted for the successful and full development of the application of this process, the effects of which cannot but be most beneficially felt by all who are concerned in this interesting portion of colonial and commercial enterprise and production. I am, Sir,

Your obedient servant,
Abr. Booth.

July 25, 1832.

⁂ The sample of sugar which accompanied the above letter would fully bear us out in any compliment we might pay the article. The product shows that the process is perfect. *Apropos* to the manufacturers. As infinitesmal proportions of copper are said to be contained in sugar, we should have no objection to the opportunity of submitting a score or two pounds to the process necessary for their detection.—Ed. L.

THE LANCET.

London, Saturday, August 11, 1832.

In No. 19 of the *Glasgow Medical Journal*, just published, there is inserted the Thirty-seventh Annual Report of the "Glasgow Royal Infirmary." From this document we select certain paragraphs, as containing matters which are not undeserving the attention of the profession.

The readers of The Lancet need not be reminded of the unremitting constancy with which we have enforced the claims of surgeons in general practice to the confidence of the public. Had our arguments been founded on prejudice or false premises, they would have carried little influence with them into the ranks of well-informed society. But founded as they have been on a correct estimate of all the circumstances which build up the medical character, they have been duly estimated and acted on by all those individuals who have had an opportunity of perusing or considering them. Convinced, however, as we were, of the delusion which existed in the public mind as to the degrees of confidence which ought to be reposed in the members of the various branches of the profession, we could scarcely hope, and certainly did not expect, that the reform produced within so short a period would lead to an acknowledgment from the managers of the "Royal Infirmary of Glasgow," that "the number of acting physicians in that city had been lately so much diminished that the managers had neither the means of appointing a third physician, if necessary, nor supplying the place of either of the present ones, when prevented on account of bad health, or otherwise."

Thus then we have the strongest evidence in proof of the rapid decay of the dubbery system, that system which, during so many centuries, has robbed ability of its merit, and industry of her rewards. The false, delusive, title of "Doctor," has caused the death of thousands; for the results of years and years of the most valuable experience, have been neglected and abandoned for the treacherous claims of a false and empty name. Until this journal was established, the public had no means of ascertaining what was the value of the title to confidence set up by the different branches of medical practitioners. Up to that period they had been taught by interested parties to believe, that "Doctors in medicine" were profoundly wise in the treatment of those maladies which afflict the human frame; that apothecaries or general practitioners, or, speaking more correctly, surgeons in general practice, were subordinate members of the profession, who, in all dangerous diseases, were called on to submit with servile complacency to the mandates of individuals boasting the title of "Doctor."

The bubble, however, has burst, or, rather, *it has been cut open*, and the eye no longer gazes on the cameleon hues, reflected, at best, by nothing but an empty phantom.

With respect to acting physicians, the city of London is pretty nearly in the same state as the city of Glasgow. Gentlemen who may legally claim the title of "Doctor," have been compelled to enter the ranks of surgeons in general practice,—observing, correctly, from what was passing around them, that if they omitted to resort to such a measure of pretension, they would be compelled to sink into irremediable poverty and obscurity. The facts recorded in the second of the two following paragraphs, deserve the most deliberate attention, not only of the members of the profession generally, but of the governors of the whole of our great national hospitals.

" The number of acting physicians in this city has been, from what cause we know not, lately much diminished; hence the managers had the means, neither of appointing a third physician, if necessary, nor of supplying the place of either of the present ones, when prevented from attending on account of bad health or otherwise. The managers, therefore, after very mature deliberation, and being well advised on the subject, whilst they disclaimed all intention of passing over the present attendant physicians, did, on the first day of August last, on the motion of the Lord Provost, seconded by the Preses of the faculty of physicians and surgeons, enact, " That any gentleman who shall have been fifteen years in general practice, and who shall have obtained the degree of doctor of medicine previous to his election, shall be eligible to be physician to the infirmary.' This measure will secure a supply of medical attendants, and, we are confident, will neither injure the character of the establishment, nor be hurtful to the paramount interest of the diseased poor.

" The plan adopted for the establishment of clinical lectures, has, after two years' experience, been found to answer the intended purposes; and has, from the zeal and abilities of the lecturers, proved of singular benefit to the medical school. It is satisfactory for the managers to state to this court, that the result of the measure on our funds has turned out the reverse of the apprehensions which some persons entertained when the subject was under discussion; for, during the last session of the College, there has been received from the attending pupils, nearly the sum of nine hundred pounds; of which three hundred have formed the remuneration to the lecturers, and the remaining six hundred have been available to the support of the institution. The regulation, therefore, has brought an increase to our revenue, while at the same time it has served another public purpose—it has improved the infirmary as a school for medical experience, from which incalculable benefits will be derived to persons of all ranks, both in this and future generations."

The managers of the Glasgow Infirmary have not left the instruction of the students to be a subject for the amusement or caprice of the medical officers, but they have prescribed rules for that particular end, the observance of which they have strictly enforced on the part of all the official functionaries. How opposite is the government of the hospitals in this metropolis! *Here*, so far from there being any well-organised arrangement, the whole business exhibits a complete chaos. If we omit the services of some three or four hospital physicians and surgeons, the pupils who resort to the medical institutions of London for clinical information, leave the metropolis just as experienced as when they entered it, and no more. Deficiency of emolument these medical officers cannot plead as an excuse for deficiency of exertion, for the fees paid by surgeons' pupils to Bartholomew's Hospital, for instance, alone, for the practice of the hospital only—leaving lectures entirely out of the question—reach 4000*l.* annually, not one shilling of which is applied to the benefit of the patients! Yet in the Infirmary of Glasgow, where the duties are so efficiently and zealously discharged, 600*l.* out of the 900*l.* paid by the students, are appropriated, according to the statement of the managers, to the charitable purposes of the institution. If the physicians and surgeons of the hospitals of London could feel any sense of shame, surely their cheeks would be crimsoned on perusing such a document as this. But, unhappily, blushing is not one of the characteristics of the cold-hearted, cruel BATS. English students may visit the establishments here,—disgorge from their pockets their last shilling,—labour zealously in the pursuit of knowledge,—study unceasingly,—and then receive, as their only reward, THE WORTHLESS LICENSE OF A TRADING COMPANY OF APOTHECARIES, or the DESPISED DIPLOMA of a mercenary, monopolising COLLEGE OF SURGEONS.

WE invite the attention of our readers to the cases of venous injection performed by Dr. GIRDWOOD, and recorded in THE LANCET of this day. Of seven cases thus treated, five have been successful*; affording, thus, triumphant proof of the positively beneficial results of this novel and interesting practice.

The errors which medical men perpetually commit in their reasoning on the effects of remedies, are amongst the most egregious in the examples of the *non sequitur* species of logic. If in any given disease a dose of calomel, of jalap, of asafœtida, be administered, and if thereafter the patient recover from his malady, straightway the professional casuist exclaims that the recovery has been a "cure," and that his remedy was the *cause* of the happy issue. Again, extending his inference a little wider, he argues that what *cured* A, will, to a certainty, *cæteris paribus*, exercise an equally sanitive influence over B. At last his generalisation reaches universality itself. How strict and sound reasoning of this description is, the following anecdote suffices to show:—

About four weeks since, the wife of a violoncellist, residing in one of the metropolitan districts, was attacked with cholera. The parish surgeon, a gentleman of known ability, and who had seen and treated nearly 200 cases, was called to her assistance. He arrived, saw her desperate condition, and sought the additional advice of the medical inspector of the division. The inspector came, shook his official head, and—the aid of a third and most experienced practitioner was invoked. A full consultation was held, and it was unanimously agreed that the case was all but hopeless. The unhappy female was cold, damp, and pulseless; she was blue, and voiceless, the evacuations were so profuse as to excite astonishment as to whence they came; in short, it was perfectly evident that if something were not done, she would die within the hour. At least so thought the three experienced attendants, for they proposed, as a last resource, the venous injection, then recently introduced for the treatment of the disease. The violoncellist at first consented, then wavered, and at last retracted his permission, and the medical men withdrew, deploring his unaccountable obstinacy. Well, strange to say, this woman, thus abandoned by her physicians, not only did not die, but actually made a more rapid recovery than is generally witnessed in the most trivial cases; and from the time of her abandonment till her freedom from danger, she took no medicine whatever.

Now, it is quite clear that had the operation been performed here, it would have been published as an example of the *successful treatment* of cholera by venous injection. And thus it is with calomel, and thus with rhubarb, and thus with the muriate of soda. Men systematise and conclude on facts, which, in reality, have nothing to do with the events on which they reason.

But the case is widely different, when,

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* One case which was under treatment when Dr. G.'s letter was written, having since recovered.

in a *group* of half a dozen instances of marked and manifest severity, we find, at the same time, a diminished degree of mortality; and when, what is more to the point, we mark the *instant* amelioration of symptoms which follows the employment of the therapeutic agent. If, after a medicine is taken, hours elapse before improvement is perceptible, we are clearly not entitled to infer that the remedy was the cause of the improvement; but, on the other hand, when, in a plurality of cases, the cold become *instantly* warm, when the arteries of the pulseless *instantly* renew their beatings, and when the voice *immediately* returns, no other conclusion is left but that the change was the effect of the measure.

Having now before us the cases recorded by Drs. Latta and Girdwood, and bearing in mind how far the success of the operation depends upon the manual adroitness of him who carries it into effect, we feel that no amount of negative evidence could be sufficient to warrant the practitioner in withholding this last resource from his otherwise abandoned cases. The remedy is doubtless in many instances too weak to cope with the virulence of the disease. In many, however, it will succeed, and we are bound to afford our patient every chance that may exist, however slender even the probability of success may be.

We have received from Dr. O'Shaughnessy an account of his analysis of the blood, drawn in one of Dr. Girdwood's cases (Miss B), four days after the operation of injecting four pounds of water, two drachms of carbonate of soda, and three of muriate of soda. In the interval between the injection and the abstraction of the blood, the diarrhœa had continued, and no urine had been passed. The results are very interesting. The quantity of water in the blood was 778.76 per 1000, being exactly the natural or healthy standard. The quantity of colouring matter 124·51—of albumen 78.86, being also natural proportions. The quantity of pure salts was only 4.15, being considerably less than half the normal standard. The serum of the blood, moreover, contained the immense quantity of two grains and one-tenth urea per 1000 parts.

During the second irruption of cholera in London, the same gentleman has availed himself of the occasion to repeat his chemical inquiries relative to this disease, on a most extensive scale, and with a view, if possible, to decide the question, whether the alteration of the blood be primary or secondary, and to ascertain what are the conditions of the blood in the several *stages* of the disease. We subjoin the conclusions at which he has arrived.

1st. In the *premonitory* symptoms, no alteration of the blood exists.

2nd. In the cases in which the evacuations are trivial, and *cramps* form the prominent symptoms, the blood is also unaltered.

3rd. The alteration of the blood consisting in loss of water and saline matter, only occurs in the collapse cases *preceded* by excessive rice-water evacuations.

4th. This alteration of the blood gradually disappears, or increases, in the fever stage, according to the aggravation or amelioration of the symptoms.

5th. The quantity of *urea* in the blood in cholera is most remarkable in the secondary fever, sometimes amounting to 3 grs., in 1000, and bearing a strict proportion to the duration of the retention of urine. Dr. O'Shaughnessy has, however, reason to believe, that urea, in minute quantities, forms a perpetual constituent of healthy blood. He is, however, pursuing this branch of the inquiry farther.

6th. The blood of healthy persons resident in infected districts or houses, is not chemically changed in the least degree.

These results are founded on minute analyses performed on a great number of specimens of blood. Full details will in due time be published in this journal. At present it suffices to say, that these experiments afford ample data for deciding on the perfect inapplicability of the "saline treatment" to the *premonitory* symptoms. They also decisively prove that the alteration of the blood is altogether *secondary*, and they show that saline substances cannot act as prophylactic or preservative agents, as some have asserted. We shall avail ourselves freely of these facts in a detailed review of Dr. Stevens' work, which we hope speedily to lay before our readers.

The cholera, we are happy to state, continues to decline in London and its environs. In some districts a considerable increase of cases took place on the 2nd inst., the day on which the great thunder-storm occurred. A similar increase of cases marked the 8th inst., the day on which the weather resumed the sultry temperature of the season. We repeat, however, that the disease is, generally speaking, on the decline. We derive our information on this subject from the best of all authorities,—the medical men attendant on the cases—and we offer our sincere thanks to the gentlemen who have so readily answered our call for information on this subject. We assure them we will not abuse their confidence.

EXECUTION OF DR. BOZZI GRANVILLE,

In the last number of the "Quarterly Review."

(From a Correspondent.)

This stanch anti-contagionist is utterly crushed in the last number of this Tory journal. The worthy doctor is treated throughout with the most biting irony. The wretched pamphlet which he published recently, and entitled "A Catechism of Health," has brought upon him this well-merited castigation. After ridiculing in a very happy style the pompous pretensions of this modest personage, the reviewer satisfactorily demonstrates, that although the title-page of the pamphlet in question indicates a fourth edition, that more than *one edition was never printed.* He next adverts to Dr. Granville's assertion, that his original work was completed *in a fortnight*; and that, consequently, three hundred and thirty-six closely-printed pages of a work, taking so universal a range over so extensive and complex a subject, was composed and written at the rate of *twenty-four printed pages per diem*; and that, too, during the *leisure hours* of a physician of "great public and private practice." What copying clerk could write with a rapidity equal to the inditing speed of this bibliopoietic physician! The reviewer subsequently intimates upon pretty strong grounds, such as the actual comparison of the two productions, that Dr. Granville's Catechism is nothing more than a plagiarism (verbatim et literatim) of the "Catechism of Health" of Dr. Faust, published in 1792, and translated into English by Mr. Basse. Some excellent specimens of intellectual pap with which the Doctor afterwards supplies his readers, are served up, well-seasoned with gall and wormwood. Since the days of Dennis, the literary sun has seldom shone upon such a merciless hanging, drawing, and quartering. The magniloquent Doctor is no more. His occupation's gone. In vain will the genius of impudence exclaim *Resurgam.*

WESTMINSTER HOSPITAL.

PUNCTURED WOUND OF THORAX.

George Garrot, a stout well-built fellow, about 30 years old, and evidently of hale constitution, was admitted on the night of the 21st June with a wound of left side. He had been fighting in a public-house of the vicinity, and during the struggle had received a stab with a knife between the seventh and eighth ribs, close on the confines of the præcordial region. On admission, the man was evidently under the influence of liquor, there was much anxiety of countenance, the respiration was accelerated, and the pulse irregular. The wound was linear without any angle, and the cellular membrane for about an inch round it was emphysematous. The patient complained of acute pain in the part, especially when he assumed the recumbent position. The stethoscope indicated nothing preternatural in the breathing, there was a loudness in the respiratory murmur occasioned apparently by the accelerated breathing alone, but it was everywhere audible. Percussion afforded no symptoms. There was, however, a remarkable irregularity of pulse both at the heart and wrist; the extent and impulse of the heat were natural, but there appeared an uncommon audibility in its sound. *Venesection to 20oz. A strong purging draught immediately.*

22. During the bleeding the patient experienced relief, and was enabled to draw a tolerably free breath. Vomiting succeeded which benefited him materially. The action of the heart and arteries became regular, and he slept at intervals during the night. This morning pulse 86, breathing nearly natural, the emphysema has disappeared from the cellular membrane adjacent to the wound, which is clean, and shows a disposition to heal by the first intention.

24. The patient is convalescent; the wound has nearly healed. All the functions are tranquilly performed, but debility and pallor remain, the consequences of the debauch and subsequent depletion.

CONCUSSION OF THE SPINAL MARROW; PARAPLEGIA OF THE LOWER LIMBS.

David Spearman, a stout lad, 23 years old, was admitted on the 12th of July, having fallen from a scaffold at the New Palace, a height of sixty feet. When picked up he was found stunned, and brought to the hospital. When received he was sensible, and complained of general numbness, pain of loins and legs, and an inability to stand. No bones were broken; there was considerable tenderness in the lumbar region, and the sensation of legs and thighs was gone. He had likewise but little motive power in the lower limbs, and when attempting to bend them on the pelvis was obliged to raise them with his hands. Pulse about 80, irregular. Breathing accelerated, countenance anxious. *Venesection to 16oz.; saline aperient medicines.*

26. A reaction occurred, and was vigorously suppressed by antiphlogistic means. The venesection was thrice repeated, and the loins were twice freely cupped. The emunctories were soon restored, and the patient soon regained his power of sleep, and his appetite for food. He has been slow however in regaining the use of his limbs, which still feel numb, and cannot be removed without assistance; but the progress towards convalescence, though slow, appears to be sure. The action of the bladder was never in the least degree impaired.

FRACTURE OF CRANIUM, DEPRESSION OF BONE, TREPHINING; HERNIA CEREBRI.

Thomas Walsh, ætat. 27, a bricklayer's labourer, of stout build and healthy look, fell from the same scaffold at the same time as Spearman; he was picked up insensible. Soon after his reception in the hospital he became sensible; a tumour rapidly rose round the wound in the posterior part of the scalp; a fissure was detected running across the upper part of the occipital bone, and crossing the lambdoidal suture above the left ear. The irides acted naturally, the respiration and circulation were tranquil, and the intelligence unimpaired. He was bled to 20 ounces, lotions were applied to the head, and aperient medicines were exhibited.

13. The patient passed a tolerably quiet night, but he has a headache this morning. The pulse is 90 and hard, the eyes impatient of light, but the intellect is unclouded. Venesection to 20 ounces again. This afforded relief, and the patient fell asleep.

14. The lowering plan has been pursued till to-day, when the patient was seen by Mr. White and Mr. Guthrie, who for reasons which they did not explain, determined to trephine him. The scalp being incised freely, the posterior lip of the fracture was found depressed, and dipping under the anterior edge. In its course the fissure described several angles, and one or two of these were removed with Hey's saw, in order to afford room for the introduction of the vectis under the depressed bone, but all efforts in this state of things were found inadequate to raise it up. A circular piece of bone being taken away from the fixed bone, success attended at last the surgeon's attempt in replacing the dislocated portion of cranium, and the patient declared himself relieved by the operation. Lotions; low diet; soothing antiphlogistic treatment.

15. The patient is removed to-day to a large ward; irritability; pain of head; pulse 96, quick; venesection to 12 ounces.

17. The patient was relieved by the bleeding, and felt comfortable all day yesterday. This morning the wound of scalp, which discharges freely, has a slight prominence in the centre, indicating an incipient hernia cerebri. There are pain and sense of fulness in the head. Cupping from the temple to 12 ounces.

21. The abstraction of blood proved beneficial. The hernia, however, rose to some height in a day or two, and then sloughing, was removed by Mr. Taylor. An immediate amendment was perceived in the condition of the patient; he became placid, slept quietly, and the pulse fell to 88, becoming soft, though slight causes made it vary considerably.

29. The patient appeared much better for several days. The wound showed a granulating surface, and the brain showed no disposition to throw out a fungus till this morning, when a slight projection became visible; pain was felt in the part. The tongue became furred, the pulse accelerated, and general fever and irritation supervened.

30. Symptoms much aggravated. The hernia is as large as a small tea-cup; the patient pallid, breathing rapid and quick, as much as 30 per minute; expression of countenance cadaverous; pulse 100, strong, quick, and yet irregular. Cupping to the amount of 24 oz. from the temples; saline aperients.

31. Quieted by the abstraction of blood; at night, a portion of the slough removed by Mr. Guthrie. The anxiety of countenance is increased; breathing accelerated; pulse 90, irregular in force and frequency; bowels open; skin bedewed with cold perspiration.

We shall keep an eye on this case, which is now desperate, and look with anxiety for the surgeon's reasons in deviating from the established canons of surgery in the treatment. We trust Mr. Guthrie will favour us with a clinical lecture on the subject.

PARALYSIS—FUNGOID DISEASE OF TESTIS.—COINCIDENCE OF THE TWO AFFECTIONS.

Patrick Cormack, 35 years old, a paviour, admitted 5th of June, 1832. He has generally enjoyed good health. About a year and a half ago, he was seized with a fit which came on in the night, and deprived him of the use of his right side, and particularly the upper extremity. This attack was preceded by headache and vertigo, which have occasionally recurred since that time.

He has been a good deal addicted to venery, and has indulged in excessive potations of a promiscuous nature, frequently to the extent of intoxication. The present attack came on about a week ago, and affected the same side as before. The right arm feels much colder than any other part of the body. The right knee is tumefied, and gives a sense of fluctuation to the touch, but the whole of that limb retains its natural warmth. The testicle of the same side is much swollen and hardened, and he states that it also became so after the first attack. He has been under the care of Mr. White as an out-patient, who (according to patient's account) made an incision into the tumour, and let out a considerable quantity of pus. There is still a purulent discharge. The pain of head has lately been increased in severity. His memory is evidently impaired, and his answers are incoherent and indistinct. Tongue slightly furred; bowels open; pulse 88, full, and soft; appetite good. To be cupped in the nape of the neck to eight ounces. A quarter of a grain of strychnine to be taken three times a day.

10. He has a tickling sensation over all the surface of his body; pulse 100, full, and soft; tongue clean; bowels open; appetite bad. The affected knee is more easy. To persist with the strychnine.

12. Feels more lively. Can stand better than he did; has a little more power in his arm, which feels warmer. Complains of pain and giddiness of head. The knee much less swelled and less painful; tongue clean; bowels open; urine scanty; testicle much the same; pulse 84, full, and soft; cupping on the temples to twelve ounces. To continue the strychnia.

16. Complains of pain of left side, increased on inspiration, and exciting a cough. To this he has occasionally been subject for a year. Percussion gives tolerably laudable sounds in every region of the chest. Can expand the left side of thorax to its full extent, but has hardly any power over the paralysed side. The respiratory murmur is natural, but a slight râle sonore is heard in left lateral and posterior region. Has cough and mucose expectoration; tongue clean; bowels confined; pulse 96; makes water with a feeble stream; cupped to twelve ounces on the nape of neck; head felt lighter afterwards, but the noise in the ears was not removed. *A strong aperient draught to be given immediately.* Twelve leeches to be applied to the side, and the strychnia to be continued.

20. Has been cupped with benefit upon the affected knee. The pain of side occasionally returns at night; secretion of urine scanty. The bowels being very costive, have been acted upon by aperients. Tongue clean. The testicle still presents an indolent ulcerated surface, assuming a fungoid character in the centre. Persist with remedies.

July 2. Since last notation, no important variation of the general symptoms; pertinaceous habit of constipation. A tumefaction of the affected testis has taken place. The muscular power of the patient is manifestly improved. Has had a warm-bath, from which he derived signal relief; pulse 100, soft; testicle in *statu quo.*

6. Power increasing daily. Great difficulty in making water, relieved by the passing of a bougie. Pain of knee; ordered a tobacco poultice.

9. Pain of knee not relieved by the tobacco poultice; has more muscular power in the affected parts. The testis was examined this day by Mr. Lynn, who is of opinion, from the lobular shape of the tumour, that the disease is of a malignant kind, and should consequently be extirpated. Dr. Roe being of a different opinion, declined consenting to the operation, and determined to treat the case with alteratives; bowels open; pulse 80, full, and soft.

13. Has more power over the paralytic arm, but the pain of the knee has much

augmented; testicle smaller and hard; expels his urine with more force.

20. Dr. Roe has prescribed the ioduret of mercury to the testicle, which appears improved; the general system is placed under a simple antiphlogistic regimen; pulse 90; bowels torpid. A strong purging dose daily.

27. The testicle is softer; a slough has separated from the surface of the ulcer, and healthy granulations remain; the ioduret of mercury ointment has been continually applied. The paralytic affection is evidently improved; pulse 80, soft, and regular; low diet.

Aug. 4. The patient's appearance strikingly improved; his bowels act spontaneously; his face is acquiring a hygienic hue, his speech is more distinct, and his step more decided. The testis is reduced to nearly its natural size, and presents a healthy granulating surface. The surrounding tumour is entirely absorbed; appetite good; middle diet; pulse 80, soft, and regular, and the general opinion is, that he has sufficient springiness of constitution to throw off every vestige of disease.

LONDON HOSPITAL.

DISEASE OF THE HIP-JOINT.

H. A., aged 12, was admitted March 22d, under the care of Mr. Scott, with disease of the hip-joint.

The following were the appearances on admission. The patient, a delicate-looking girl, complained of great pain in the joint on motion. She lies in bed, with the knees drawn up towards the abdomen, and on drawing them down, she complains of severe pain. She has also one symptom which is so characteristic of disease in the hip, viz., pain in the knee, the affection is of about six months standing. The following treatment has been pursued:—eight leeches were applied around the joint every morning for a fortnight, after which blisters were applied to the same part; the blistered surface dressed with the ung. hydrarg.

May 5th, Mr. Scott ordered the hip to be dressed with the *unguentum hydrarg. c. camphor.* spread thickly on lint, then to be strapped with the *emplastrum plumbi*, after which to be covered with *emplast. sapon.* on leather, and over the whole a roller, nine yards in length, so as to keep the joint in a state of perfect quietude.

The treatment in this case has succeeded remarkably well. The patient has been discharged, having been in the house for a long period. Previous to the child leaving the hospital, the hip was dressed in the same manner. The child could move the limb in any direction without feeling pain but was ordered to be kept quiet in order to prevent any return of the disease.

STRUMOUS OPHTHALMIA.

E. S., ætat. 33, married, of a strumous habit, has been subject to attacks of inflammation of the eyes for several years. When admitted, she stated the eyes had been inflamed for about a month; but within a week preceding her admission they had become much worse.

On attempting to raise the lid, for the purpose of examining the eye; the intolerance of light was so excessive, that she could not for a moment bear the impression. There appeared to be great increase of vascularity of the eye, a number of vessels were seen running towards the cornea, and extending over its margin; the cornea itself appeared to be altered in texture, and opaque, there was a copious discharge of hot scalding tears, and shooting pains about the temples and forehead, vision was much obscured.

Mr. Scott ordered a large blister to be applied to the back of the neck, an issue to be made in each temple, and to take two grains of calomel every six hours, until the mouth became sore.

Under this plan of treatment, the patient experienced the most decided benefit. In the course of a short time the intolerance of light was much lessened, and the cornea appeared less opaque. The blister and issues afforded great relief on their discharging freely.

After a time, Mr. Scott directed the undiluted *liquor plumbi* to be dropped into the eyes, night and morning, and the patient has since left the hospital cured.

INJURY OF THE HEAD.

M. L., labourer, ætat. 38, was admitted under the care of Mr. Andrews with injury of the head.

On his admission, from intoxication, and the effects of the injury, he was incapable of giving any account of the manner in which he came by the accident, but we learned from one of the men who brought him to the hospital, that he had been quarrelling, and continued stunned for more than half an hour after the accident, and that there had been considerable hemorrhage from the head.

On examination, an extensive laceration of the scalp was found a little above the left temple, which left the bone denuded. The bone was at first supposed to be fractured, but on more minute examination this was found not to be the case, the supposed fracture being merely an indentation in the bone. The wound of the scalp, he said, was inflicted by a pewter measure.

The wound was dressed with straps of adhesive plaster, the man was bled to twenty ounces, and ordered to have a dose of house-medicine directly, and to keep the head constantly wet with spirit wash.

3rd. Passed a bad night, and complains of great pain in the head; tongue dry, and covered with a white fur; pulse full and strong; bowels opened once; he was ordered to be bled freely, and to take calomel with jalap, a scruple directly, and two grains of calomel every four hours.

4. Slept for several hours during the night; bowels freely opened; pain in the head diminished; tongue less furred; pulse full and strong. The wound of the scalp does not look healthy; the adhesive straps have been removed, and a poultice applied. To repeat the venesection and continue the calomel.

5. Passed a very indifferent night, had no sleep, and was at times delirious; pulse 104, full; skin hot and dry; tongue furred; bowels open three times. To repeat the venesection, and continue the pills. The wound has a very unhealthy appearance, but the surrounding parts are less tumified.

6. Slept tolerably well during the night, and feels more comfortable this morning; pain in the head much diminished; tongue less furred; bowels open; skin more moist and cool; complains of his mouth being sore. To take the pills night and morning, instead of every four hours.

8. Much the same as on the 6th. The wound looks better, and discharges healthy pus; his bowels are open; skin cool; tongue clean; mouth much affected by the calomel. To take the pills only at night.

10. In every respect doing well; to discontinue the pills; the wound looks healthy.

12. From this time the man has had no bad symptom, and has since been discharged quite well.

The lad operated on by Mr. Andrews for femoral aneurism on the 4th ult. has continued doing well; the upper ligature came away on the 11th, seven days after the operation, and on the following day the other ligature likewise came away; the tourniquet was applied round the thigh as a measure of precaution, but no hemorrhage has taken place; the circulation appears to be properly carried on; the leg and foot are of the natural warmth and sensibility, and the arteries of the foot can be felt pulsating. The incision has not quite healed; the patient's general health appears to be pretty good.

The man operated on by Mr. Hamilton, for removal of the testicle, has left the hospital with the wound entirely healed.

TO CORRESPONDENTS.

A Correspondent intimates, that there are physicians residing in the neighbourhood of new Broad-street, " who meet, in consultation, persons who are neither surgeons, apothecaries, or chemists, to the manifest injury of the qualified practitioner," and he wishes us to give them " a hint." Perhaps, however, the parties, though coming under neither of the above denominations, are Dubs.

The ignorant individual who wrote the article in the literary journal forwarded by J. F. has only added another proof to the host of evidence, before us, that the occasional medical writers in the weekly literary publications know nothing of their business. They are, for the most part, a shallow set of fellows, unable to perceive the truth themselves, and incapable of displaying it to others. Books are put into their hands to be *praised* not *reviewed*, and he who buys a professional work on the strength of an opinion expressed by a bookseller's scribe, can only expect to be duped in the purchase.

Mr. R. C. King. We doubt whether the cases treated by the " peculiar, cheap, and universally successful remedy," were cases of genuine Asiatic cholera. The author himself avows the opinion.

We shall always be glad to hear from *C. R. B.*, but the great number of practical papers constantly offering themselves to our notice, prevents us from inserting his present communication, which shall be preserved at the office for him.

The letter which we received a short time since, from *Mr. H. B.*, respecting Calais and the saline spa, is, excepting the private portion of it, so completely an advertisement, that we cannot accede to his wishes by giving the account insertion in our pages. We shall be happy to hear from Mr. B. jun.

The letter of *Mr. E. King* was not printed, because the case is so rare of occurrence that there was no chance of its publicity effecting its chief object,—a change in the law.

If *An Apprentice* (London) cannot by his own remonstrances, made in a proper manner, procure attention to his wants, his parents or friends should interfere.

The letter of *Castigator* next week.

Mr. Porter. The particular No. is out of print, but of most others of the volume, copies may be had through the medium of any bookseller. The No. will be reprinted.

Mr. Parkin must favour us with a sight of his MS. before we answer his question.

Errata.—In Mr. Youatt's Lecture of last week, page 545, col. 2. line 46, for *motions partly voluntary and partly involuntary*, read *involuntary motions*.

Page 546, col. 1, line 15, dele *and deglutition*.

Ditto, ditto, line 18, dele *organic*.

Page 547, col. 1, line last but one, for *Both the animal and organic nerves*, read *The animal nerves*.

Erratum in The Lancet Advertisement.—On the cover of the last Lancet, of the reprint of Vols. 9 and 10, last line, for price *one pound*, read *one pound seventeen shillings*.

THE LANCET.

Vol. II.] LONDON, SATURDAY, AUGUST 18. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XL.

THE MEMBRANES OF THE SPINAL MARROW —THE ORIGINS AND DISTRIBUTION OF THE SPINAL NERVES.

Of the compound spinal nerves, of which I spoke in my last lecture, there are thirty-six pairs. I find the first of them at the very commencement of the medulla spinalis; it is the *sub-occipital*, or, more properly, the *first cervical nerve*. It arises in the way that I have already described from the lateral furrow on the superior and inferior surfaces of the spinal chord; the fasciculi from the superior surface converging, and forming a ganglion—the fasciculi from the lower surface converging, uniting with the other beyond the ganglion, and the two trunks entering into one sheath, and forming one inseparable compound nerve.

The Membranes of the Spinal Nerves. The Dura Mater.—There are however some material differences between this compound nerve, and the only one, the fifth, which I have yet examined; and, glancing over the spinal canal, I perceive that these differences are common to all the nerves of the spine. I can trace on the spinal chord the membranes which envelope the brain, but there are some peculiarities about them which deserve my observation.

Within the cranium the dura mater adheres to the bone—it is the internal periosteum; it continues to line the cavity until we arrive at the foramen magnum; it adheres closely to the edge of the foramen magnum, and also to the upper edge of the foramen in the atlas which is the commencement of the spinal canal. That edge, however, being passed, it is no longer the internal periosteum of the bone; the spinal canal has a periosteum of its own, and a very singular and effectual one—a ligamentous as well as membranous one. The dura mater is perfectly detached from it, but continues to envelop and to protect the prolongation of the brain, and in a manner best suited to its altered situation and character. It is still a dense and firm membrane, but it has acquired an elasticity which it had not before.

The Interposed Cellular Substance.—It is still the proper investing membrane—the *theca vertebralis*; but the cavity in which it is placed admits of very great change of direction and caliber, and is composed of a succession of joints, which, during the waking hours of the animal, are in almost continual motion. The spinal chord must not be affected or injured by these changes; therefore, as I have said, the dura mater, now the *theca vertebralis*, is detached from the parietes of the canal, and there is interposed between it, and the bony circle around it, a reticulated membrane, some of the cells of which are filled with adipose matter, and others with serous fluid, and which, combined together, will form the most effectual resistance to any occasional concussion or pressure. From the delicacy of the membrane, so contrived that it shall adapt itself to every motion or change of form in the bony canal, it has a gelatinous appearance; the fluid which it contains, however, has not the slightest viscidity: and this fluid is enclosed in separate cells, that the lesion of some may not endanger the general safety of the chord.

The Inner Membranes of the Chord.—This interposition of membranous and adipose and aqueous matter would seem to afford a tolerably secure protection to the spinal chord; but, to render assurance doubly sure, there is a similar provision within the theca. The *arachnoid coat*, if such it be, is no longer a gossamer secretory membrane floating between the dura and the pia mater, but it too takes on a reticulated, cancellated structure; it is composed of innumerable cells, containing likewise a limpid fluid, and performing the same office of defence; or, rather, completing, and that beyond the reach of common injury, the defence of the spinal chord.

The admirable contrivance of this Interposed Cellular Substance.—Now, Gentlemen, please to observe the effect of this. That portion of the spinal chord which assumes a somewhat vertical direction in the neck is suspended by a thousand elastic springs, and surrounded by a yielding elastic substance, which neutralise or annihilate every possible concussion, and which, in all the extensive changes in the direction of the neck, afford a security against concussion, with which no complication of springs of human contrivance will bear the slightest comparison. You see this yet more evidently in the horizontal dorsal part of the spine; there observe the action of the elastic suspensory chords or springs above, and the downy yielding, yet strong, illacerable cushion beneath; add to this the elasticity gained by the cartilaginous, ligamentous structure and mechanism of the joints of the spine, and you have the most perfect illustration of benevolent contrivance of which you can form any conception. Compare the increase of this cellular interposed substance in the spinal canal of the quadruped in whom the rapidity and extent of the changes in the direction and caliber of this canal, whether in the natural motion of the animal, or the forced exertion which we sometimes brutally extort from him, require additional provision against injury—compare, I say, the relative proportion of this interposed substance in the horse and in the ox, both within and without the theca, and our subject is complete.

The Ligamentum Denticulatum.—This interposed cellular substance assumes a more condensed form at the side of the spinal chord, and is there closely connected with the pia mater within, and, by a line of *tooth-shaped* prolongations, with the dura mater without; hence it is termed the *ligamentum denticulatum*. It is not however any separate ligament, but a condensation of the arachnoid membrane or interposed substance, and which is thus deprived of its cellated structure and fluid contents. I need not point out to you the additional security to the spinal chord which this affords, by limiting the motion of the chord in a way which the extensible strings above and the elastic cushion below could not effect; and by giving sufficient firmness of attachment to prevent laceration of the roots of the nerves in sudden and forcible flexion of the spine. It passes in its condensed form between the superior and inferior root of the spinal nerves, being attached to the pia mater during its whole extent, and to the dura mater in the intervals between the perfect nerves.

The sub-occipital nerve, or *first cervical* nerve, or *first nerve of the spine,* arises, as I have described, at the very commencement of the medulla spinalis, and from the *lateral furrow* on its superior and inferior surfaces; while from this furrow its roots are referable to, and can be traced from, the *central column* above and below. The fasciculi from the superior surface are few, and comparatively large, and they unite and form, or pass through a ganglion; those from the inferior surface are smaller and more numerous;—(they err who speak of any definite number—that varies in different nerves and different subjects;) they are more superficial; more easily lacerable; they converge, unite, join the other branch beyond the ganglion, and then, escaping from the dura mater, we have the perfect compound nerve of sensation and motion. Having pierced the dura mater, it emerges from the spinal cavity through a foramen in the anterior part of the atlas; and no sooner is its escape accomplished, than it divides into two main trunks, which subdivide and ramify in every direction, and give the power of motion to, and bestow sensation on, the neighbouring muscles and parts.

The Anastomoses of the Nerves.—This is not the time to trace minutely the course of these, or of any of the spinal nerves. I shall have more satisfactorily to speak of them when we describe the various muscles and portions of the frame, and the nervous influence with which each is endowed. All that I shall attempt at present is a mere sketch of their origins, connexions, and ultimate destination; and for minuter information I would refer you to that admirable division of my friend Mr. Percivall's "Series of Elementary Lectures on the Veterinary Art," in which he describes the course of the spinal nerves. I shall scarcely dare to deviate from his very accurate description of them.

I would beg your attention to the evident fact of the compound nerves of the spine dividing into innumerable ramifications, and, more particularly, anastomosing with branches from the neighbouring nerves. This sub-occipital nerve has direct communication with the second cervical, the lingual, the recurrent, the glosso-pharyngeal, and the great organic nerves, and indirectly with the fifth, the eighth (portio dura), and almost all the cervicals. In the second cervical the direct and the indirect anastomoses increase, and they increase as we proceed down the frame, and not only between considerable branches of the nerves, but the minutest ramifications of them, and not merely a single anastomosis, but numerous ones between any two given nerves. They meet—they separate—they reunite, they admit others into association—they coalesce with every neighbouring one—they extend the bond of union to distant nerves—to the most distant; in fact, the whole system is united together. We cannot unravel the entangled and matted fibres, but we have a clue to the nature and the design, and I would almost say, the simple arrangement of the whole.

The Sensitive Fibrils.—The nerves of sensation ramify into branches of extreme minuteness. It was necessary that they should do so—that they should spread themselves everywhere, in order that there should not be the minutest part of the frame in which they did not give warning of danger or injury. Their anastomoses probably are few; for it was necessary that the impression should be referable to the precise spot on which it was made, nor would it be consistent with the enjoyment of the animal, that any painful feeling should spread around, unless when the injury is extending and needs instant remedy. Yet we can conceive of minute fibrils of inter-communication between the sensitive nerves, in order to keep up a common feeling and sympathy of parts, and on which, in truth, not only the well-being of the system, but the proper action of every part depends.

The Motor Fibrils.—I can form a clearer conception of the anastomoses of the motor nerves. There is scarcely an action of the simplest nature that can be performed by the power of a single muscle. Many muscles must work together, and many nerves combine their influence, in order to its accomplishment. For the more perfect performance of this—to secure the consentaneous action of these muscles, the fibrils of the nerves by which they are supplied are brought together, and have inter-communication with each other. A certain nerve is not only given to a certain muscle, and there results only one contraction or action of that muscle, but the nerve spreads over the muscle, and enters into, and is diffused over, the substance of it, and, according to the part or division of the muscle on which its principal or only influence is bestowed, the action is evidently and considerably varied. I can conceive, therefore, the necessity of the anastomoses of minute branches, in order that by the co-operation of parts of various muscles, many complicated motions may be effected. He who has a perfect knowledge of the anatomy and function of the muscles, could without much difficulty trace out any seemingly complicated arrangement of nervous fibrils. But we shall have other nerves by-and-by with which to associate those of sensation and voluntary motion, and which will enable us more satisfactorily to account for the intricacy of their course.

Comparative Survey of the Cervical Nerves.—I return to the nerves of the neck, and I find no material difference in any of our patients, except that the three first nerves are proportionally larger in the dog than in the horse, because they have comparatively bulkier and more powerful muscles to supply, and which are so placed as to act with greater mechanical disadvantage than in the horse. In the ox they are rather less developed than in the horse, for the neck is shorter, although the muscles are large. The number of pairs, namely eight, is the same in all of them, considering the suboccipital as the first. In our occasional patients—the domestic poultry—the number is greater, and varies in the different species; the common fowl has 14 cervical nerves, the duck 15, and the goose 16. The parrot, respecting which we are sometimes consulted, has 12, and so has the lark, the goldfinch, and the canary, and most of our imprisoned warblers; and in all of them the nerves are proportionally larger than in quadrupeds.

The Nerves of the Fore Extremities.—The sixth, seventh, and eighth cervical nerves, and the first dorsal, not only supply the muscles in their respective neighbourhoods, but a small branch of the sixth, and the greater portion of the three others, go to form a singularly complicated plexus of nerves, whence proceed branches to the external thoracic muscles, the scapula, and the whole of the fore extremity. The *external thoracic* nerve in the horse supplies the muscles of the breast and side, and even goes to the serratus and the latissimus dorsi, and the panniculus carnosus. The *scapular* ramifies on the muscles on the outer and inner side of the scapula, giving branches to the triceps, the teres minor, and the levator humeri. The *external cutaneous* nerve of the human being is not found, but the *spiral*, or rather a branch from it, and not from the *ulnar*, as asserted by Cuvier, supplies the place of it; while the *spiral*, and the *radial*, bestow nervous influence on the upper part of the extremity, and the *ulnar* on the whole of it, and with no other difference than occurs from the one phalanx of the horse, the two which constitute the foot of the ox, and the four that are found in that of the dog.

The principle of Neurotomy.—In the metacarpal and the plantar nerves of the horse, the ox, and the sheep, we have an interesting illustration of the distribution of the filaments of different function, of which the compound spinal nerve is composed. There are no muscles below the knee in these animals, and as it seems to be a law of nature that nothing is given which is not required, we imagine that the motor fibrils cease to exist below the knee, for there they would be useless; and accordingly, in the operation of neurotomy, we destroy the sensibility of the foot, and relieve a noble animal from that torture which in various diseases of the foot our own ill management or barbarous usage had inflicted upon him; but we take away no muscular power, and no otherwise interfere with the action of the limb, than by giving some incertitude of motion and increase of concussion, which plainly result from the loss of sensibility in the part.

The Nerves of the Wing in Birds.—These spring from the last cervical, and the two first dorsal nerves. The plexus is more regular and compact than in the quadruped. The pectorals, as may be imagined when we observe the muscles of the breast in the bird, are of great comparative bulk, and there are four of them. Two nervous chords only are sent to the wing; the first, taking its course under the inferior surface of the wing, answers to the *spiral* and the *ulnar* nerves; the other, turning round the humerus to reach the superior surface, is the *radial* nerve.

The Dorsal Nerves.—We have observed the spinal chord enlarging at the inferior part of the neck, that the nerves which are to form the humeral plexus may be sufficiently developed. It now rapidly diminishes, and becomes little more than a round chord; but still a succession of nerves from above and below are given out from it, opposite to each of the articulations of the spine, yet with this singular circumstance, not existing however to so great an extent in the brute as in the human being, that while the origins of the cervical nerves are opposite to the foramina through which they escape, the posterior dorsal and the lumbar nerves spring from portions of the spinal chord anterior to these foramina, and increasingly so, and therefore they run a certain way along the side of the spinal chord, before they escape from the canal. Do they thus receive any minute filaments or influence from the lateral columns of the spinal chord? This is worth observation and inquiry.

The Intercostals.—There are eighteen pairs of dorsal nerves. They are smaller than the cervical nerves, and present fewer anastomoses, and even fewer ramifications; for there is less muscular substance to supply, and far less muscular action. Each dorsal nerve, as it escapes, devides into two branches. The inferior one, after giving a branch to the panniculus carnosus, and to the integument, shelters itself with the blood-vessels in a sulcus under the posterior edge of the rib behind which it escapes from the spinal canal; as it passes it gives minute ramifications to the muscle between that and the next rib, and then, in the anterior ribs, expands itself on the muscles of the sternum, while the nerves of the posterior ribs, after supplying the intercostal muscles, send their ultimate fibrils to the internal oblique and recti-abdominal muscles. These are plainly respiratory nerves, and we shall have reason to suspect that they proceed from, although they have not been actually traced to, the lateral column of the spinal chord. At all events, each intercostal nerve anastomoses with the great organic, and forms with it a ganglion.

The superior branch of the nerve turns upwards, becomes deeply buried in the muscles of the back, and gives filaments to the serrati, the rhomboides, the trapesius, the latissimus dorsi, the quadratus, the psoas magnus, the iliacus internus, the superior abdominal muscles, and, in fact, all the muscles of the back and loins.

Comparative View of the Dorsal Nerves.—The only important difference in the dorsal nerves of other domesticated animals is in their number, and that corresponding with the number of the ribs. In the ox, the sheep, and the dog, there are but thirteen; there are fourteen in the hog, and in the bird they seldom exceed seven or eight.

The Lumbar Nerves.—There are five lumbar vertebræ, and a corresponding number of nerves. The spinal chord is now beginning to enlarge. About the third lumbar vertebræ the increase of size is very evident. These nerves, as soon as they escape from the canal, divide also into a superior and inferior branch; the first going to the muscles of the loins, while the inferior one, as I shall have to show you when describing the muscles, is of great consequence. The inferior branch of the first lumbar nerve takes a long course over the transverse processes of the two next vertebræ to the iliacus, and along the muscles of the thigh, almost to the stifle. It has also, in common with every nerve of the spine, communications with the nerves next before and behind, and with the great organic nerve.

The second lumbar nerve is of more importance. It not only gives ramifications to the psoas and the iliacus, and extending even to the stifle, becomes the principal subcutaneous nerve of the thigh, but, passing through the abdominal ring, it dispenses nervous influence to the scrotum and testicle of the male, and the uterus, the pudenda, and the udder of the female. It even does more than this, it helps to form one of the nerves of the hind extremity. Of these, then, I must next speak, but it will be a hasty glance indeed that I can now take of them.

The Nerves of the Hind Extremity.—These do not arise from one common nervous plexus, as in the anterior extremity, the branches that issue from which it is difficult or impossible to trace to their true nervous origin; but the formation of the nervous chords can be satisfactorily followed to distinct plexuses, referable to particular nerves.

The second lumbar nerve contributes a branch, and the third and fourth the greater part of their substance; and the fifth a considerable branch, to the formation of the *crural* nerve. Its principal distribution is to the rectus, and to the internal and external vastus; while no mean branches go to the iliacus, and the psoas magnus, and the pectineas, and then becoming a subcutaneous nerve, it ramifies on the interior and forepart of the thigh and the leg, and may be traced as low as the fetlock. It accompanies the vena saphena in the greater part of its course, and should be remembered when we are bleeding from that vein.

The third lumbar nerve sends a branch to form the *obturator*; the fourth more largely contributes, and a smaller branch is derived from the fifth. This nerve, after a short course, passes through the foramen magnum ischii, gives branches to the obturator muscle, to the pectineus, the triceps, and the gracilis.

The fifth lumbar nerve detaches a branch to help to form the *sciatic* nerve; it receives a much larger one from the first sacral, and others from the second and third. This nerve quits the pelvis through the sacro-sciatic ligament, passes between the hip-joint and the tuberosity of the ischium, enters deeply into the haunch, and having given branches to the biceps, the semitendinosus and the semimembranosus, divides into the three *popliteals*. The first of these answers to the ulnar nerve of the fore extremity; the second feeds the gastrocnemii, the flexors of the foot, and reaches to the stifle-joint; and the third, after sending branches to the semimembranosus, the gastrocnemius externus, the flexor metatarsi, and the extensor pedis, pursues its way down the leg, and becomes at length a subcutaneous nerve, terminating on the pastern.

The fifth lumbar nerve sends a considerable branch to help to form the *gluteal*; but the main substance of it is derived from the first sacral. It is expended on the glutæus maximus.

I have no material difference to point out in the distribution or function of the lumbar nerves in our other domesticated quadrupeds.

The Sacral Nerves.—Besides contributing to form these nerves of the hind extremity, the sacral nerves are of no little importance. The *second* ramifies posteriorly among the muscles of the tail; the *third* gives branches to the muscles of the haunch; the *fourth* supplies the bladder, and rectum, and perinæum; and the *fifth* belongs to the muscles of the tail. All these are composed of branches of the inferior divisions of the sacral nerves, and which likewise communicate with the sympathetic nerve, and with those before and behind. The superior divisions, as in the dorsal and lumbar nerves, supply the dense mass of muscle above, or penetrate through it and become subcutaneous nerves.

The Cauda Equina.—The spinal chord had begun rapidly to diminish at the last lumbar vertebra; and at the second of the sacral bones it may be said to end. We can trace a little pyramidical point in the centre which is its apparent termination. This bulb, however, is surrounded by numerous nervous chords, forming at first a bundle of prolongations or large filaments more bulky than the chord from which they sprung, but gradually diminishing in size and in number, and ceasing altogether about the fourth caudal vertebra. The three last sacral nerves are formed from these chords, which like those from the spinal marrow itself are perfect nerves of sensation and voluntary motion. The sacral nerves, however, issue by two distinct foramina from the spinal canal; the superior penetrate at once into the muscles above; the inferior escape into the pelvis; but they are not nerves of different character or function, for after the motor portion had joined the sensitive and the compound nerve was formed, the first division took place *within the canal*, and both branches as they issued were true compound nerves.

The Coccygeal or Caudal Nerves.—There are four other pairs of nerves proceeding from these chords, and which escape from the four first vertebræ of the tail. They are also compound nerves of sensation and voluntary motion; they anastomose with each other, and keep up the chain of nervous intercommunication; but they have no connexion with the great organic which has here ceased to exist. They likewise divide into a superior and inferior branch; the superior connected with the erector, and the inferior with the depressor muscles of the tail, and both of them being also subcutaneous nerves.

And so ends our anatomical description of the spinal marrow, brief, dry, and somewhat unsatisfactory; but we should hereafter have had much repetition, if I had at present more fully traced the course of the different nerves. We are now prepared to return to the commencement of the spinal chord, and enter into the consideration of its lateral column, but this deserves a separate lecture.

FORENSIC CHEMISTRY.

ON THE DETECTION OF MIXED POISONS.*

By M. Orfila, *Professor of Chemistry, Paris.*

For some years past the various scientific journals have contained instances of poisoning occasioned by mixtures of arsenic and laudanum, proto-nitrate of mercury, and verdigris; but as far as I am aware, no particular attention has been paid to the chemical questions involved in these discussions. I have deemed it my duty to study this subject with the greater care, inasmuch as it offered to my consideration many remarkable and unexpected phenomena. Several times, poisons, generally thought to exercise no decomposing action with others, have been so powerfully influenced by the mixture, that the ordinary re-agents failed in the detection of the individual substances, and only denoted the existence of the compounds which had been formed. I have indeed no hesitation in stating, that the most experienced operator would in these cases commit the most serious errors, did he not possess the data which form the basis of this paper. He might, for example, conclude from a given number of experiments, that an individual had been poisoned by a mixture of arsenicous acid and calomel, or metallic mercury, when the poisoning was the effect of corrosive sublimate and arsenicous acid, or he might suppose that a mixture of the peroxide of antimony, and proto-tartrate and proto-chloride of mercury had been employed, when in reality corrosive sublimate and tartar emetic had been given. These examples suffice to prove the importance of the essay I have undertaken.

I will, therefore, in the first place, point out the effects of the principal reagents on *mixtures* of the most important mineral poisons, in such case using concentrated solutions in varying proportions. I shall then consider the means of separating the several poisons composing the mixture; and where this cannot be accomplished, I shall at least be able to show the processes by which the nature and proportions of the metals and metallic oxides may be ascertained. My experiments have all been performed with perfectly *pure* reagents, and I allude to this the more pointedly, because on some occasions having employed reagents of *reputed* purity, I did not invariably obtain the results I have described. I also find it necessary to observe, that in the analytic departments of the experiments, I principally had in view the detection of the *nature* of the poisons, not devoting so much attention to the rigid determination of their proportions. For the latter purpose indeed, various modifications would be necessary in the processes I describe, modifications too which none but the most experienced could successfully practise.

MIXTURES OF CORROSIVE SUBLIMATE AND ARSENICOUS ACID.

Concentrated solutions of three parts of corrosive sublimate and three of arsenicous acid.—In this mixture *sulphuretted hydrogen* causes a dirty yellow precipitate with some black specks, being a mixture of the sulphurets of arsenic and mercury. *Sulphate of copper* precipitates the solution in greenish yellow, being a mixture of green arsenite of copper, and of the white precipitate occasioned by the excess of ammonia in the test in solutions of corrosive sublimate. The *nitrate of silver*, if acid, produces a white precipitate of chloride of silver, but if a little ammonia is added, the deposit becomes slightly yellow from the formation of arsenite of silver. *Caustic potash* furnishes a white precipitate which becomes black when an excess of alkali is added, while from corrosive sublimate alone a yellow would have been thrown down, and from arsenicous acid no precipitate whatever. The black deposit is metallic mercury and its black protoxide. From this it follows, that the arsenicous acid is changed into arsenic acid, while the corrosive sublimate is first changed into calomel, and then into metallic mercury. The following experiments prove this position.

If after washing the white precipitate it is allowed to dry on a filter, it will be seen to assume a pale yellowish colour. Treated by cold nitric acid, part is dissolved (the arseniate of the protoxide of mercury), and the solution is precipitated black by potassa. The undissolved portion washed, dried, and heated in a glass tube, sublimes in the manner of calomel. If the liquor be examined in which the calomel and metallic mercury were originally formed, it will be found highly alkaline. If this be saturated after filtration, by pure muriatic acid, then evaporated to dryness and the product redissolved in water, a solution will be obtained from which *sulphate of copper* will throw down a *blue* arseniate of copper and *nitrate of silver*, a white precipitate of chloride of silver, mixed with red arseniate of silver. It follows, therefore, that potash occasions in this mixed solution of arsenicous acid and corrosive sublimate this transformation of part of the arsenicous into arsenic acid.

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* Annales d'Hygeiene publique, &c.

Ammonia causes, with this mixed solution, a white precipitate. A *slip of copper* and the minute *galvanic pile*, composed of a slip of gold, and a leaf of tin rolled into a spiral, produce with the mixture the same effect as with pure corrosive sublimate.

Concentrated Solutions.—Three volumes of Sublimate and one of Arsenicous Acid.

Sulphuretted hydrogen furnishes a very dirty yellow precipitate, in the midst of which is almost instantaneously deposited some black sulphuret of mercury; *ammoniacal sulphate of copper*, a greenish yellow; *neutral nitrate of silver*, a white, very slightly yellow; *ammonia* a white ditto, soluble in excess of alkali; *copper* and *electric pile* act as with pure corrosive sublimate.

Concentrated Solutions of three vols. Arsenicous Acid and one of Corrosive Sublimate.

Sulphuretted hydrogen, a yellow precipitate speckled with black dots. *Ammoniacal sulphate of copper and nitrate of silver*, as before described, except that the green and yellow shades are more decided. *Potassa* acts as with solutions of equal parts of sublimate and arsenicous acid. *Ammonia* gives a white precipitate, soluble in excess of alkali. Copper and the electric circle act as on sublimate alone.

Analysis.—In the fifth volume of the "Archives Générales de Médecine," a case is recorded by M. Juliè Fontanelle, in which an apothecary's apprentice swallowed a drachm of corrosive sublimate, mixed with three of arsenicous acid. Sublimate will be easily separated from arsenic by treating the powder by cold sulphuric ether, and agitating the mixture from time to time in a phial with a ground stopper. The sublimate alone is dissolved. The same remarks apply to the aqueous solution as to the powder.

(To be continued.)

FURTHER RESULTS OF

DR. AYRE'S TREATMENT OF MALIGNANT CHOLERA

BY SMALL AND REPEATED DOSES OF

CALOMEL.

To the Editor of The Lancet.

Sir,—Having been applied to by several correspondents for information as to my further experience in the treatment of cholera by small doses of calomel, frequently repeated, and the results of it, I beg leave, in anticipation of such inquiries, to give through your journal a summary account of both.

Since the date of my last communication of the 28th of June, and published in your journal of the 14th ult, I have had ninety-nine additional cases, making the whole number 175. Of the first series of 76 cases, ten died; of the last series of 99 cases, 20 have died, making the whole number of deaths thirty. Of the 20 cases which terminated fatally, it is right to add, that seven of the patients were dying when I saw them, and for whom therefore I did not prescribe; and an eighth patient was lost by the nurse getting (drunk?)* and wholly neglecting to give the medicines. The twelve fatal cases which I did prescribe for, were all subjected to circumstances which might have been obviated, and which, as originating in the prejudices or poverty of the patients or their friends, would be guarded against by persons in a better class of life. And it is necessary to notice these circumstances; for in estimating the merit of any given mode of treatment, they must be taken into account; for other things being equal, treatment that proves beneficial, or appears to do so, to one patient in this disease, should be so in another, and if it be otherwise, it must be owing either to something essentially wrong in that treatment, or to causes which are foreign to it and control it.

In my former letter already referred to, I noticed the causes which influenced the issue of the ten fatal cases, and I shall now do the same with respect to those which occasioned in the twelve the same result.

Of these patients, the first two were a man and his wife. The man, who was about fifty years of age, and was first attacked, had long been of drunken habits, and latterly very destitute. He was pulseless when first seen, but recovered from this state, and struggled on to the sixth day, when he died. His wife, who watched him through several nights, became exhausted by fatigue, and grief, and want, and being seized by the disease, was taken to a hospital (but through mistake to a wrong one), and, losing sight of her for several hours, I found her in a state of irretrievable collapse.

The third case was that of a poor woman, residing, at the time, alone, who, having been seized with the disease in the night, kept her door locked, that her neighbours might not gain access to her and discover her state, as she was reluctant to have any advice. She was pulseless and nearly dying when I saw her.

The fourth case was a very dissolute woman, who was drunk and fighting on the evening preceding the night of her attack, and was twelve hours in collapse before I saw her.

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* Our correspondent has here left out a word.—Ed. L.

The fifth and sixth cases were aged men, who were seized early in the night, and for whom no assistance was sought until the morning.

The seventh patient was a young woman who, after recovering from a most severe attack of the disease, relapsed into it from eating too hearty a supper at the house of a neighbour, and who sank at the end of a fortnight, from exhaustion caused by a miscarriage, which the second attack of the disease had induced.

The eighth case was a watchman, whom I was requested by Mr. Sharpe to see with him. He was brought out of the collapse, and appeared during several days to be recovering, when, unknown to us, he had ginger beer and other improper things given to him, and he died from an inflammation in the mucous lining of the first passages.

The ninth case was a young married woman, who, after exerting herself with great humanity and activity in her gratuitous aid to the very numerous sick of her neighbourhood, became herself the subject of the disease, and at length the victim of it. I saw her early in it, and she had become convalescent; when, being hungry, and having no other food, she ate the half of a flour-and-water dumpling, relapsed in the night, and in the morning when I was called to her, she was pulseless and dying.

The tenth case was a poor stranger, who had been many days almost without food, and who worked through a whole day in a hay-field with the disease upon him, and was brought home in the evening pulseless.

The eleventh case was a child three years of age, who had nearly recovered after being in a state of extreme collapse, when all her attendants became ill, and incapable of duly attending to her, when an affection of the head came on, which speedily terminated in effusion.

The twelfth case was the grandfather of the above, who, with his wife and daughter, became affected with the premonitory symptoms, and who, from my omitting to join calomel with the other preventive means, fell into the true disease in the night, and in the morning at my visit he was dying. The wife and daughter by the calomel treatment were secured from its further progress, or perhaps, to speak more correctly, they were relieved from a mild and incipient attack, which indeed was the case with a great number whom I prescribed for, and whom I did not report to the Board of Health.

Such, then, are the cases which I have lost, and which differ in nothing from a very large proportion of those which have recovered under the treatment I recommend, excepting in the extrinsic circumstances which I have detailed. All the patients had passed into the stage of collapse, and many had been in it several hours before I was called to attend them. A few only had passed through it into the consecutive stage, and these were of the milder form, and chiefly of late occurrence, and in whom the cramps were principally confined to the abdominal muscles, and the dejections tarlike, with pain on pressure in the *epigastrium*, or about the head, with the other symptoms of that state.

In regard to the treatment, the same methods, precisely, have been pursued in the last series of cases as were described and recommended in the treatment of the former ones. The calomel has been given nearly uniformly in single grain doses, made into exceedingly minute pills, with bread crumb rubbed down into a mucilage, and mixed with a little gum, and given with one drop of laudanum in a teaspoonful of water every five or seven minutes, for one, two, or three hours, or longer, as the urgency of the case demanded, widening the intervals of giving them as the urgent symptoms subsided, and discontinuing the laudanum. No other limit was found necessary to be set to the quantity of calomel given, than that which the circumstances of the disease imposed, nor was any fear entertained that the medicine would affect the mouth; for pending the duration of the collapse, no absorption of the medicine will take place. By one patient, a young woman (Rosanna Curry), whom I had a few days ago in the hospital, and who had been, some time before I saw her, in the stage of collapse in its most malignant form, no less a number than 300 grains of calomel were taken, and two-thirds of them retained; and who, notwithstanding the extreme severity and obstinacy of her disease, and the quantity of calomel taken, was well, and discharged from the hospital, in less than ten days, and without any material soreness of the mouth.

In my former letters I dwelt upon the necessity of resorting early to the antiphlogistic treatment upon the passing away of the stage of collapse, grounding my belief of its importance on the theoretical view I took of the nature of the disease. My experience since, in the disease itself, has fully confirmed the view I took of it. Early leeching of the *epigastrium* on the first rise of the pulse beyond it's natural standard, is always to be effected, if possible, and leeches should always be left in the evening with the attendants, to be applied in the night, if necessary, that no time may be needlessly lost. Of not less importance is care in the choice of the food for the patient. Rice appears most suitable with thin broth, and bread and flour, meat, and all acids, most injurious; wine is rarely required. The patients often desire to have it, and in the early cases of the disease I granted it to relieve their weakness after they appeared to have got through it, but I am now convinced that it was injurious to many, and I doubt if it was beneficial to any; and latterly I have never allowed it.

In conclusion I have only to add, for the information of those who may hereafter have to treat the disease (for those who have seen it cannot want the information), that in the treatment of the disease, and to render that treatment efficient, it is necessary to submit to the toil of almost hourly visits to the patients; for such is the imminent character of the disease, and the extraordinary rapidity of its course, that the slightest intermission of the treatment may prove an irretrievable loss to the patient; and indeed his imminent danger is akin to the condition of a drowning man, and the obligation of the practitioner is scarcely less than that man who holds within his hand the means that are to rescue him. I am respectfully yours, &c.,

JOSEPH AYRE.

Hull, Aug. 11, 1832.

CASE OF

TIC DOLOUREUX.

By W. DOBSON, *Esq., Surgeon, Pimlico.*

MR. P——, ætat. 65, a tall plethoric man, had enjoyed uninterrupted good health until about nine years ago, when he was attacked by tic doloureux in the right side of the face. He attributed the accession of this malady to adverse domestic circumstances. The affection has progressively increased, both in the intensity of the paroxysms and the frequency of their occurrence. Numerous remedies had been employed, and various plans of treatment adopted, but all equally unsuccessful.

All the pathognomonic symptoms existed, and as violent as imagination could conceive them. The ordinary excitants were, eating, drinking, speaking, and touching the parts. The pain resided in both maxillary nerves, but especially in the superior, the whole cheek exceedingly sensible when touched, particularly around the emergence of the superior maxillary nerve, from the foramen infra orbitarium. During the most urgent paroxysm no discoloration of the integuments was perceptible. The muscles of that side were flaccid, and the cheek more prominent than the other. Restless nights from pain; intense mental anxiety; torpid bowels, defective appetite, and all the phenomena exhibitive of severe alimentary disturbance. After the intestinal canal had been regulated by aperients, carbonas ferri, in doses of from ʒij to ʒvj, were exhibited three or four times a day, and hydrarg. submur. gr. ij sing. noct., until the mouth became slightly affected. Still, after the elapse of some weeks no relief was obtained. Various remedies were subsequently used, ex. gr. quinine, carb. ammoniæ, camphor, opium, hyosciamus, ext. humuli, and ext. belladonna, applied extensively to the pained parts, in short, every antispasmodic and narcotic medicine was prescribed, without either removing the disease or alleviating its severity. Liq. arsenicalis was now commenced with, and with a full determination to give it a fair trial, by pushing it as far as the system would permit, beginning with the dose ♏v, and increasing it to ♏xxxv ter die, at the expiration of which time decided benefit was obtained; the paroxysms were less frequent, of shorter duration, and less violent; but the medicine was obliged to be discontinued in consequence of the accession of gastric irritation—muscular tremors and twitchings. Gentle aperients were in the mean time administered. In a few days the arsenical solution was resumed, commencing with only ♏x ter die, and gradually increased. In six weeks from the commencement of giving this medicine every symptom decidedly mitigated, and in two months had entirely vanished. It is now three months since the drawing up of this case, and as there has been no return of the disease, we may presume that it is eradicated, and by the agency of *arsenic*.

August 6th, 1832.

EFFICACY OF THE PLUG IN

UTERINE HEMORRHAGE.

To the Editor of THE LANCET.

SIR,—July 2nd, I was called to see Mrs. Merser at Queenborough, who was at her full time of pregnancy, and expecting every day to be confined, when I found her labouring under hemorrhage to an inordinate degree, which had been the case for four days antecedent. She has had four children before, with no extraordinary circumstance attending the delivery of them; but on this occasion received news which greatly alarmed her, and caused her to jump off a place some few feet from the ground, when something tore from her side, according to her description. Upon examination per vaginam, I found the orificium uteri dilated to the size of half-a-crown: no pains, but a protrusion of a detached portion of the placenta. The hemorrhage being excessive, and uterine action not having come on, I determined on the application of the plug, and accordingly disposed of one between the detached portion of the placenta and the head of the fœtus, enjoining strict quietude, and cooling diluents. I consider in such cases, that it is better to suppress, if possible, the hemorrhage, to allow the exhausted frame to recover itself, than to endeavour to bring labour on, either by manual operation or medicinal means. Circumstances obliged me to leave her, and I was called again in about six hours. On my arrival I found her suffering under an inability of passing water, which caused pain, and led her to suppose that labour was commencing. Conceiving that the pressure of the plug was the cause, I removed it, when she made water freely, and all unpleasant symptoms being, consequently, removed, I found things on examination just "statu quo," saving that the hemorrhage had entirely ceased. I then left her once more, and four hours after, my attendance was again requested. She had had no return of hemorrhage until about an hour before I arrived, when it came on to a most alarming extent. I found more protrusion of the placenta; the head of the fœtus descending into the pelvis; slight labour pains, and the membranes exceedingly tough. From her exhausted state, consequent upon previous loss of blood, I apprehended serious results, and finding no time to be lost, immediately lacerated the membranes, when the labour came on more satisfactorily; but the detached portion of the placenta became a material obstruction to the progress of the labour; I therefore resolved to endeavour to force the placenta back into the uterus, which happily was effected, but exhaustion and hemorrhage increasing, I found it necessary to keep her up by the administration of gruel with brandy, and sent off for the assistance of Dr. Jardine. However, on the return of the placenta, labour came on rapidly, and nature effected her purpose before his arrival. Now the hemorrhage was suspended for nine hours by the application of the plug, but for which, in my opinion, she must have inevitably perished.

I am, Sir,
Your obedient servant,
J. R. Hancorn, Surgeon.
Sheerness, August, 1832.

EMPLOYMENT OF THE
NITRATE OF SILVER
IN
VAGINAL DISCHARGES.

To the Editor of The Lancet.

Sir,—Having perused with much satisfaction the clinical remarks of Dr. Elliotson in the last week's Lancet, on the use of the nitrate of silver in a case of inflammatory leucorrhœa, I am induced to solicit the favour of a small space in your journal of next week for a few brief observations on the same subject. In the preface to my work on leucorrhœa, which has now been before the profession nearly two years, I there stated, that "whether the mode of practice I have slowly and cautiously adopted, and which is a novel one, at least in this country, will prove so successful in other hands as it has in mine, time and experience alone can determine." It is, therefore, with no little degree of pleasure that I find the practice not only adopted, but, after repeated trials, approved and recommended by so distinguished a member of the profession as Dr. Elliotson. It must be admitted, that in the cure of disease, and before any remedy can be employed upon anything like sound principles, a strict pathological inquiry is of the highest importance, but probably in no other disease is it more urgently demanded than in the different varieties of vaginal discharge. The pathological view which I have taken of leucorrhœa is, that for the most part it is a disease of increased action, and not one of debility and local relaxation, as is too generally believed; and I find my opinion materially strengthened by the judicious remarks of the gentleman alluded to, and the successful result of the remedies employed. In the use of the nitrate of silver in solution, a decomposition necessarily takes place when the pewter syringe is employed. To obviate this defect, often complained of, and knowing that the common straight instrument is a very inefficient one, a bone syringe has been made by my directions, and can be procured at any of the surgeons' instrument makers, somewhat upon the principle of the one commonly designated Clarke's syringe, which answers the purpose exceedingly well. I have seldom employed a solution so weak as that recommended by Dr. Elliotson, although I quite agree in the propriety of commencing its use with caution; at the same time I have scarcely ever found a strong solution to occasion much uneasiness or pain, certainly not more than is occasionally experienced in the use of the common astringents. Before I conclude, permit me to remark, that having been nearly seven years a public teacher in this metropolis, the author of the work alluded to, &c., a little more courtesy might have been expected from a gentleman so justly eminent as Dr. Elliotson, upon the introduction of the subject to the notice of his clinical class.

I have the honour to remain, Sir,
Your obedient servant,
GEO. JEWEL.
Sackville Street, Aug. 14, 1832.

VACCINATION.

To the Editor of THE LANCET.

SIR,—The only remark I have to make at present regarding vaccination since the publication of the preceding ones already detailed in the former Numbers of THE LANCET, is with regard to the *thinness* and *thickness* of the skin of the arms of the children vaccinated; and the care to be taken by the operator in inserting the virus.

During the numerous vaccinations carried on by me for years past in the Public Vaccinating Establishment of Scotland, in making incisions into the arms of the children for the purpose of introducing the vaccine virus with the round-pointed vaccinating lancet, which I invariably make use of (as formerly mentioned), I have at times found the skin as thin as gauze paper or gold-beater's leaf, and turgid with circulating blood, which I must remark is rare, but occasionally presents itself; and drops of blood, following each other in succession, wash out the inserted virus, rendering the inoculation ineffectual.

In other infants, on the contrary, of the same age, and under the same circumstances, I have found the skin of the arm *thick*, like a piece of parchment, and not the slightest appearance of blood to be observed, until the lancet, by repeated scratches, had penetrated to a considerable depth in introducing the virus. Betwixt these two extremes, I have met with every gradation.

In consequence of possessing a knowledge of the above facts, I conclude that the vaccinator should always be on his guard whilst inserting the virus, lest the skin should prove *thin*, as already mentioned, a circumstance which no individual can tell beforehand, and if so, blood will follow upon the slightest scratch of the lancet, washing away the virus, rendering the future vaccination useless, running down the arm, proving discreditable to himself, and hurtful to the feelings of the mother. Should the skin prove thick, as already alluded to, I consider it as an advantage allowing of the free and slow absorption of the vaccine lymph, and proving of no consequence in any other point of view. I am, Sir,

WM. HOWISON, M.D.
Vacc. to the Royal Disp. Edinb.
9, Nicolson Square, July 19, 1832.

PROPOSAL TO USE THE

SERUM EXTRACTED FROM COW'S MILK,

INSTEAD OF THE SALINE SOLUTION,

For the Purpose of Injecting the Veins of Patients in the last Stage of

MALIGNANT CHOLERA.

To the Editor of THE LANCET.

SIR,—There cannot be any doubt now, that cholera originates from inhaling a certain aërial poison during respiration; that it manifests itself by a specific influence on the function of secretion, both in the stomach and bowels, accompanied by other symptoms of less importance; that the fluid secreted is sufficiently copious to drain in a few hours so much, from the vascular system, of the serous part of the blood, as to impair, and at length destroy, the means of circulation altogether. In cases of cholera asphyxia, death appears to be the immediate result of the influence of the poison imbibed; but in the more usual form of cholera, such as occurs in England, death cannot be considered the immediate effect of the poison, but of the deficiency of the fluid part of the blood; it has therefore been proposed to inject a weak solution of salts, in order to supply this defect; however, this fluid, although, according to chemical analysis, it is the same as the serum of the blood, is evidently such a crude imitation of a vital fluid, that it is wonderful that it should have proved successful in any case; but since it has, and in the last stage of the disease too, when no means are admitted to avail as a remedy even by the most experienced in India, it certainly becomes the duty of every one to resort to the only plan which offers any reasonable chance of success. In fact, in a stage of the complaint where there is all but a bloodless state of the vessels, and a pulseless circulation, what other indication is left than to restore the circulation if it be possible? This has been done, and repeatedly. Yet, because it has more often failed than succeeded, it is falling into disuse, as if it were to be expected that an operation of this kind should be always successful on the first attempts of different practitioners, and with instruments not exactly suited for the purpose. Maw's apparatus is by far the easiest to use, and requires two persons only, while the others require three.

The object of this communication is to propose the use of serum instead of the saline solution. The superiority of this substitute is too apparent to need comment. The best mode of procuring it is from cow's milk, which can be readily obtained in any quantity, and may be deprived of its caseous substance in the following manner. To every quart of milk made hot, add eight grains of rennet (the gastric juice of the calf); as soon as the curd ceases to be separated, it may be strained and brought to the proper temperature for use; if it should be thought necessary, this might be further cleared of a small proportion of caseous matter yet remaining in solution, by adding to it the white of an egg beat up in a wine-glassful of the serum; or a few grains of tartaric acid, then boiling and straining it; however, this latter process would probably not be necessary.

I have had but one opportunity of trying the injection into the veins, which was performed under unfavourable circumstances, being hurried as to time; the milk was merely boiled once, skimmed, and strained, which deprived it of but a very trivial portion of its caseous substance, and not sufficient caution was observed to prevent the injection of air with the milk. The effects were, at the time, a sensation of nausea during a few minutes only, with this slight exception, the patient was tranquil until her decease: the profuse cold perspiration ceased, and the skin became warm, and remained so; the pulse became apparent at the wrist. She died between two and three hours afterwards, and, upon examination after the lapse of fourteen hours, the temperature within the pelvis was 89°; there was no reaccumulation of the lympid fluid, either in the stomach or bowels, which generally occurs after the injection of the saline solution; the blood was fluid in the heart and large vessels; there was no appearance of coagulum, or of curd; the case, in all other respects, was the same as usual, whether before or after death.

Being convinced that nothing but perseverance is wanting to render the injecting plan more effective, I should be obliged to those who have gratuitous patients with cholera, to let me attend; but from other engagements, I cannot afford the time which these cases demand, to allow me opportunities of putting this practice fully to the test. I am, Sir, your obliged servant,

H. Searle.

8, Dean's Place, South Lambeth,
Aug. 10, 1832.

REMARKS ON THE

DISINFECTING POWER OF HEAT,

As an Agent for destroying Contagion or Pestilential Miasmata;

AND ON THE

SALINE TREATMENT OF CHOLERA,

Plague, and Malignant Fevers; and on the Use of Opium, &c., in such Disorders.

By J. Hancock, *M.D., London.*

It has been with some surprise that I have observed, that the disinfecting power of heat is regarded by gentlemen of science as a new discovery. I considered it a very common and prevalent idea, and recollect seeing the method employed, many years ago, on clothing, which had been worn by persons in small-pox, being smoked and *roasted*, I might say, by nurses, before a strong fire, for the purpose of destroying infection. I do not say this to invalidate the experiments of Dr. Henry, who, it may be said, "improves whatever he touches." I have seen the same alluded to, or directed, I think in the old French Encylopedia, and other works, which I cannot now call to mind; but I may refer, on this subject, to the letters of Dr. Lobb, addressed to Martin Folkes, Esq., president, and read at the meetings of the Royal Society, 1743-4. From these I beg leave to give a brief abstract. "I will take up no more time," says Dr. Lobb, "at present, than to mention two methods for removing the *pestilential particles* from *infected goods* or apparel, which may prove both effectual and expeditious. One is by airing them before *brisk fires* made in chimneys;" and he conceives that, "as water is carried off by heat, so the pestilential particles also, which are more volatile, must be speedily removed by the action of heat from infected goods; that although simple exposure to the air would in time effect the same purpose, yet the means proposed *by heat* is likely to do more in one hour, than the other in several days." On the same principle he recommends, that persons who have been exposed to the *fomes* of pestilences, should take the earliest opportunity of approaching a brisk fire, turning round before it to get rid of any infection received in their garments. This, he thinks, deserves the attention of all who have been exposed amongst the sick.

The other method of disinfecting is, by boiling all such articles as may not be spoiled by the process, by which means, as in the application of *dry heat*, the pestilential particles will be carried up the chimney; for he believed the miasma is not destroyed or decomposed, but driven off by heat. *Lobb on the Plague*, p. 48—51; and this opinion would seem to have been borne out by experiments on small-pox, as cited in *Lyon's Treatise on the Prevention of the Plague*, where it is stated that the smoking of infected clothing conveyed the infection of small-pox to some distance, denying that fire will destroy the pestilential miasm, if confined, but he believes it extremely evanescent on the approach of heat, p. 359. I may observe that this book of Dr. Lobb, although abundantly mingled with the superstitious philosophy of his day, contains much sound sense and useful information, and that his pathological opinions are rational, and rather superior for one of his time.

It would appear that, in earlier times, ships, merchandise, and all, were frequently burnt to get rid of contagion, but the good Dr. Lobb, conceiving the *pestiferous particles* may be thus disseminated in the air, proposes another measure equally rational and efficient—that of "*sinking* the infected ships to the *bottom of the sea*! and burying infected goods deep in the earth," which, he says, is likely to prove very effectual for preventing the propagation of *pestilence*!" See p. 366. This would put out of countenance, perhaps, even the most zealous and froward advocates for contagion and quarantine of the present day. The fact seems to be that, in those days, they actually believed in the vagaries which are now known, and acknowledged by most honest men, to be set up as mere *scarecrows*.

Although this author regards the great plague of 1665 to be so very contagious, there are many passages he has cited from the *Journal of the Plague*, which would seem like facts intended to prove quite the contrary; he says, that "through the whole course of the plague, there died in this great city and liberties, but two aldermen, five physicians, thirteen surgeons, and sixteen clergymen, which, considering their numbers, shows very manifestly God's special grace and protection of persons in the discharge of their duty;" and the author of the *Journal*, who appears to have been an honest and impartial man, says that charitable women, "pious ladies," went about distributing alms, visiting the poor sick families in their infected houses, and that none of these suffered from it!

I may take the opportunity of citing here certain old authorities which tend to show that the famed *saline practice* in malignant distempers is not altogether so *fresh* as many seem to imagine, and would beg leave to suggest, that the reclamations of gentlemen lately put forth, respecting the *priority* of the *discovery*, appear to be founded on the mistaken view of its being a new discovery, instead of a *revival* merely of former practice and opinions.

Hippocrates gave salt, sulphur, and wine, in the plague; and Van Helmont says, that the vital spirit is salt, not sour; and further on, that the *vital* spirit is *arterial blood*—an assertion not less true in his time than since it was pronounced by John Hunter! Why should we consider the opinions of the moderns as preferable to the *same* expressed by the ancients? He observes too he has found that *arcanums* do operate as *salts*; "that hurtful matters are removed by resolving, cleansing, expelling, &c., which properties are agreeable unto salts." *Oriatrike*, p. 195-6, and at p. 733. "The vital spirit is plainly salt, therefore balsamical, and a preserver from corruption." Salem esse *antidotum præstantissimum*, tam ad preservationem, quam ad *curationem*. *Angenius*, lib. 9. *Joubertus*, tractatas *De Peste*, cap. 13. And according to Wittichius, the inhabitants of the island of Malta made use of salt as a *preservative and most efficacious remedy* against the *contagion of plague*. And thus Brudus Lusitanus (lib. 3. cap. 19. *De Febr.*):—"Sal esse efficacissimum preservat. ac curat. in peste, et refert chirurgum quendam in anglia esse *piscibus salitis* peste laborantes feliciter nutrire."* This antidote to pestilence may be ridiculed by some, but without drawing any inference, it might be remarked that the negroes in the colonies who eat much salted fish, are rarely attacked with pestilential or malignant fevers, to which the lower ranks of society are most obnoxious in Europe; and furthermore, that certain non-medical persons in Martinique, who had charge of patients in yellow fever, assured me they had found the greatest advantage in the use of *sea-water*, both by the mouth and by frequent ablution, applied at various temperatures, such as seemed most agreeable to the patient's feelings, and that it rarely failed if seasonably resorted to.

Such considerations give additional weight to the reports of Dr. Stevens, and the recent modification of his plan by means of *venous injection*, which modification, however, can hardly be entitled to any preference, and is not likely to be fully established in practice, since it has been found, that the operation of remedies is nearly identical, whether exhibited by the mouth or by venous injection, as proved by injections of tart. ant. and other emetics, cathartics, &c.

Experiment is certainly the touchstone of truth; but until the contrary be proved, I should consider the probability to be in favour of Dr. Stevens' method, along with the external use of salt in *baths,* as just noticed. In the yellow and remittent fevers of Demerara, we frequently employed *cold affusion* (of fresh water), but it never occurred to me to try the *salt* water baths,[*] or salt in any form, not at least as a *chemical alterative* agent, or for the purpose (as technically expressed) of *decarbonising* the blood. From all I have learnt here and in South America, I cannot but regard this problem as one of the utmost importance, and heartly join in the recommendations, so powerfully urged in THE LANCET, for the fullest possible investigation as to the real merits of *saline* treatment in malignant fevers, cholera, &c., and at the same time I would beg leave to hint, that from the hypercatharsis induced by the salts, it becomes especially necessary to guard their operation by restringents, and especially opiates, which are of the utmost avail in maintaining the force of the circulation, and the vital power, in the cold and collapsed state, which often characterise those disorders.

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* Diermerbroek de Peste, p. 170. Lobb on the Plague, p. 255.

In addition to the foregoing it may also be observed, that as antidotes to the poisoned wounds, there are none, perhaps, more generally useful than common salt, (mur. of soda), and the great use of sal prunel. (nitr. potash) has long been recommended in putrid and spotted fevers, plague, and cholera, as also in leprous and scorbutic affections, putrid sore throat, &c.

Unfortunately, amidst a vast farrago, the most valuable remedies are overlooked or neglected at the onset of disease, when they might be efficient: and here I consider it most unfortunate that the use of opium is so little appreciated, and that its due application is so misconceived: [†] those who have tried full doses at the onset of putrid diseases have seldom been disappointed. Although many, from mistaken theories and false *indications,* are led to condemn the use of opium, excepting in the decline of the malady, it must be remembered that remedies become in a short time unavailing after the attack of malignant distempers, such as cholera, camp-dysentery, putrid fevers, &c. When such prevail, an opiate pill or potion ought ever to be kept in readiness to be swallowed at the instant that symptoms are approaching. After the disease is fixed, or the blood has lost its spring and mobility, or, retiring from the surface, stagnate, in the great vessels, medicine has little effect. It is not the choice of the weapons merely, but their ready employment, which must guard against the inroads of an insidious enemy. This matter, and the powers of nature, being duly considered, we discover why so many remedies get a reputation, and why some persons find one remedy to succeed, and some another. But, with respect to the use of opium, a pernicious idea prevails, that, in most diseases, although marked by excessive prostration of strength, yet that opium must be avoided till re-action flags, or the powers of life are fast declining: it is a fact not generally recognised, that the powers of opium are most pre-eminently efficient in the stage of depression, or at the very onset of those malignant distempers. Yet the golden moments and hours are often cast away in waiting for *medical assistance.*[*]

In the treatment of such untoward diseases as spasmodic cholera, jail and putrid fevers, so called, it is ever desirable to take advantage of a union of powerful remedies. The cordial and exhilarating effects of opium, are greatly exalted, and rendered more permanent, whilst its narcotism on the brain is corrected, by taking, soon after, a cup of strong coffee, or a glass of brandy or other spirit. By these means I have often known inveterate agues subdued, both in my own person and in others; some of which, too, in the Orinoko, were of a very malignant kind. It may not be out of place to mention here, that on one occasion (in Stabroek, now Georgetown, Demerara) I had for some months suffered under one of the most obstinate intermittent fevers, in which the fullest trials of the bark, arsenic, &c., had proved unavailing. On the fit approaching one day with severe rigors, I took 100 drops of laudanum in a glass of punch, and, soon after, a cup of strong coffee, covering myself warm in bed; the cold shivering soon ceased, and a warm perspiration came on. I slept for a short time, an hour or two perhaps, when I awoke in a profuse perspiration, quite free from fever and all uneasy sensations. At the next return I was prepared to take the same course, but found no occasion for it. I had no return of ague for years afterwards, It is only by *additions* to opium that we can rationally expect to improve its virtues and modify its narcotic effects. The *learned* will before long, it is to be hoped, learn better than to waste their time in fruitless speculations on morphia and narcotine.[*]

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[*] The water of the coast of Guiana is muddy, and brackish only, and is therefore seldom thought of for domestic purposes.

[†] It is curious to observe the inconsistencies of certain medical writers, who, at times, would seem to be utterly ignorant of the action of remedies. One gentleman has lately set forth *experiments,* which are termed the results of cholera cases, treated *chiefly* with *corrosive sublimate* and *acetate of lead;* these with half a grain of p. opii, and some other remedies he administered every *half hour,* but having no effect, he increased the dose of opiate power, making it *two and a half grains* every *half hour.* On this the patient rallied, and from a hopeless state of collapse recovered most rapidly. Such were the happy results of "Cases of cholera morbus treated *chiefly* by *corrosive sublimate* and *sugar of lead.*" No doubt the statement is a faithful one; the result indeed is what might be expected from the use of opium given every half hour. We thus see the most valuable remedies pass for nothing, and daily resorted to for no other purpose than to support the fictitious claims of the most useless ones.

[*] Along with the opiate, or presently after, a few grains of calomel may be given, and saline diaphoretics, as nitrate of potash or ammonia, effervescing draughts; as also sudorific tisans, made with equal parts of rue, sage, mint, and hyssop.

If therapeutical indications are demanded, it is impossible to adduce stronger ones than may be offered for the employment of opium in cholera—a disease marked by excessive declension of the vital powers—the *pulse*, strength, and natural heat, failing, with colliquative sweats, diarrhœa, spasmodic pains of the bowels, and of all the muscles of the body; for all which affections opium is the most direct resource, if not the only one worthy of confidence; yet it is surprising to hear some gentlemen assert, that the necessity for opium appears not to be indicated in cholera. Again, the blood in this disease is found to be thick and grumous, as fully proved by dissections: it is generally supposed to be rarified and attenuated by opiates;[†] this is inferred from the great and full, though soft and slow, pulse, induced by a full dose; and it is asserted, that in Turks and Indians slain in battle whilst under its influence, the blood has been found in a fluid state for a day or two after death! (See Geoffroy Mat. Med.) Opium has long been esteemed as the most advantageous and effectual remedy for all *inflammations*, and is especially commended by Bate, Willis, Motherby, and others, in pleurisy. It has probably been too exclusively relied on, to the neglect of its requisite adjuncts; and, through the caprices of certain leading authorities of the last century, this valuable remedy fell in repute, and was totally proscribed in all cases of inflammation.

Dr. Armstrong again attempted to establish its use in fevers and inflammations; he has been ably seconded, but with little effect, however, such is the force of prejudice and early impressions. Motherby, who gave the collective opinions of his time, says it is anodyne, antispasmodic, that "it *abates inflammation* both internally and externally; *it moderates the heat in fevers*, and assists other medicines in producing a more speedy effect; it discusses *stagnating fluids*, removes obstructions arising from spasm, promotes the discharges through the skin, but retards all other evacuations; abates all kinds of pains, &c."

As to the subsequent treatment of these disorders, when further developed, the use of baths, stimulant frictions, and other important measures, it is not my purpose to advert here, having, in a pamphlet on the subject, stated the result of my experience on the *vomito prieto*, and cholera of South America.

Respecting the alleged contagious nature of the present epidemic, it appears sufficient to remark, that were the disease really contagious, it would assail those chiefly who have been in company and in contact with the sick, or whose occupations expose them most frequently to its fomites; whereas it appears that people who are thus daily compelled to take the breath and handle the sick and dead bodies, are not more frequently attacked than others who have not seen or been near a case of it. This is proved by daily experience both in and out of the hospitals.

Turner-street, Commercial-road.
August 13th, 1832.

P.S. The important experiments of Dr. O'Shaughnessy on the conditions of the blood in *health* and *disease*, merit the serious consideration of the faculty, as calculated to throw much light, perhaps, on the nature of our disorganizations. They appear to indicate, that the morbid changes in the blood are rather the consequences than the cause of disease, at least with respect to cholera; no change being appreciable till the disease had made great advances. I would suggest the query, however, whether the latent germ of disease may not previously exist, and yet be totally incapable of detection by chemical agency, or by any mode of examination whatsoever. Those morbid states of the atmosphere which give rise to epidemic distemper, have, hitherto, eluded the researches of chemistry. Innumerable experiments had been made on mineral waters and that of the ocean, before the smallest traces or indications of iodine or bromine were discovered; and we know not what other principles may yet remain to be elicited from them. Ancient philosophers, we are told, have opened the body to search for the soul, but we must not suppose that the *vital principle* is to be detected by chemical research, as if it were an alkali, an acid, or *salt* according to Helmont. It can only be said, in the present state of our knowledge, that the *observable* changes in the vital fluid, are in all cases, the results, and not the cause, of disease.

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[*] Such speculative conceits are of no real value, but are exceedingly pernicious, deluding both young and old, under the specious misnomer of useful science. Our knowledge of natural bodies has been greatly improved by the march of chemical science; but in pharmacy, although many senseless fancies have been abrogated, it appears that the science has of late years rather retrograded in point of real and practical utility, notwithstanding a vast display has been made of the wonderful discoveries of some industrious Frenchmen, as Derosne, Sertuerner, and Robiquet. These conceits serve to occupy the pages of our new dispensatories, as most important discoveries, whilst no one undertakes to investigate their real or pretended value. It is right to observe here, that the black drop, and later pretended discoveries, the *liquor opii sedativus*, &c., are mere modifications of receipts known 300 years ago, or more, and subsequently under the name of Bate's *laudanum cydoniatum*, or opium fermented in the juice of *quinces*, with an addition of *yeast*; a preparation at least equal to any at present employed, whether English or foreign.

[†] Similar resolvent powers have, from the earliest times, been ascribed to divers herbs, especially to sage, rue, mint, and hyssop.

CALCULUS IN THE BLADDER.

To the Editor of The Lancet.

Sir,—I have the honour of forwarding to you the history of another case treated with the *percuteur courbe à marteau*, begging you to give it insertion in your valuable Journal. I am, Sir, your obedient servant,

Baron Heurteloup.

1, Vere Street, Cavendish Square,
Aug. 10, 1832.

Mr. M., of the East India Company, ætat. 63, of a tolerably good constitution, tall, but rather emaciated and weak, experienced for the first time, about twelve years ago, pain in the loins, which returned at different intervals, with more or less severity. These attacks were at first slight and of short duration, and were accompanied by the evacuation of red sand with the urine; but after some time they became more severe, and the patient frequently voided smooth, round, calculi, about as large as small peas. Besides these nephritic attacks, Mr. M. sometimes found great difficulty in making water, and occasionally felt considerable pain all along the urethra, after he had voided his urine. About six years after the first attack, Mr. M. finding that the symptoms increased in severity, and fearing that they were caused by the presence of a stone in the bladder, consented to be sounded; but although a very careful exploration was made, no stone could be discovered.

Quieted by the assurance that he was not labouring under stone in the bladder, Mr. M. pursued his usual occupations for nearly six years more, and paid but little attention to the inconveniences and painful sensations which he sometimes experienced. At the commencement of the present year, however, the symptoms assumed a much more severe and serious character; Mr. M. was much alarmed, and wished to be sounded again. A stone was distinctly felt in the bladder, and the patient immediately left his country seat, where he had taken up his abode with a view to improve his health, and came to London. He consulted Mr. Green, who, finding the symptoms too indicative to consider it necessary to sound the bladder, did me the honour to recommend the patient to me.

I sounded with the *recto-curvilinear catheter*, and found the urethra rather narrow, especially at its anterior portion; the bladder moderately sensitive and dilatable, considering the size of the stone, which appeared to be rough and heavy, and about an inch thick, and from eighteen to twenty lines in its longest diameter; it was held by the sides of the inferior part of the bladder, but with the sound it was possible to turn it over. Notwithstanding the advantages to be derived from the employment of the *percuteur courbe*, this case was rendered difficult by the size of the stone, and the little physical force of the patient; I was undecided as to whether it would be well to operate, and I was only induced to do so by the anxious wish of the patient, and more especially by the resolution and moral energy which he displayed.

After having, by a small incision, enlarged the external orifice of the urethra, which was extremely narrow, and prevented the introduction of an instrument sufficiently large to fill up the remainder of that canal, I commenced the operation with a *percuteur courbe* of about three lines and a half in diameter. Having first, with the catheter, removed the stone from behind the neck of the bladder, where it was continually situated, I introduced the instrument, and immediately seized and broke this large calculus into fragments; two of the pieces, which the *percuteur* showed to be about twelve or fifteen lines in diameter, were afterwards comminuted in the same manner, and the instrument was then withdrawn. The patient passed a considerable quantity of the *debris* of the stone immediately, and during the three or four successive days. In four other applications of the *percuteur*, each of which, as well as the first one, lasted from three to four minutes, the patient was enabled to evacuate all the portions of stone contained in the bladder.

Mr. M. having naturally a small stream of urine, and but little repulsive power, found difficulty in voiding the fragments of stone, and I was consequently obliged to have recourse to the *evacuating catheter*.

Mr. Rose, the patient's medical attendant, saw the first operation, and Mr. Green was present at all of them.

Reflections.

This case is not only remarkable on account of the size and hardness of the stone, which was composed of oxalate of lime, but also on account of a deficiency of physical force in the patient; and it proves that cases may be prudently undertaken notwithstanding those difficulties, provided the patient have on his side firmness and moral energy. It may, however, be added, that his firmness was not put to a very severe trial, for Mr. M. obtained relief rapidly, and without much suffering. It is to the use of the *percuteur* that this favourable result may be attributed; for the size of the stone was such, that I can safely assert, that the application of any other instrument would not have been attended with success; for whatever be the insensibility of a patient, a moment must arrive after which he can no longer bear the manœuvring of the instruments; and Mr. M. was unusually disposed to fall into this unfavourable condition. It was, therefore, exceedingly fortunate that he had only to undergo very few applications of the instrument, and each of short duration.

We may fairly suppose that the stone had, in this case, been forming for twelve years, and it was only discovered after that long lapse of time, although the patient was sounded about five years after the appearance of the first symptoms. This circumstance shows the importance of seeking to possess the most perfect possible means of exploring the bladder. A large *curved catheter* is, I think, less accurately adapted for performing a careful exploration than a *small curved* one, which by being straight in the portion that remains in the urethra, allows the instrument to be revolved on its own axis, whilst with a catheter presenting a large curve, this movement is impossible. The curve of my *recto-curvilinear* catheter* has appeared to me the one best suited; and I think I may venture to recommend it as such. It would have saved Mr. M. years of pain and inconvenience had he sooner been apprised of the nature of his complaint, and sought earlier for relief from it. The stone would then have been smaller, and the difficulties of the case would have been greatly diminished.

CASE OF

IMPERFORATE VAGINA.

OPERATION — PERITONITIS — DEATH — UTERUS WANTING.

By JOHN MACFARLANE, *M.D., Sen.-Surg. Roy. Infirm. Glasgow.*†

To show the danger which sometimes follows operations on the sexual organs of the female, as well as to confirm a curious physiological fact regarding the uterus, I subjoin, from my private note-book, the following case:—

Mrs. C., about twenty-eight years of age, consulted me on the 4th of February, 1823. She was rather a stout, well-formed woman, but had a pale and sickly appearance. After stating that she had been subject to severe attacks of epistaxis, at irregular intervals, since she was sixteen years old, and to vertigo, flatulence, palpitations, pains in the lumbar region, vomiting, occasional diarrhœa, &c., she confessed that she had never menstruated. On farther inquiry I found that she had been married for twelve months, and that there existed an obstruction in the vagina, which she was anxious to have destroyed.

On inspection, the external orifice of the vagina was found completely closed by a thick, firm, muscular-looking substance, continuous with the inner margin of the labia, and adhering to the pubes below and around the urethra, so as to leave not the slightest vestige of an opening. It had neither the feel nor appearance of a membrane, but was granular on the surface; and when the labia were freely separated, bands of muscular-looking fibres were seen passing to it. The little-finger could, by pushing this obstacle before it, be introduced about half an inch into the vagina, which was excessively small and contracted. The external parts were well formed, and the orifice of the urethra was natural, but the clitoris was unusually small. There was no swelling in the hypogastrium, the bladder and rectum were free of obstruction, the mammæ were plump and well formed, and the sexual appetite was not deficient.

From her great anxiety to have this malformation destroyed by an operation, I agreed to perform it on the afternoon of the day on which she first consulted me. Having placed her in the lithotomy position, I commenced, with the assistance of my friend, Dr. Weir, by making an incision with a bistoury an inch and a half in length, in a perpendicular direction, through the centre of the fleshy-looking obstruction. I then thrust forward a sharp-pointed, narrow bistoury, for more than an inch opposite the orifice into the vagina, directing the point rather downwards, with the blunt edge to the pubes, to avoid wounding the urethra. I found, however, from the great thickness of the substance, and the uncertainty regarding the position of the parts in the track of the vagina, that a continuance of this mode of procedure was neither safe nor justifiable. I could not succeed in forcing a probe or a director through the remaining portion; I therefore divided and removed, by means of the forceps and scissors, several layers of it. By keeping a catheter in the urethra as a guide, and by occasionally introducing the finger into the rectum, I was successful in clearing a passage fully two inches in depth. There was still, however, an obstructing portion, beyond which there appeared to be a free cavity. Through this a pair of dissecting scissors were passed, and the remainder of the obstruction divided by introducing a director into the opening thus made, getting it behind the upper part of the obstruction, and forcing it nearer the external orifice. The finger was then passed into the upper part of the vagina, which was lubricated apparently by mucus, but no uterus could be detected. A small piece of soft sponge, through which a strong thread was passed, was inserted, and a few strips of lint were placed between the external lips of the passage.

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* See my work on Lithotrity, page 261, plate 4.

† From Macfarlane's Clinical Reports of the Surgical Practice of the Royal Infirmary, Glasgow. 1832.

5th. Could not be persuaded to keep the house, but called for me to state how free from pain she had been since the operation; considerable sanguineous discharge from vagina; the sponge was withdrawn, and the parts lightly dressed with oiled lint.

On the 8th she was exposed to a severe snow-storm, while assisting her husband in his trade as a broker; in the evening she was intoxicated, and even guilty of other irregularities; to which rigors, acute pain in the right side of the abdomen, flatulent distention and nausea, speedily succeeded.

9th.—The pritoneal inflammation appears to proceed rapidly; the tension and pain of abdomen are now more urgent; there is vomiting, with furred tongue and diarrhœa; pulse 136, small and wiry; skin hot and dry. *Was bled three times, to the amount of forty oz.; had a dose of ol. ricini, followed by fomentations to the abdomen; calomel and opium; and the injection of the tepid water into the vagina.*

10th. The symptoms, for several hours in the forenoon, were moderated by the application of a large blister to the abdomen; in the evening they were much worse, and appeared to be aggravated by the imprudent conduct of her attendants, who, contrary to the most positive injunctions, gave her wine to support her strength, opium pills to procure sleep, and nitre to remove the strangury occasioned by the blister. The belly was excessively tumid, tympanitic, and exquisitely painful on pressure; the breathing hurried, the countenance sunk and anxious, and the vomiting almost incessant; pulse 150, tremulous and compressible; tongue dry and furred.

11th.—Vomiting abated, pain of abdomen still continues as acute and extended; appears to be sinking rapidly. Died on the morning of the 12th, at two o'clock, about 203 hours after the operation.

Post-mortem examination.

13th.—The body was inspected in presence of Dr. Weir, Mr. Stirling, and several of my pupils. The omentum and the peritoneal covering of the intestines and abdominal muscles were covered with patches of inflammation of a bright-red colour, as was also, in a smaller degree, the peritoneum, covering the fundus of the bladder, and that portion of it in which the uterus is usually involved. The ovaries were well formed and large, the Fallopian tubes were each one inch and a quarter in length, their fimbriated extremities being perfect. There did not exist the slightest vestige of a uterus, its usual situation between the transverse folds of the peritoneum being occupied by a portion of condensed cellular substance, about the size of a filbert, which was more than an inch distant from the uterine extremities of the Fallopian tubes, and was but loosely attached to the peritoneum. When the finger was introduced into the vagina, the cavity at its upper part was ascertained to be formed by the peritoneum, between which and the upper boundary of the obstruction there was a free space capable of containing a small line. The vagina, when slit open, presented an ash-grey appearance; the urethra, bladder, rectum, and peritoneum, were uninjured.

Remarks.

The obstruction in this case was evidently congenital, and differed, both in situation and appearance, from that cohesion of the parts which is sometimes produced by neglected ulceration. An obstruction from the latter cause may occur both in children and adults; it is, however, rarely confined to the vagina, but more generally involves the external parts, as the nymphæ and labia. The most extensive case of this kind which I recollect to have met with, was in a prisoner in the Bridewell of this city in 1817, during the short period of my attendance as surgeon to that establishment. From a long-continued gonorrhœa and want of cleanliness, the lining membrane of the labia, and the inner surface of the nates around the anus, became excoriated and ulcerated, and adhesion by granulation took place through their whole extent, leaving only two small openings for the escape of the urine and feces. The adhesions were divided, and the parts restored to their natural state.

We are told that when the uterus is altogether wanting, or of a small size, the external parts are but imperfectly developed, and the vagina is either very short, or altogether obliterated. In this case, all the external parts were well formed and entire, and the mammæ were fully developed, yet no uterus existed—this circumstance is, however, easily explained, when we find that the ovaries were present. These organs are known to be more intimately connected with the growth of the external parts of the female than the uterus, and when they are absent, the marks of puberty are rarely, if ever, exhibited.

MODIFICATIONS

IN THE

SYMPTOMS OF CHOLERA.

As the symptoms of cholera are somewhat modified in this country, I shall attempt to describe, in a general way, their modifications, and also the modifications which the treatment necessarily assumes, before entering upon an enlarged consideration of the disease, and of the remedial measures. The symptoms are commonly much slower in their progress to death or to recovery in England than in India. In England the disease is commonly attended with premonitory symptoms of many hours', or perhaps of some days' duration, previous to the commencement of the severe attack, affording a most favourable interval for the adoption of medical treatment; and even when the severe symptoms have set in, it is but seldom they advance with such rapidity as was their usual course in India. In England, the cramps are less severe in the extremities, and the spasmodic action of the stomach and bowels, evinced in vomiting and purging, is commonly less violent. The cold and clammy perspirations are less profuse. The patient loses his animal powers less rapidly, and the blue, or *poisoning*, appearance, as it may be called, is slower in establishing itself. In India, the disease was seldom attended with severe consecutive fever, and when it was, the fever assumed the types indigenous in that country. In England, acute cholera is almost uniformly productive of a severe fever, and this fever assumes the different types indigenous here, appearing, according to circumstances, in the form of typhus, or of simple inflammatory fever, or of the synochal. In this outline are enumerated the more prominent modifications which the symptoms of cholera have presented in England; but, it must not be forgotten, many cases occur which preserve throughout the whole of the acute period an exact resemblance to the cholera in India, and without undergoing the slightest modification in any of the phenomena that precede the consecutive fever.—*Kennedy's History of Cholera, third edition.*

IRREGULARITIES

IN THE

SYMPTOMS OF CHOLERA.

The above description of the symptoms will apply to the most ordinary course of cholera; but a deviation from this course is not unfrequent, consisting either in the absence of some one or more of these symptoms or of a variable degree in their intensity. These irregularities in the phenomena have been the cause of much apparently contradictory testimony and strong feeling among medical men, which might have been avoided by an appeal to general experience—for, in no disease do we find *all* the symptoms known to have attended it, present in every case, or of uniform intensity. Severe cramps in the limbs are commonly experienced in an attack of cholera, yet many cases occur in which the cramps are slight, or altogether absent. This exemption has been more particularly remarked in the attacks of young children. Complete suppression of urine is a common symptom; yet many cases occur in which the suppression is only partial, and in some the urine is voided in its natural quantity. After being suppressed, the secretion of urine is usually restored early in the consecutive fever; yet occasionally it remains absent during the whole of the febrile period. In an exceedingly rare case, there has been no purging, and less rarely the absence of vomiting has been remarked. I have not seen both of these symptoms wanting in England, but in hot countries where the disease has proved almost instantaneously fatal, such cases are recorded. The blueness and macerated appearance of the skin of the hands and feet are frequently not present. Instead of the shrinking of the features, a puffiness of the face has been noticed in children, and in elderly persons of lax fibre and loose adipose structure. Some patients, while in the acute period of disease, breathe with difficulty, and suffer from jactitation; others again die in the calmest manner, without either of these symptoms. In some the voice remains clear and pretty firm to the last. In three cases I have seen blood expelled from the bowels in considerable quantities; and in two cases I have seen hemorrhage from the nose. The abnormal symptoms do not seem, in the order of their occurrence, to have any connexion with each other. The secretion of urine may be present equally in a blue case as in one where this change does not take place—and so of the remaining symptoms.—*Kennedy's History of Cholera, third edition.*

Debility does not Predispose to Cholera.—If we except the condition of the mucous membrane of the intestines, it may be said in general terms that the organs of those who die of cholera are peculiarly healthy. It was remarked by all who assisted at our autopsies in the Albion Street Cholera Hospital, Glasgow, that they had never seen the same number of examinations exhibit as little disease. The brain, heart, liver, spleen, kidneys, and urinary bladder, were almost universally found free from organic lesion. The kidneys were more frequently diseased than any other gland. The lungs, disease of which constitutes so large a part of our annual bills of mortality, were found so generally healthy, that I could not have believed that the same number of individuals, taken indiscriminately, would have presented these organs in a condition so free from organic lesion. From this I am justified in the inference that organic disease, or debility arising from it, does not predispose to cholera. This is a startling conclusion, but for its truth I refer to our hospital journals, which contain accurate histories of upwards of six hundred cases. How many hundreds are at this moment labouring under phthisis in Glasgow, and how few of them have been carried off by cholera!—*Dr. Lawrie's Report, Glasgow Jour.*

Deficient Nourishment not a Predisposer to Cholera.—If full muscular vigour, and plump (at times overloaded) cellular and adipose tissue, be any proof of adequate nutriment, deficient nourishment does not predispose to this disease. The majority of our patients exhibited apparently full muscular power, and few were much emaciated. A reference to the tables will show that out of 455 cases, 331 were between the ages of 7 and 50, while 30 were below 7, and 94 above 50. This conclusion is still more startling than the last, but must be admitted with greater limitation. We know that a very small quantity of vegetable food is sufficient to support life, and give the frame the appearance of muscular vigour and ample nourishment, yet when such persons are brought into competition with others fed on animal and more nourishing diet, their inferior capability of fatigue is soon manifested. It is probable that their relative power of resisting disease may be in the same ratio. Be this as it may, our experience falsifies the loose assertions of those authors, who assert that the diseased, the gaunt, and the emaciated, are the only victims of cholera.—*Ibid.*

Invariable Symptoms of Cholera.—Of all the patients admitted into the Albion Street Cholera Hospital, Glasgow, there has not been one in whom the disease did not begin in the bowels and stomach. In the vast majority purging was either premonitory, or the first symptom. Some there were who said that nausea and vomiting first annoyed them, but all had purging early in the disease. In some there was no vomiting, in others no cramps, in a few the pulse, capillary circulation, and respiration, were good, but all, to a man, had a discharge of characteristic watery fluid from the bowels. So much am I convinced of this, that I now consider watery purging essential to cholera, and have no hesitation, in my own practice, in pronouncing any disease not cholera in which this symptom is absent. The purging is usually unattended by griping pains. In every case, which I have had an opportunity of watching from its commencement, the failure of the pulse, capillary circulation and other symptoms of collapse, have been in marked proportion to the amount of fluid discharged, the rapidity of its discharge, and its approach to water in colour, consistence, and smell. I know no symptom by which a common diarrhœa or disordered stomach and bowels can be distinguished from the early stage of cholera.—*Ibid.*

Sulphate of Copper, or Nitrate of Silver, in Cholera.—It strikes me as very remarkable, that during the various and long-continued discussions on cholera and its treatment, no one has ventured to recommend the use of the sulphate of copper, nitrate of silver, or some of the more powerful astringents. The rapid exhaustion attending the disease, produced apparently by the extremely copious alvine evacuations, seems to me to point out the necessity for having recourse in any early period of the disease to the use of such remedies as the above. Nor can there be considered anything rash or unwarrantable in the use of such remedies in the disease in question. It is now well known that some very violent diseases, although commencing with inflammation, very rapidly change their character, and with an atonic condition of the parts affected, are attended with profuse discharges. These, it is now well known, are best treated by powerful astringents. I need scarcely instance what seems to me a very analogous disease (the mucous lining in both being the primary seat of diseased action) occurring in a very different, more limited, and less vital part of the system than that affected in cholera, viz., purulent ophthalmia. But it is scarcely needful to travel so far from the seat of disease; we have an example in the case of dysentery nearer home. If any one having opportunities should be induced to put this plan to the test of experience, I think it would be best to follow the mode of combining the salt with a due proportion of opium, as recommended by Dr. Elliotson.—*Communication from Mr. John Walker, of Manchester.*

Blistering in Cholera.—An officer in the Honourable East India Company's Service was attacked by the cholera on his homeward-bound voyage to England. By mistake the stomach-warmer, filled with boiling water, was applied without the intervention of flannel; it very quickly produced a blister, and all the urgent symptoms of the disease disappeared within three hours, leaving the patient with only one inconvenience,—the unsought-for blister, which confined him to his hammock for a week. Although the remedy may appear severe, yet as the skin in the first stage of the disease is insensible to the action of chemical agents, the pain arising from it is not so great as might be supposed, and I think it would be well if the heads of families were to add a stomach-warmer to their kitchen furniture.—*Communication from Mr. S. Millard, Jun., of Bristol.*

Nitrous Oxide in Cholera.—From the effects which are sometimes experienced in consequence of inhaling nitrous oxyde gas, I am led to think that it would answer very effectually the purposes of a stimulant in cases of cholera; for when the state of excitement is over, the sedative will not, as after the use of other stimulants, ensue. Not having a sufficient number of cases to make the experiment on, I suggest the remedy without trying it myself, but of this I am fully convinced, that it cannot do any possible harm. The nitrous oxyde may be very easily prepared by disengaging the gas by means of a lamp, from nitrate of ammonia, and receiving it (previously passed through water) into a bladder with a stop-cock and tube attached to it, any quantity may be inhaled by that means without disturbing the patient in the least. Should any gentleman make trial of it, I should feel extremely obliged if he would relate the case in The Lancet. J. Hudson.

Aug. 11.

Insanity.—The raving mad are more frequently cured than the melancholy, in Bedlam, in the proportion of 62 to 27: a proof that it is easier to moderate too great irritability than to animate insensibility. In the year 1789, Black found in Bedlam,—insane from grief and misfortune 206, religion 90, love 74, jealousy 6, fright 51, study 15, pride 8, drunkenness 58, childbirth 79, constipation 10, hereditary complaints 115, contusions and broken bones 12, venereal disease 14, smallpox 7, repercussion of the itch and healed ulcers 5. From the year 1748 to 1820, the number of insane recorded in England was 32,744, of whom 12,254 were cured; from the year 1801 to 1813, there were admitted in France 4213, cured 1893; from the year 1798 to 1823, admitted in Würzburg 528, cured 292. The lists of the Berlin Charité exhibit, on an average, not more than 4 cures out of 15 cases. It appears, therefore, that the method of treatment pursued at Würzburg has proved most successful.—*Freidrich's Diagnosis of Mental Diseases.*

DUBLIN CHOLERA HOSPITAL.

To the Editor of The Lancet.

Upon looking into a late number of The Lancet, I was not a little surprised to find a letter, headed Juvenile Appointments to the Dublin Cholera Hospital, complaining that pupils should be allowed to risk their lives in the public service, while members of the College of Surgeons might be found who would be glad to have accepted the situations affording pay. Having lately held one of those situations, I feel myself called upon to reply to this complaint. The writer states, that the surgeons of Stevens's and the Richmond Hospitals, on the breaking out of the disease, immediately appointed a number of their own apprentices, at the same time questioning, in an under-hand manner, the right those gentlemen possessed to make these paltry appointments, they being, as members of the Board of Health, intrusted by their fellow citizens with the regulation of the public hospitals. Mr. Cusack of Stevens's and Mr. Prile of the Richmond, are evidently the gentlemen alluded to; and of the four resident pupils at Grange Gorman Lane Cholera Hospital, two were apprentices of Mr. Collis, who had nothing whatever to do with the appointments, one of Mr. Carmichael, who was in the same predicament, and but one of Mr. Prile's. He states that *they* were mere boys, and at most but three years at the profession, the youngest of whom was in his twenty-first year, and the most junior within six months of the termination of his apprenticeship. But I should be very sorry to urge these as qualifications to be compared with the possession of a diploma from the College of Surgeons. On the contrary, I venture to affirm, that there was not a pupil in Dublin, who had the most remote idea of being employed, so long as there remained a licentiate in want of a situation, but when those situations remained for nearly a whole fortnight open, and in the want of men to fill them (and what qualified man, may I ask, would accept of a situation, the duties of which were in the highest degree arduous, harassing, and fraught with danger, for a bare *7s. 6d.* per diem?), it was then that the offer was made to the pupils, by many of whom it was refused before we even heard of it. It is true, that after a short time, in consideration of our services, our salaries were increased so as to make them desirable to even a qualified person;* but would it have been then in any respect fair to take them from us, who had "borne the burden and heat of the day," and given them to others who did not think them at first worth their acceptance?

The insertion of these remarks in your far-famed publication will particularly oblige,

A "JUVENILE" CHOLERA DOCTOR.
Dublin, June, 1832.

HOSPITAL OPERATIONS.

To the Editor of THE LANCET.

SIR,—Having lately witnessed many unsuccessful operations at the London Hospitals, I have been induced to make a comparison between operations performed in private practice and those occurring at hospitals, and the success is greatly in favour of the former. This being admitted, it becomes a question why nearly all cases amongst the lower orders requiring surgical operation should be sent to those institutions? I am aware the general answer is, "Because they have more comforts and are better provided for than at their own houses;" but I am inclined to think it more generally arises from reluctance to the part of the medical attendant to use the knife. How many patients are yearly sacrificed in consequence of the operation for strangulated hernia not being performed in proper time! I am confident the following is not an exaggerated case, and many of your readers have witnessed several such during their hospital attendance. A poor creature labouring under strangulated hernia sends for a general practitioner, who, after trying various means to reduce it, is unsuccessful, and orders the patient to be sent to the hospital, perhaps a distance of four or five miles, and the vehicle a rough cart. On his arrival, the dresser (perhaps a perfect tyro) commences another attack upon the parts, which probably fails. The surgeon is then sent for, and arrives in four or five hours. The taxis is again applied without success, and as a dernier resort the operation is had recourse to, the intestine found gangrenous, and the patient in a short time is a corpse. I have seen this operation performed frequently amongst the poor in the country, and generally with success; but I confess at the London Hospitals the result has been far otherwise, arising from the operation being too long delayed. Yours respectfully,

A CONSTANT READER.

REBUILDING OF

WESTMINSTER HOSPITAL.

(From a Correspondent.)

THE committee appointed to conduct the arrangements for rebuilding this hospital have at length brought their labours to a conclusion. Last spring a plot of ground, situated in the Broad Sanctuary, opposite the north front of Westminster Abbey, was purchased from the Commissioners of Woods and Forests, for the sum of 6000*l.* sterling, which was immediately paid. The ground purchased comprises an area of 26,000 square feet, and was deemed sufficiently spacious for the erection of a building adequate for the reception of 200 patients. It was thought desirable that the architecture of the building should harmonize as much as possible with that of the adjacent abbey, and it was resolved accordingly that the elevation should be Gothic, of the later Tudor style. Consonant with this resolution, prospectuses were forwarded to many eminent architects of this metropolis, by whom several plans were sent in. After six weeks' deliberation, the design of Mr. Inwood, the well-known architect of St. Pancras church, and of the Colosseum, was chosen. In this act, it must be allowed, the Committee displayed much taste and judgment, for it is difficult to determine whether the design of Mr. Inwood be most admirable for its beauty, or most estimable for its convenience. The mansion is designed in the collegiate style, and is remarkable for lightness, simplicity, and a certain *otium cum dignitate* highly appropriate to the purpose of its institution. What chiefly attracts the eye of the medical connoisseur, however, is the economy of the interior arrangements, in which the artist has ingeniously contrived to afford unstinted supplies of light and abundant ventilation, and to combine the utmost amplitude of space with the most judicious distribution of offices. The building is adapted to accommodate 220 patients, and each patient is allowed a cubic space of 1900 feet. Each ward is calculated to hold 15 patients, a number which experience shows to be most convenient for the enforcement of that regiminal coercion which constitutes the principal advantage of hospital treatment. The distribution of water-closets, bathing-rooms, and other necessary offices, is effected with much convenience and simplicity, and the whole building, in short, is well suited for the efficient management of a large hospital. The design of Mr. Inwood forms a striking and advantageous contrast to the much-vaunted and baby-house refinement of the Derby Infirmary.

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* The emolument attached to the situation varies, we are informed, from 10s. 6d. to 15s. per day, with board and lodging. The above letter was mislaid.—ED.

The plans and elevations having obtained the approbation of his Majesty's Commissioners, have been contracted for by Mr. Baron, who undertakes to carry into execution the designs of Mr. Inwood for 24,965*l.*, and the alterations suggested by his Majesty's Commissioners for a further sum of 570*l.*, making a total of 25,535*l.* To this must be added, the architect's commission of 1250*l.*, which will enhance the expense to 26,785*l.* To meet this expense there is a building fund of 22,000*l.*, and it is anticipated that some of the wealthy supporters of the charity will contribute to make up the deficiency. Mr. Lynn, the senior surgeon, has declared his intention of making the munificent present of 2000*l.* The Archbishop of Canterbury, the Duke of Northumberland, and the Marquis of Westminster, have also signified their intention to contribute large sums. But active exertions in behalf of the institution have not been confined to the titled governors. Mrs. Ann Cox, the zealous and indefatigable matron of the charity, ever ingenious in promoting its welfare, recently originated a sixpenny subscription, with a view of enabling those who had derived benefit from the institution to contribute to its prosperity, and her exertions have been crowned with signal success; a sum of not less than 500*l.* has been accumulated by this apparently inadequate means. Mrs. Cox required a donation of sixpence only from each contributor.

It is but justice to observe, now that the negotiations are brought to a favourable issue, that the result is principally due to the judicious management and unwearied exertions of Mr. Charles Hallett, the treasurer and chairman of the committee. The delicate tact and equanimity of temper which he exhibited in leading and controlling a body consisting of such discordant elements as compose this heterogeneous committee, can only be fully appreciated by those who witnessed the many perversities, perplexities, and vexations, that arose during a tedious negotiation of ten months' duration.

It is to be hoped that the governors of this old charity, in withdrawing from their present dingy and dilapidated edifice, will not neglect the opportunity of shaking off the innumerable abuses which have hitherto disgraced the institution.

The engineers commence their labours forthwith.

SHUFFLERS, APOSTATES, AND BATS.

To the Editor of The Lancet.

Sir,—Excuse the heading, it is a queer one, but it concerns queer subjects. Pray, what have you been at, Mr. Lancet, lately? And I suppose you will ask—Pray, what have *you* been at, *Mr. Castigator*? Why, I have been slumbering, Mr. Editor, and so have you. Not lodged in the tomb of the Capulets. No. I have been in a Promethean Lodge, where I have imbibed new heat, a fine store of it, with which I shall now scorch those delinquents who, ever dabbling in the mire of corruption, have attempted to tarnish the bright and glorious shield of medical reform. No, no, *Castigator* has not slept. Somnus has had no influence over him; or, if indeed he have been dreaming, he has indulged only in the reveries of daylight observation.

When I last addressed you, the subject of my brief epistle was Dr. James Johnson. Did I speak reproachfully? If so, he will forgive me, because my complaint was founded upon what I conceived to be his ill usage of you—the father of medical reform. Why, I knew Dr. Johnson of old. Animosity towards him was not comprehended in the phial of my wrath. On the contrary—barring his Latin quotations, I always regarded him as an acute writer—if not a profound reasoner—as a kind father, a sincere friend, and a shrewd physician. But when he allowed prejudice or any other cause to pervert the stream of approbation from the direction in which it ought to flow, then it was that *Castigator* applied the lash; and then, when I do strike, sundry culprits whose wrists were tightly fastened to the halberds, will allow that I do not hit without cutting deeply. The Doctor felt the lash. He awoke from the trance into which he had been thrown, and became sensible of the injury which he had inflicted upon his character by denying justice to others, and no sooner had the inquest on the body of the slaughtered

Miss Cashin terminated, than Jem became himself again, and was the first to do justice to your exertions on that extraordinary occasion. Could I, then, continue to strike? No. My arm was stayed, the thongs dropped, and I freely admitted that my friend Johnson had nobly shaken off the pestiferous influence of slander; that he had regained his manhood—that correct feelings had asserted their predominance over the unworthy aspirations of party. Such was my opinion, and I candidly acknowledge it. Henceforth I am at peace with the Doctor.

But there are enemies of the cause whose steps are ever directed in the paths of unrighteousness. Am I from such men to withhold the lash? No; and if I have restrained my hand during some months past, the forbearance has not been the effect of decrease of power. I *can* strike, and when I do, the blow shall be felt. You, Mr. Editor, appear disposed to tickle the ribs of those whose backs you were wont to scourge in former times. Are the disgusting Bats agreeable to your sight? Accursed monsters. I will make no truce with them.

If you hold out the flag of peace towards them, I question not your honesty but your discretion. You have been cautioned not to write so severely. Who uttered that caution? Your friends or your foes? Why the guilty culprits who writhed under the frequently-repeated distilment of your attention. Are you so weak as to be deluded by such treacherous advice? I will not believe it. You are reserving your powers for some more fitting occasion. Remember, while on this subject, what it was that made THE LANCET such an instrument of mighty influence. Remember what it was that gained for the faintest tinkling sound of your weapon the attentive ear of the whole English public! Not only your honesty, Mr. LANCET, but your just and bold severity; and sternness of justice must still be exhibited in your pages, or the brilliant polish which is reflected from the shield of medical reform will be eclipsed for ever.

Such, Sir, is my hint to you on this occasion. A gentle tap on the cheek, to direct your eye towards certain parties, in order that you may show them that you are neither dead nor sleeping, and to tell you that I have a few rods in pickle, which shall make some persons smart, that shall castigate the most corrupt of the BATS, and not only the BATS but their advertisers, their sycophants, their food-supplying caterers, among whom stands foremost that rough-headed Irishman, Michael Ryan. Half Hibernian myself, I use not the term Irishman in an invidious sense; for, from my soul do I love the Irish; and where is the Englishman who does not feel that he and all his children are deeply indebted to the glorious exertions of Irishmen in the cause of liberty? But the virtues of that nation in the cause of reform, make the renegades of that country appear the more detestable and loathsome. Now I have watched Michael—I have tracked his course. I observed him when first he seized the handle of the spade on becoming sexton to Underwood's mausoleum. I have seen how the uncouth bungler has attempted to thrust his clumsy-bladed instrument into the skulls of decayed Yoricks. I have beheld his imbecility with contempt, his impotence with satisfaction, his malignity with horror. Can I correct him? Can I let out the poison which corrupts his blood? I will try. His coat is off; his undergarments are on the ground; his back is bare; the arm is raised; the nine thongs are nine times nine knotted, and a voice loudly exclaims "Mick, beware of

CASTIGATOR."

Scorpion-street, London, Aug. 8, 1832.

THE LANCET.

London, Saturday, August 18, 1832.

IN the *Quarterly Journal of Education*, No. 7, published under the superintendance of the "Society for the Diffusion of Useful Knowledge," there is an article occupying 21 pages, entitled "Recent Improvement of Medical Education."

Struck by this imposing and captivating introduction, we were led to examine the essay with no small degree of curiosity and care, expecting to find thrown together in an agreeable and concentrated form the changes in medical discipline which have produced the "Recent improvement of medical education."

The sequel of our undertaking has been —disappointment. For the essay is ill written, and its contents are miserably arranged, and it is altogether wholly divested of those materials which ought to have been collected under its interesting title.

When it is considered that this paper

has emanated from a committee, or under the sanction of a committee, of which the Lord Chancellor is chairman, and which contains amongst its members five or six physicians and surgeons, besides a large number of individuals who claim a station amongst the *literati* of the country—this paper, if it exhibit any signs of "recent improvement of medical education," must at the same time furnish indubitable testimony that there was no medical education in any part of the united kingdom, during a very short period antecedent to the publication of the article in question.

The pegs on which the writer has thought it his duty to fasten his remarks on the "improvement," are the most curious that could well be devised—namely, two introductory lectures, and a *Cyclopædia* of practical medicine; and then, springing off, like a blind man jumping from a precipice, without knowing where he should fall, or what would be the result of his hap-hazard adventure, he starts off to the "School of Leyden," and concludes by instructing us in the "recent improvement in medical education," by stating, in the very last paragraph of the article, that "an improvement which *will be* effected, and which *will* materially change the whole face of medicine, will arise from a minute investigation of the state of the fluids of the body, and especially of the blood!".

Without questioning the sagacity of this prediction, we may remind the writer, that discoveries *yet to come* have little to do with *recent improvements*. But it is quite clear, that the gentleman who indited this paper, does not understand what is meant by the word "education;" at least, as it relates to scholastic discipline and arrangements.

The author is enamoured of the school of Edinburgh, which, he tells us, "began to be conspicuous, as the reputation of that of Leyden declined." In referring to the deceased teachers in the medical department of the Northern University, he exclaims "HOPE *primus*, MONROE *secundus*, worthy of his father, HOME, HAMILTON, RUTHERFORD, and the venerable and excellent ANDREW DUNCAN, all these have now closed their long and useful lives; but the ardour with which anatomy, botany, and other branches of medicine, are well known to be pursued in Edinburgh, sprung originally from them, and the benefit ought to be had in eternal remembrance." Thus the account of the "recent improvements" is made to embrace a period of time commencing about a century back, and to terminate probably five centuries hence, "when an improvement *will be* effected which will materially change the whole face of medicine,"—a somewhat ingenious description of the doctrines actually taught in modern times in our medical schools!

When this writer speaks so pointedly and boldly of the great renown and excellence of the medical school of Edinburgh, does he know that the curriculum for students in medicine embraced no notice of dissections of the human body until the year 1823? Does he know that up to that period, and even since, the examinations for the medical diploma were far more loosely conducted, were more superficial and more vaguely theoretical, than are the examinations in the same branch of study as at present instituted by the learned bags of rhubarb hall? Enough of this. Edinburgh, at the period to which he has referred, unquestionably boasted of several celebrated teachers of medicine; but the place in reality was a mere manufactory of doctors.

Almost any dunce, ever so great a blockhead, could obtain the diploma, though a few only, by constant and diligent attention in the wards of the Royal Infirmary, accumulated those materials which enabled them to practise medicine, without committing the crime of professional murder. If the writer of the paper in the *Journal of Education* be a medical man, quite certain are we that he is not noted by his friends either for his power of drawing correct inferences or that of making shrewd and accurate observation. England has been literally pestered with "doctors" of Scotch manufacture. When the DUBBERY system was in the height of its glory, the general practitioners of this country were open to daily mortification and insult, and could scarcely attend a patient suffering from a raging tooth, or an aching finger, without being annoyed and insulted by the question,—"Would you object to my having further advice, Sir? Would you object to my sending for Dr. Dub?"—this "Doctor" probably possessing his diploma from one of the Scotch universities, where he had been examined theoretically, and not practically, and never having witnessed a hundred cases of disease in his whole life, while the practitioner thus insulted may have been an aged person, with his brows over shadowed by grey locks, and his mind richly stored with the results of thirty years' experience.

Next, we are informed, "that in proportion as Glasgow and Dublin possessed, and possess, the advantages which were, and are, enjoyed by Edinburgh, the medical schools of those places also flourished and still flourish, while London long stood still —*a mere school for general practitioners or for apothecaries.*" If, to use the elegant and appropriate phraseology of this writer, "London long stood still," with whom rested the fault? Why, surely, with the teachers, the governors of our hospitals, and the heads of our medical and surgical colleges. Are these bodies denounced, then, by this penetrating observer of the "recent improvements of medical education?" Certainly not. For he has omitted all notice of their infamous conduct, with the most marked and guarded silence.

This abandonment of duty in a writer who has attempted to instruct the junior branches of the profession in the circumstances connected with the recent improvements in medical education, is utterly inexcusable, and indicates a deficiency of knowledge, or a want of candour, wholly unpardonable. Instead, however, of the medical school of London having been the dark and odious chaos which he would represent it, as compared with that of Edinburgh, towards the close of the eighteenth century, and the by-gone portion of the nineteenth, it was the only school, with the exception of Dublin, in which anatomy was constantly taught by actual demonstration of the dead body. It contained, in truth, the only schools wherein students could acquire a competent knowledge of their profession; nor can it be denied, that nine-tenths of the Scotch diplomatists who may have acquired success and celebrity in practice, received all the really useful and substantial portions of their knowledge in the medical establishments of London. Young gentlemen did not visit the northern universities with the expectation of acquiring a knowledge of disease, but with the prospect of purchasing—by means of a short residence, a slight application to study, and a heavy expenditure of money—the, to them, important title of "Doctor." If the London schools were actually in the deplorable condition which our author has pronounced them to have been, it appears, from what he afterwards states, that they were quite worthy of their visitors, or their visitors worthy of them; for he says—

> "The London students intended, *for the most part*, for the *inferior walks of the profession*, confined in their circumstances, and almost uneducated, were sent to London by those who could often but ill afford the expense, to obtain, in the course of a few months, all the knowledge of their profession, except what they had gained in a long and servile apprenticeship. Taken from school perhaps at thirteen years of age, and consigned, for seven long years, sometimes to a self sufficient and tyrannical master, such unfortunate youths were kept during all the years when they were ripening into manhood, and when perhaps every feeling of ambition was kindling in their minds,

> in the prison of a back shop in the country, among larger coloured bottles to make up a show, and lesser ones which they were diligently taught how to fill and how to cork. * * * If the master happen to be a *little better than the* GENERALITY, his apprentice had the indulgence of poring over some ancient book of general anatomy, illustrated by a dilapidated skeleton. Thus prepared, he comes, at length, to the first burst of his freedom, from the darkness of his seven years' servitude to the light of the great metropolis."

Surely we trace here both the impotence and the venom of a BAT; but, in order to judge of this person's capability for writing on the subject which he has selected for the exercise of his mental prowess, namely, the "recent improvement of medical education," let us extract a passage in relation to apprenticeships, at page 10:—

> "The *hurtful system of apprenticeships* has undergone much *mitigation*; and time is afforded for education to be acquired at school, and as well as, in many instances, for improvement afterwards during the years of apprenticeship."

The insult offered to the great body of English surgeons in general practice in the first of these extracts, can only be equalled by the gross, the rayless ignorance, which is so prominent in the last. Instead of the "hurtful system of apprenticeships" having "undergone much mitigation" in the "recent improvement of medical education," it is a fact well known to every person at all connected with the practice of medicine, with the exception of the writer in the *Quarterly Journal of Education*, that the system of medical apprenticeships remains precisely as it was instituted by the act of 1815, which, so far from being an improvement upon the old system, makes that servitude compulsory, which before was only adopted in accordance with the capacity of the student, and the disposition of the parent or guardian. The seven years' apprenticeship, or the six years' apprenticeship, or the four years' apprenticeship, was optional up to 1815, but from that period no man has been entitled to his license to practise as an apothecary, without serving an apprenticeship of five years, at least; and this is what our intelligent journalist calls "a *mitigation* of a hurtful system!"

What object the writer had to attain by indulging in the use of such coarse and disrespectful terms towards general practitioners, we cannot possibly divine. Ignorance, we presume, must be pleaded as his excuse in this, as in several other instances of error, vulgarity, and misrepresentation. Students who enjoy the opportunity of passing some years with country practitioners of extensive practice are often better qualified to undertake the duties of physicians or surgeons, than many of those young men who pass several years in sauntering about the squares and passages of our metropolitan hospitals. Few persons can believe the neglect with which pupils are treated in many of the lecturing establishments. In fact, a knowledge of medicine cannot be communicated by lectures, and he alone is conversant with the details of practice, who has witnessed disease at the bedsides of the sick.

But what does our doughty author mean by saying that the London students were intended for the most part for the *inferior* walks of the profession? What, we repeat, does he mean by the term "inferior walks of the profession?" He will not, of course, afford a candid reply to this question, though it is quite evident that he points to the path of duty followed by surgeons in general practice. There is something monstrous in dividing diseases and the treatment of human maladies into *superior* and *inferior* walks,—so monstrous, indeed, that the idea could have had no other origin than the dull brain of a BAT. Intelligence in the science of medicine elevates its possessor to the highest pinnacle of honour. If he descend from such a seat of glory to confer benefits on the poorest members of society, does such conduct deserve to be stigmatised as a movement fitted only to be designated as the "inferior walks of the profession?" There is, however, as much of injustice as of insult in this gross allusion to general practitioners; for their attainments as practitioners in medicine pre-eminently entitle them to the confidence of the public, and to all the respectful attention that can be displayed towards the most essentially-useful members of the community.

A Dub possessing a purchased title, according to the medical nomenclature of this writer, belongs to the higher walks of the profession, while a general practitioner, whose title is his usefulness, belongs to the "inferior walk,—confined in his circumstances, and almost uneducated."

Having, under the sanction of the "Useful Knowledge Committee," withheld all knowledge of the misdeeds of the medical corporations, in relation to the "recent improvements in medical education," such a writer as this of the *Quarterly Journal*, is entitled neither to attention nor to courtesy; and the committee, by presenting to the public such papers as the one we have noticed, have shown but too clearly, that they are entitled to only a very small portion indeed of the confidence of the public.

That direful epidemic the pestilential cholera is spreading its ravages from one extremity of England to the other. Nor is it less fatal in several parts of Ireland. Yet notwithstanding the vast exertions, mental and bodily, of medical practitioners, we are still left to seek for remedies for this awful scourge to the human race. The returns from some parts of the country swell out the catalogue of the afflicted to a prodigious and frightful number, and the mortality continues to progress everywhere in a proportion of rather more than one-third of those who are attacked. But are we to stand aghast and refrain from further exertion, because we have hitherto failed to discover a cure for this distemper? Are we to shut up the senses, to closet the understanding, to refrain from seeing and reflecting on all the circumstances connected with the appearance and course of the disease? Science and humanity aloud forbid it. It is the duty of practitioners in medicine to watch, to investigate, and to publish, the results of their inquiries. Collections of facts bearing upon habits of life, age, temperament, premonitory signs, description of food, situation of dwellings, are all of the highest importance in drawing general conclusions, as to the utility of certain modes of practice. The most trifling symptom should not be overlooked. A shadow of a shade, even, should not be disregarded in such all-important statistical reports.

While we record with apprehension and alarm the dreadful destruction which the cholera still continues to cause in the country, we are enabled to state, from the best sources of information, that its effects in the metropolis are not on the increase, but are declining. Still it is much to be feared that the inhabitants of London have yet to receive a last visit from the pestilence, and, under such a supposition, is it too much to implore all those individuals who command the means of acting like rational and intelligent creatures, to abstain from any practice which may act as an exciting cause of the disease, or predispose the system to the destructive influence of the baneful poison. The avoidance of extremes, as relates to food, drink, and exercise, cannot be too strictly enjoined; for although it is indisputable that many persons apparently in perfect health are attacked by the pestilence, yet in almost numberless instances, it appears to have darted upon individuals whose health had been deteriorated from the normal standard, by excess of some kind or other. In noticing the cases which may fall under their care, we once more entreat surgeons, physicians, and apothecaries, to note down every the minutest symptom which may present itself in the disease, and to record with scrupulous nicety every circumstance connected with the condition of the patient, within a reasonable period preceding the hour of attack.

WESTMINSTER HOSPITAL.

PHLEGMASIA DOLENS IN THE MALE.

Thomas Wicks, a sailor, twenty-three years of age, was admitted under Dr. Bright, 20th of June ult. He has led a seafaring life for nine years. His health was perfectly good until about a half year ago, when he had syphilis in a severe form, and was treated for it in the hospital at Malta. On going back to his ship he caught cold, and was obliged to return to the hospital with pain of chest, cough, and expectoration. He did not completely recover whilst in the Mediterranean, and returned to England two months since, and has been under the care of a medical man at Edmonton. He is a stout, well-formed lad, with a well-developed muscular system, and a plentiful adipose covering, but the entire surface has a pale lardaceous hue. He was treated with blisters and other medicaments whilst at Edmonton, but without much benefit. He complains now of great pain, increased on pressure, and tenderness across the upper part of abdomen. He has had much nausea, and has vomited a considerable quantity of greenish fluid; he has also been much purged. The bowels are open at present; the tongue is furred; pulse 70, full, and soft. Passes freely his urine, which is high-coloured; and his skin is moist and warm. *A calomel and rhubarb bolus at bedtime, and an aperient dose in the morning. A saline mixture every four hours.*

23. The pain and tenderness of abdomen have abated; the tongue is slightly furred; the bowels freely open; no nausea; pulse natural.

26. The pain of bowels has entirely subsided; a dull ache is perceived across the loins. The chest is perfectly healthy; tongue clean; bowels open; micturition free; pulse 82, feeble; appetite reviving; continues the saline mixture.

28. Great tumefaction; pain and tenderness have rapidly come on in the left thigh and leg. The tumefaction is general over the limb, but the pain and soreness are most marked over the course of the femoral vessels. Pulse 90; bowels open; tongue furred. To be cupped to eight ounces on the inside of the thigh, and to continue the saline medicine.

29. The patient declares himself relieved by the abstraction of blood; the swelling of the limb is less, but the pain of thigh is undiminished. The whole limb is extremely tense and sore, and the leg is a little œdematose; pulse full; tongue clean; bowels open. Venesection to twelve ounces, and two tablespoonfuls of the following mixture to be swallowed every four hours. Take of

Antimonial wine, one ounce;
Sulphate of magnesia, one ounce;
Camphor julep, six ounces. Mix.

Fomentations of poppy-heads to be assiduously applied.

30. The blood drawn from the arm is neither buffed nor cupped. The tumefaction and tension have abated, but the pain is undiminished. Pulse 86, full, soft, and regular; tongue clean, and bowels open.

July 1. The pain is relieved by the fomentation. The limb is still swollen; the pain is most acute in the groin, extending upwards along the iliac, and downwards along the femoral vessels; pulse 98. Continues the remedies.

2. Has still pain, especially on moving the limb; he cannot bear pressure over any part of the femoral vein; tongue furred; bowels open; secretion of urine diminished; pulse 100, rather hard; countenance anxious; twenty leeches to be applied to the groin, and subsequently fomentations.

3. Pain not so severe; swelling less; the pain is principally felt in the groin, and in the ham; secretion of urine scanty; pulse 102, feeble; countenance less anxious; bowels open.

4. The pain greatly increased along the course of femoral vessels last night, and was relieved by the application of twenty leeches. There are still pain and soreness of thigh, and particularly in the popliteal space, this last is augmented by any attempt to straighten the limb; tongue flabby, exsanguous, and slightly furred; pulse 100, feeble.

6. No very material alteration. There is no great difference in the size of the two limbs, except in the circumference of the upper part of the thigh, where the affected limb is by much the larger, and at the ankle, where a relative difference of size exists; bowels open thrice daily; stools liquid; urine more copious, and of lighter colour; pulse 104, regular, feeble. Twenty-four leeches to be placed on the inside of the affected thigh.

8. The limb is much the same in appearance. Twenty-four more leeches applied.

9. The patient imagines the pain is increased by the use of fomentations. An

evaporating spirit lotion is consequently substituted.

12. Pain and swelling nearly in *statu quo*. The redness and tension are perhaps a little diminished; the foot and ankle are œdematose; tongue pallid and flabby; bowels freely moved; pulse 100, tolerably full.

14. The pain, redness, and tumefaction, have rapidly diminished since the use of the spirit lotion. Tongue clean; bowels open; pulse 88.

17. The limb is perfectly easy whilst the patient remains quiet, but in the slightest attempt at motion, the pain returns as severely as ever. Tongue clean, but flabby; bowels open; pulse 80, full.

18. The patient was seized with rigors about an hour ago, and still continues shivering. The limb, too, has rapidly swollen since that time, particularly in the groin, and he cannot bear the slightest touch; tongue furred; bowels open once; pulse 120; twelve leeches to the groin; ten grains of Dover's powder exhibited immediately.

Eight o'clock p.m. Pain relieved by the leeches, but the swelling remained unreduced; the pain, however, returning as severe as formerly. Venesection to 16 ounces was had recourse to, and proved effectual in removing the pain.

19. The limb has remained easy since the venesection, but the intumescence has rather increased. Erysipelas has made its appearance in the leg from the foot to the knee, and appears to be occasioned by the leech-bites. Tongue pallid; bowels open once; pulse 102, full and hard. A strong purging draught with alkali to be given; to continue the antimonial mixture.

20. The erysipelas is subsiding; swelling of groin smaller; pain less severe; tongue furred; pulse 80, full and soft; countenance cheerful; to repeat the alkaline purge.

23. The swelling in the thigh, and the erysipelas in the leg, are gradually disappearing; tongue clean, but flabby; bowels open; pulse 84, full. The patient can move the limb without pain. He sleeps well, and his appetite is reviving.

24. A resuscitation of the erysipelas on the dorsum pedis; bowels constipated; the thigh is still rather tumefied, but there is no pain; six leeches to be applied to the foot, and an alkaline purging draught to be swallowed immediately.

Aug. 3. The erysipelas has nearly gone; the tumefaction still unremoved; the pain and inflammation have subsided; the emunctories apparently act well, but the *ensemble* of the patient presents an unhealthy leucophlegmatic character.

10. The patient's general condition has improved; his appetite is good; the pain and tumefaction of thigh-bone abated, and are scarcely appreciable, but the œdema of the leg has very much augmented. The use of the evaporating lotion is continued, and occasional purges are administered; his colour is slightly improved.

This is a case somewhat analogous to those described by Sir Henry Halford at one of the evening converzatione in the College of Physicians. The exciting cause, viz. the sudden application of cold, was the same, but in the present case no remarkable slowness or intermission of pulse has been observed, probably because no obliteration of the veins has taken place, although a suspicion of such a fact is excited by the increased œdema of the affected, and a certain hardness perceptible in the course of the femoral vessels. This is a circumstance, however, which remains to be proved by the subsequent history of the case.

LONDON HOSPITAL.

STRICTURE OF THE URETHRA, PRODUCED BY EXTERNAL VIOLENCE.

H. I., seaman, ætat. 34, was admitted into the hospital about three months since on account of difficulty in voiding his urine. The patient stated, that about ten weeks previous to his admission, he fell across a plank, which passing between his legs, struck the perineum violently, there was considerable hemorrhage from the urethra immediately after the accident, which was arrested by means of cold applied to the part, and he continued for some time to pass blood with his urine. In a short time he found considerable difficulty in passing his urine; this gradually increased. At the time of his admission the urine flowed in a very small spiral stream. There was a very firm stricture at the membranous portion of the urethra.

After considerable difficulty, Mr. Scott succeeded in introducing a catheter (No. 2) through the stricture into the bladder, which was kept in the bladder by tapes, and withdrawn at the expiration of two days, when the next size was introduced, continuing to increase the size every second or third day, each catheter being allowed to remain in the bladder until succeeded by a larger, until No. 12 was introduced with ease, and the man left the hospital with the obstruction apparently quite removed.

FRACTURE OF THE OLECRANON FOLLOWED BY ERYSIPELATOUS INFLAMMATION.

On the 26th May, H. L., a stout muscular man, about thirty-eight years of age, came to this Hospital, and was admitted under the care of Mr. Scott, labouring under fracture of the olecranon, with erysipelatous inflammation of the arm.

He stated that about four days previous to his applying here, while walking on the pavement he fell down, and putting out his arm to save himself, fell with the point of the elbow on the stones; he thought very little of the accident, and continued to follow his employment; the arm was very painful, but not so much so as to cause him to lay by until the day previous to his admission, when it became much inflamed.

On his admission the limb was greatly swollen from just above the elbow-joint to near the wrist; the integuments of the swollen parts were at the same time of a bright-red colour, tense, and when touched excessively painful, there were also red lines extending in the course of the absorbents towards the axilla. Owing to the swollen state of the integuments the fracture was not so distinct, but the crepitus could be felt. The constitutional symptoms were severe; countenance anxious; face flushed; pain in the head; tongue furred; skin hot and dry; pulse 120, full and strong. Was ordered calomel and jalap half a drachm immediately; to take two grains of calomel every six hours, to have 30 leeches to the arm, and afterwards the spirit lotion.

27. Swelling of the arm not at all diminished; the pain is very severe; the inflammation is extending up the arm; bowels freely opened; pain in the head is not so severe; pulse full and strong; tongue less furred.

To repeat the leeches and continue the spirit lotion; continue the calomel.

28. Pain in the head much diminished, and the constitutional symptoms are much relieved, but the pain and tension of the arm are much the same as yesterday. To repeat the leeches, &c.

29. The arm continues very much swollen and painful, but the redness is rather diminished; his bowels are confined, and complains of pain in the head; skin hot and dry; tongue furred; to have a dose of calomel and jalap immediately.

30. Bowels freely opened by the powder; mouth affected by the calomel; the feverish symptoms present yesterday have subsided, but the local symptoms are much the same as on his admission. Mr. Scott considered it a fair case for incision, and consequently made three or four incisions with a scalpel the length of the part inflamed, and directed the arm to be enveloped in flannels wrung out of hot water, after which a linseed-meal poultice to be applied.

31. Passed a much more comfortable night than any since he has been in the hospital: the pain in the arm is much relieved, and the redness on the anterior part of the fore-arm considerably less; the swelling around the back part of the fore-arm, elbow, and lower part of the upper arm, much the same as yesterday; bowels open, tongue clean; pulse soft and compressible; to have three grains of the sulphate of quinine three times a day, and eight ounces of wine.

June 1. Had several hours comfortable sleep during the night; bowels open; tongue clean; the tension and redness of the arm much the same as yesterday. Mr. Scott made two incisions about six inches in length, commencing above the elbow through the inflamed integuments, and directed the arm to be again enveloped in flannels wrung out of hot water. To continue the poultice.

3. The arm decidedly improved; tension, pain, and redness, much diminished; pulse quiet; tongue clean; bowels open; in every respect doing well. To continue the quinine and wine, and to have a pint of porter in addition.

3. Sleeps well, and is free from any unfavourable constitutional symptoms; the arm looks well.

4. In every respect doing well.

6. Continues to improve, and spirits excellent; wished to be allowed to get up; bowels open; tongue clean; pulse good; the limb is nearly of the natural size. Mr. Scott ordered it to be gently rolled. To continue the quinine with the wine and porter.

12. Going on extremely well; appetite good; spirits excellent; bowels open; is allowed to walk about the ward with the arm in a sling.

19. Has continued doing well, and is about to leave the hospital at his own request to attend as an out-patient.

INFLAMMATION OF THE TONGUE.

J. A., weaver, ætat. 29, was admitted on the 29th of July, under the care of Mr. Andrews, labouring under inflammation of the tongue. The tongue is very much hypertrophied, so much so as almost to prevent the patient articulating; has considerable difficulty in deglutition, and there is a constant flow of viscid saliva. The patient has been in this state for about seven days, and cannot assign any reason for the attack; has not taken any mercury, or any other medicine, excepting a dose of salts, which he took the day previous to his applying here. The glands beneath the tongue are not much enlarged; he was ordered a blister to the throat, and to use the following gargle:—

℞ *Myrrhæ* ℥ss;
Aceti ʒij;
Aquæ ℥vj. *Ft. gargarisma*;
Calomel and jalap ℈j *statim.*

The following day he appeared relieved; the blister rose well; there was less salivary discharge; the powder opened his bowels freely.

Aug. 1. Going on well; his bowels not having been relieved, to take a dose of house-medicine.

3rd. The patient suddenly refused to remain in the hospital any longer, and under the pretence of taking a walk in the garden of the hospital, walked himself home. When he left the hospital he was much relieved.

NOTE FROM DR. LATTA.

To the Editor of The Lancet.

Sir,—In the "index" on the cover of The Lancet of August 4th, I was much surprised to see noted, "Quarrel between Drs. Latta and Craigie, of Leith," which, by the *accompanying statement*, is represented as having originated in consequence of Dr. Craigie not mentioning me in his newspaper communications as having first practised venous injection. Now all this is quite a mistake. In the first place, *the quarrel* is not mine, but Dr. Craigie's and Mr. Mitchell's, and originated, not from any complaint on my part, but in consequence of Dr. Craigie applying to a remark made by Mr. Mitchell, the offensive terms of "utterly false." As for my name being coupled with venous injection, that is an honour I have not been remarkably fastidious about. Nay, the good Doctor mentioning me at all in his lucubrations, is what I did not look for, the omission, therefore, could not be the ground of very serious quarrel.

The matter is too insignificant for you to revert to, or you would find I did not write to complain of the want of courtesy on this point, but was compelled to come forward in vindication of Mr. Mitchell's integrity (who, unknown to me, made the remarks in the *Observer* which called forth the Doctor's ire), and in defence of my own conduct and practice, both of which have been misrepresented in Dr. C.'s pamphlet.

I have the honour to be, Sir,
Your most obedient servant,
Thomas Latta, M.D.

Aug. 8, Leith, 1832.

Dr. Ayre's Letter.—Dr. Ayre has requested us to add the following note to *Rosanna Curry's* case, page 616. As it reached us, however, too late to be appended to the case itself, we insert it here: "The unusual quantity of the medicine given in this case arose from two causes; first, from a mistake in intermitting the medicine during some hours when the urgency of the symptoms rendered its continuance most important; and, secondly, from an officious friend giving her some orange, when the disease, which was subsiding, relapsed into all its violence."

CORRESPONDENTS, &c.

"Mr. Editor,—Can you inform me if medical officers of the army and navy, who entered the public service before 1815, are entitled to practise and recover their bills by law, *as apothecaries*, without undergoing any examination at old Rhebarb Hall?"—They are not entitled to practise as apothecaries, and consequently cannot recover the amount of their charges. The "Declaratory Act" of 1822, was passed in order to remedy this gross and flagrant injustice, but as it was enacted for only one year, naval and military surgeons are left in the same unprotected state as they had been in from 1815. The laws relating to practitioners in medicine will never undergo any really beneficial change, except through the interference of the London College of Medicine, and every physician or surgeon who may be the victim of the existing defects in our statutes, becomes, by withholding support from that institution, the voluntary instrument of his oppression, degradation, and poverty.

Jachin is not legally qualified, and could only be admitted on examination.

A Care-worn Student.—This part of our journal does not admit of the insertion of such a lengthened notice as a full reply to his question requires, but if he will obtain a copy of the regulations, to be had at the Company's Hall, he can ascertain all that he wishes to know.

A Medical Pupil.—Such an occurrence is, we imagine, impossible. From very trifling causes, such as the mere shutting of a drawer by slight pressure on the thigh-bone of an aged person, an accident of the kind may take place, but not during "*perfect* quietude."

M. S.—Gregory's "Practice of Physic" is the best elementary system he can study. By-and-by he may read Abercrombie on the Head and Abdomen, and Laennec on the Chest. He may finish with the recondite Mason Good.

Errata.—Dr. Girdwood's paper on Venous Injections, in No. 467.

The paragraph at the end of Case 4, page 595, beginning "Continual vomiting," &c. should be transferred to Case 5, and come in five lines from the bottom, directly after the words "the collapse was very great."

Page 595, col. 2, lines 17, 18, for "and she wasted in flesh," *read* "used in washing flesh." Lines 19, 20, 21, alter the word "all" to "until," and transpose the sentence "No urine was passed until the day after the bleeding," to line 44, and place it after the words "She is now out of danger."

Dr. Girdwood's address is "Paddington," not "Islington."

THE LANCET.

Vol. II.] LONDON, SATURDAY, AUGUST 25. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XLI.

THE NINTH AND TENTH (THE GLOSSO-PHARYNGEAL AND SPINO-VISCERAL MOTOR) NERVES — THEIR ORIGIN, COURSE, AND FUNCTION.

The Lateral Column of the spinal chord appears in view on each side immediately posterior to the pons varolii. It is distinct from the very beginning in the human being, under the name of the *corpus restiforme.* The corpus olivare separates it from the central column on the inferior surface of the medulla oblongata, and is in a manner interposed between the central and the lateral columns, while there is a distinct sulcus on each side, which unites at the base of the corpus olivare, and forms an evident furrow that can be followed along the whole course of the spinal chord. On the superior surface of the chord, and commencing within the fourth ventricle, there is likewise a line of demarcation between the central and lateral columns. There is no distinct corpus olivare in the solipede or the ruminant, or any sulcus on either side of it; but *the lateral column* is as evident, or more so. The sulcus between it and the central column cannot be overlooked; and the rounded head of the column is composed of the corpus olivare, compressed and flattened out, with which the corpus restiforme is blended, and in which it is lost.

The Nerves from the Lateral Column.—From the sulcus between the corpus olivare and restiforme, in the human being, and from the anterior surface of the spinal chord and the front of the lateral column, in the same subject, (but from the side of the rounded head of the lateral column in the quadruped), there arise three distinct sets of nerves which now demand consideration. One portion of the seventh (portio dura), the ninth, or glosso-pharyngeus, and the tenth or cerebro visceral motor nerve. The seventh, with its double root, I will for the present pass over—we are not prepared for its consideration. I will proceed to the ninth.

The Ninth Pair of Nerves, or the Glosso-Pharyngeus.—This has been usually described as a branch of the tenth, or cerebro-visceral, but it is perfectly distinct from it. The glosso-pharyngeus arises from the lateral column of the medulla oblongata, immediately posterior to the inferior root of the seventh. It consists of an uncertain number of filaments, generally four or five, small, arising in a line, approximating and uniting. Professor Mayo says that a small ganglion is formed upon them. Although I had not observed it until it was pointed out by that accurate anatomist, for it is a very small one, I am disposed to acknowledge its existence, and I regard the discovery of it as one of singular importance. This ganglion being formed, the nerve pierces the dura mater separately from the par vagum (as it used to be called, and with which it should never have been confounded), and is seen deep under the angle of the jaw. It passes over the internal carotid, "and runs," as Mr. Percivall well describes it, "forwards and downwards," giving branches to the styloid and digastric muscles—anastomosing with the cerebro-visceral and the accessory nerve—sending fibrils to the constrictor muscles of the pharynx—then dividing into other branches—anastomosing with the lingual nerve, and finally losing itself in the base of the tongue.

The Function of the Glosso Pharyngeus.—Now, Gentlemen, let us pause. We must tread cautiously here. Where do we find this glosso-pharyngeal nerve? On the fore part of the corpus restiforme in the human being, that is on the anterior (inferior) surface of the spinal chord, and therefore at once begetting the suspicion that it has a motor character: but in the quadruped, arising from the side of the head of the column—more equally and decidedly between the motor and sensitive columns of the spinal chord, and occupying a kind of border ground between them. What is its anatomical character? It arises by minute fibrils in a line, resembling a motor nerve; these fibrils pass through, or form, a ganglion like a sensitive nerve; and this ganglionic nerve with a motor origin, is distributed over muscles or parts, some of which are already plainly and plentifully supplied with nervous influence from the cerebral or spinal nerves, while from others this influence is withheld, or bestowed indirectly and sparingly.

The portion of the Nerve belonging to the Tongue.—We call it the glosso-pharyngeal; belonging to the tongue and the pharynx. Now the tongue is plentifully supplied from the linguales and seventh pair,—nerves of voluntary motion,—and the fifth pair, a sensitive nerve. Although this nerve from the lateral column were taken away, there would be scarcely any part of the frame more lavishly endowed with nervous influence. But the muscles of the tongue are connected in a more decided way than many of the voluntary muscles are, with organic life, and with two of its main functions—digestion and respiration.

The Function of this Nerve as connected with Digestion.—The spinal and cerebral motor nerves are under the influence of the will; but before the understanding can be said to have existence, or a volition, the result of some mental act, can be formed, the animal requires nutriment, and is guided to it, and to the reception and the disposal of it, unerringly and perfectly. The foal, the calf, the lamb, seek out the teat, and, with an accuracy on which the experience of after existence cannot improve, extract and swallow the milk. The principle of organic life is in full power, and has been so long ere this; and we shall, perhaps, by-and-by, see reason to conclude, that these nerves from the lateral column are influenced by this principle before the will could issue its mandate, and likewise designed afterwards to call into requisition, and to control and combine the action of the voluntary muscles when the purposes of organic life require it. We are little conscious of the varied influence of the muscles of the tongue in the act of deglutition. The animal acquired the power of performing this organic operation without experience, and without mental exertion; and he continues to perform it, I will not say without an effort of the will, but in perfect unconsciousness of the various steps of the process.

This a Respiratory Nerve as it regards the Tongue.—The tongue is indirectly concerned with respiration;—not much, probably, in undisturbed breathing;—but when respiration is quickened, as in rapid motion, or from any other cause, we plainly see how much the motions of the tongue are influenced, and especially in those animals in which it is only slightly confined by the frænum. The tongue of the horse is somewhat closely tied down by the frænum, that it may constitute a convenient and firm cushion for the bit; but where it is freer, as in the ox, and especially the dog, if the breathing is a little hurried, the tongue is protruded from the mouth, and, in the dog, is in constant motion. The connexion of this with the mechanical act of respiration, has been explained in a former lecture.

Conclusion with regard to the Function of this Nerve.—Then I begin to form some idea of the intention and function of these nerves from the lateral column distributed to the tongue. They are designed to call into action, and to combine together, the muscles of the tongue for the discharge of two important functions, as well before the mind could determine their action, as afterwards, when it had power to do so, but was in a manner unconscious of what took place, and also when, under unusual circumstances, it was necessary to add another and a stronger influence, to produce their more powerful and consentaneous action. I can account for their decidedly motor origin in the human being, and for the rounded head whence they spring being in the inferior surface of the chord in the quadruped; for their function is that of motion. I can account for their sensitive ganglion, for it is necessary that there should be a sympathy and common feeling between all the parts concerned in the discharge of a certain function, in order that their action may be suited to the situation and wants of the animal; and I can understand the anastomoses of these nerves with those of the spinal chord, that this sympathy may be more extensive and stronger, and the consent of numerous muscles in the discharge of the appointed function more complete. I have, therefore, no hesitation in denominating it an *organic motor nerve.*

I must here differ a little from him whose track I have gratefully and cheerfully followed. I cannot call this a *respiratory* nerve, for the tongue is more concerned with *deglutition*—one of the processes of digestion, than it is with respiration. I term it an *organic motor nerve*, not in denial of the theory of Sir Charles Bell, but as comprising an extension of the influence of the nerve to other functions of organic life.

The Portion of the Nerve belonging to the Pharynx.—When you observe the meshwork of nerves about the lower part of the face, and the larynx, and the throat genetally, you can scarcely fail to remark how few, comparatively, from the spinal chord are given to the pharynx. There are two branches from the seventh which have considerable similarity to this glosso-pharyngeus, and which I shall in another lecture have to describe, viz., the *chorda tympani*, which, after performing its duty within the ear, takes its course to the tongue, and another branch which may be traced to the styloid muscles, and the dilators of the pharynx generally: the constrictors, however, seem to be comparatively abandoned, and the fibrils which they derive from the fifth, and the seventh, and the upper cervical, render them voluntary muscles to a comparatively insufficient and imperfect degree. The truth of the matter is, that that portion of the mechanism of deglutition with which the pharynx is concerned is performed instinctively, without the command, and in defiance of the resistance of the will. When a portion of food has entered the pharynx, and dilated it to a certain extent, the fibres of the constrictors contract and press it onward whether we will or not. It is an instinctive act, and performed by the foal and the lamb as perfectly as by the adult animal. The pharyngeal branch of the nerve under consideration, is that by which this portion of the act of deglutition is effected. It is to a certain extent, as every vital action should be, independent of the will, and not accompanied by any indications of common sensation; yet the pharyngeal branch, and the muscles to which it is sent, are connected with nerves of voluntary motion, to force their alliance, and obtain their co-operation, under extraordinary circumstances: and also with those of sensation, to preserve a sympathy between the parts, and a consciousness of their healthy or morbid state of action.

I have dwelled the longer on this first of the nerves derived from the lateral column of the spinal chord, for if I have been enabled to give you some idea of its function, we may pass more rapidly over the others.

You will see still stronger reason for venturing to refuse to this nerve the designation of a *respiratory* one, for it is so only in an imperfect and indirect degree, and under unusual circumstances; and is more properly designated an *organic motor nerve*.

The Tenth or Cerebro-visceral Motor Nerve.—This is composed of fibriculi placed in the same line, and still more posteriorly. They unite,—they form or pass through a small ganglion, and they have a sheath distinct from the glosso-pharyngeal, although they escape with it through the same aperture, the foramen lacerum basis cranii. From its strangely wandering path it has been called by some the *par vagum*, and by others the *pneumo-gastric*, from its being principally concerned with the functions of respiration and digestion. We shall see, however, that it has a great deal more to do,—that it is connected with organic life generally,—that it governs or influences every visceral function—that it seems to preside over the motor or mechanical part of the function, and, therefore, as the only visceral nerve derived *directly* from the spinal chord, I have ventured to withhold from it the titles of respiratory or pneumo-gastric, as indicating only a part of its function, and to term it the *cerebro-visceral motor nerve*.

Its Course.—On escaping from the cranium, we find it becoming connected with every neighbouring nerve,—the great organic, the glosso-pharyngeal, the spinal accessory, the seventh, and the upper cervicals, and forming an intricate plexus of nerves, which we can scarcely unravel in dissection, but for which we can account when we consider the importance of the parts at which we are arriving. Having contributed to form this plexus, it enters into the same sheath with the *great organic*, and in company with, and on the outer side of it, and also distinguished from it by its greater size and firmer consistence, and having also the carotid artery on the inner side of the great organic, it pursues its course down the neck.

The Course of the Nerve continued.—I will endeavour rapidly to trace the course of the trunk of this nerve, and its principal ramifications; and here I would again refer you to the accurate delineation of the nervous system by Mr. Percivall; while those of you who are acquainted with the French language will also study with pleasure the equally accurate, although somewhat differently arranged description of M. Girard. I shall have little to say of the comparative anatomy of this nerve in our different patients. The function seems to be clearly the same in all,—the motions of the various parts connected with organic life are the same,—and the only difference in the nerve will consist in the number and distribution of some of its fibriculi.

Anastomosis with the Great Organic.—The first anastomosis is with the great organic before they are united in the same sheath. Some tell us that it helps *to form* the superior cervical ganglion. I am rather disposed to say, that it receives filaments from the great organic: but, at all events, if our surmise should appear to be well founded, that these are the nerves of organic life, the one the *motor*, and the other the *secretory* and the *nutritive* one (as the nerves of the central columns of the chord being those of animal life, and the one the motor the other the sensitive one), we can well imagine that anastomosis should soon take place between them, and that we should so soon find them in one common sheath, and at length blending together in one grand central plexus.

The Pharyngeal Branch.—This is a very complicated one, and before it is fairly distributed over the pharynx, it has connected itself with the pharyngeal branch of the last nerve, and with a branch likewise from the spinal accessory. The pharynx, although, as I have already stated, comparatively deserted in the distribution of nervous influence from the spinal chord, is richly imbued with organic power, for the act of deglutition is an important and a complicated one: it must be performed, as I have said, long before the mind can influence the action of the body, and it requires the co-operation of many muscles. An attention to what takes place during the act of deglutition, will convince us how difficult it is to arrest the progress of the food when it has once entered the pharynx, and the constrictors are pressing upon it: and, in fact, it is almost impossible to produce the act of deglutition when there is nothing in the pharynx to stimulate the organic nerves to action.

The Œsophageal Branch.—The food having once entered the œsophagus, is delivered over to the influence of the organic nerves; and as the contraction of the fibres of the constrictors of the pharynx forced the food along that canal, so the contraction of the fibres of the œsophagus causes the pellet to descend towards the stomach. The animal has little to do with the force, or even the action of these fibrils. If the pellet is too large, and cannot be driven on by the proper muscles of the œsophagus, even when they are excited to forcible or spasmodic action by the continuance of their natural stimulus, the pressure of the food, no act of the will can avail aught; and mechanical means alone, as the swallowing of some liquid, or the attempted deglutition of air, or, as the last resort, the probang for the human being, and the probang alone in the quadruped, will be effectual in removing the obstruction. In the action of this branch we recognise the organic principle alone.

Arterial Branches.—Next are branches to the carotid, ramifying upon, and winding around it, and forming a complicated plexus, particularly observable in its bifurcation. I will not enter here into the disputed question of the power of the arteries, but I plainly recognise a muscular coat, thin and weak, near the grand central machine, but increasing in thickness and strength as I recede from the heart. It doubtless has its peculiar action, or it would not be found here. What that precise action is, I will not here inquire; but the vessel is indebted for it, be it what it may, to the agency of these nervous branches. No spinal influence can be traced to these vessels, except by the most circuitous anastomosis: it is the organic principle which is here also at work.

The Laryngeal Branch.—The branches to the larynx are very large, and can scarcely fail of being recognised in your dissections. They cross the carotid artery, wind down upon the side of the pharynx, and, in order to reach the larynx, pass through an aperture on each side at the base of the cornu by means of which the hyoid bone articulates with the thyroid cartilage; they then ramify upon those muscles of the larynx, which are concerned in the constriction of the glottis. This branch anastomoses with another derived also from the spino-cerebral, and at which we shall presently arrive, and the fibres of which are principally expended on the dilators.

Course of the Nerve down the Neck.—I now trace the path of the cerebro-visceral down the neck, still in the same sheath with the great organic, and in company with the carotid artery. There are no branches given to the neighbouring parts, for there are none here that are necessary to, or that can be well employed in, the purposes of organic life; but there are almost innumerable filaments of communication between the spino-visceral and the great organic, more closely allying them together, although not identifying them, and preparing them for the completion of the all-important functions which they have soon to discharge. At the bottom of the neck the two nerves separate; and the cerebro-visceral climbs above the carotid artery, and is found between it and the axillary, and, accompanied by them, enters the cavity of the chest between the two first ribs.

The Cerebro-visceral in the Thorax.—The arrangement of the great viscera within the thorax, the inclination of the heart to the left side, and the great bulk of the right lung, give a different direction to the nerve on the opposite sides of the chest. Both are found within the superior mediastinum; but the right nerve continues to follow the trachea; it passes over the root of the right lung, and reaches the under surface of the œsophagus. The left nerve accompanies the anterior aorta, and, crossing the root of the posterior aorta, also reaches the œsophagus; and both of them enter the abdomen attached to the œsophagus. The divisions of this nerve within the thorax deserve close attention.

The Tracheal Plexus.—First there are many branches, united with a still greater number from the sympathetic, entwining round the lower part of the trachea. The transverse muscle has not yet quite ceased to exist; nor the interposed ligamentous muscular band connecting together the rings of the trachea, while there is beside a new and complicated mechanism of cartilage at the commencement of the bronchial tubes, and also considerable motion or change of situation in this part of the respiratory canal during the inflation and subsidence of the lungs. We may consider this plexus, so far at least as this nerve is concerned, as presiding over and regulating all these motions and changes. Some branches are next given off to the heart, but more of these hereafter.

The Recurrent Nerve.—Somewhat differently originating on the opposite sides of the chest, a branch is next given off from the cerebro-visceral, which takes a retrograde course; it escapes from the chest, and climbs up the neck as far as the larynx. From this singularity of direction it is called *the recurrent nerve.* It gives, at its commencement, fibres both to the pulmonary and carotid plexuses, and to the posterior cervical ganglion of the great organic; and then, as it pursues its singular course up the neck, it sends many filaments to the œsophagus and to the trachea. The first seems to be indebted solely to it for the power of its muscular coat; and the latter owes to it the action of the transverse muscle, whether employed in tightening and smoothing the lining membrane of the trachea, in order to facilitate the passage of the air, or in preserving the arch of the wind-pipe from alteration of form, or from laceration. The power of the ligamento-muscular substance interposed between the rings of the trachea, must also be traced to the same source.

Expending itself on the Larynx.—Having reached the upper part of the trachea, the recurrent nerve terminates in ramifications on the dilator muscles of the larynx, and also on the membrane of the glottis. This is a very important branch; the dilators are the muscles most concerned in the adaptation of the larynx to the inspiration and expiration of air in common or in hurried respiration, and also in the production and management of the voice. They are numerous and powerful, while the closing of the laryngeal aperture is effected by the elasticity of the cartilages, assisted by one small muscle alone.

The Explanation of the Retrograde Course of this Nerve.—We can see, then, some reason why the branch which is to accomplish, or to aid in the accomplishment of, the most important division of these processes, should take this singular course. Being given off from the very base of the trachea, climbing up on each side of it, yielding filaments to it at almost every assignable point from its bronchial divarication to its laryngeal termination, and its ultimate fibres diffused over the larynx, there results a sympathy, a co-operation between every part of the respiratory tube best adapted to produce, and which alone can secure, the perfection of this portion of the respiratory function.

The Effect of the Division of the Laryngeal and Recurrent Branches.—When speaking of the respiratory system, and the mechanism of the voice, I described the effects produced by the division of the laryngeal and recurrent branches.* When the laryngeal branch is divided, the voice is changed and rendered hoarse, for the dilators continue to act, but the power of the constrictors is lost. When, on the other hand, the recurrent branch is cut, the dilators have lost their power, the constrictors alone act, the glottis is closed, the voice lost, and the animal threatened with suffocation. The power of deglutition likewise is materially affected by the division of these nerves; when the recurrent nerve is divided, and the constrictors alone act, the glottis is closed, and the food passes over without danger; when the laryngeal is divided, the constrictors cease to act, the dilators expand the glottis—a portion of the food enters the windpipe, and distressing cough, if not suffocation, ensues.

The Pulmonary Plexuses.—These are very remarkable ramifications of the cerebro-visceral nerve. They are given off on each side, both from the main trunk of the nerve and the recurrent branch, and are divided into two portions; the *superior* one is partly given to the pericardium, and entwines around the pulmonic arteries and veins, and accompanies them into the substance of the lungs; the *inferior* plexus is more attached to the bronchiæ and the bronchial vessels, and can be satisfactorily traced along these tubes and vessels to their termination in the lobuli of the lungs.

Function of these Branches.—There have been contradictory accounts of the consequences of dividing the cerebro-visceral nerve, with reference to the function of respiration. In the generality of cases, and when the nerve has been divided about the middle of the neck, the respiration has almost immediately become quickened and laborious, and the animal has shortly died; post-mortem examination has afterwards shown, that the vessels of the lungs are gorged with blood, and the bronchial cells clogged up with a serous or mucous effusion. Although, therefore, I have stated in a former lecture, that the lungs are passive in the double act of respiration, or that their only active power consists in the elasticity of their cellular membrane and air-tubes; that they yield to the pressure of the diaphragm and the ribs in the act of expiration, and, that pressure being removed, the cells and the tubes start again, and resume their previous form and caliber, yet this must be received with some limitation: there is an inherent power in the blood-vessels to hasten the circulation of the blood through them, and also in the bronchial tubes to expel not only the air, but the secretions which they contain. This is derived from the cerebro-visceral nerve, and when the influence of that nerve is removed, the vessels remain filled with blood, and the bronchial tubes with serum. In these respects the cerebro-visceral may be considered as a motor nerve.

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* That lecture (the 18th) was imperfectly drawn up. It was sketched for report under the pressure of severe indisposition, and the revisal of it necessarily committed to those who, however able, could not fully enter into the views of the lecturer.

Different Results of the Division of this Nerve.—There is a very considerable difference, however, in the result of these experiments. When extreme difficulty of breathing has been produced by the division of the nerve, it has been immediately relieved by making an opening into the trachea, showing that a great deal of the disordered function, and perhaps the death of the animal, was produced by the closing of the glottis. The result was also different according to the place in which the divisions was made; if high up in the neck, the effect was striking and satisfactory—if close to the formation of the plexus, the disarrangement of the function was considerably less, and, in some cases, very slight. There is not much difficulty in accounting for this when we consider the frequent anastomoses between the two visceral organic nerves, and the share which the great organic nerve takes in the formation of all those plexuses. If the division is high up, there will be few or no anastomoses, and few or no fibrils of the cerebro-visceral will be brought through the medium of the great organic; if the division is close upon the viscus, the communication through the main trunk may be cut off, but it may be still kept up by means of many anastomosing fibres, through the medium of the great organic. In one instance, the animal lived three days after the division of the nerve, although the division was made in the neck, but there was sadly laborious breathing and frequent vomiting, and at length he died completely exhausted.

The Cardiac Plexus.—The cerebro-visceral nerve furnishes one more considerable, but somewhat mysterious plexus, within the thorax, and that is to the heart. As soon as the nerve on each side has entered the thorax, small branches are directed to the base of the heart, but afterwards, and after the giving off of the pulmonary branches, a very considerable one proceeds from the right nerve towards the heart, and this separating into two as it approaches the base of the heart, is distributed to each auricle, and to the blood-vessels of the two divisions of the heart. Now, Gentlemen, I have very little proof of the agency of this nerve as a motor one as it regards the heart; and comparing the cardiac with the pulmonary plexus, and, by-and-bye, with the gastric one, I find that this organ is sparingly supplied with nervous fibrils, and I cannot help attributing some importance to the circumstance which I have just pointed out to you, that the cardiac plexus is principally made up of ramifications from the *right* nerve alone. I presume not in the present state of veterinary knowledge to explain this; although I can fancy the reason why, viz. that this central and all-important organ is endowed with a contractility of its own, independent of nervous influence, and that the character by which it is distinguished from every other part of the frame, is that of inherent and inexhaustible contractility. Then I can conceive that the influence of this motor nerve shall be little evident, or, in the usual working of the machine, comparatively little felt, yet at all times existing, connecting the motions of the heart, with the changes and the wants of the general system, or the diseases of particular parts, and to such a degree that, in our patients who cannot tell us the situation or the nature of their maladies, the variable character of the pulse, is, although too little studied among us, the most valuable of all the indications of disease. Thus far I can fancy, for I confess, Gentlemen, we have no direct proof, and we are wandering where I seldom dare to venture, on the tempting but dangerous paths of mere theory, that these nerves are here maintaining their character in controlling the action of the heart according to the changing circumstances of health and disease, and even of mental emotion; and when I regard the other supposed power of the cerebro-visceral nerve,—when I look to the sensitive ganglion as well as to the motor origin, I can comprehend a thousand sympathies, and perceive how this central machine is bound up with the system generally, and forms with it one connected whole. In my next lecture we will follow this nerve into the abdomen.

CASE OF
INTUS-SUSCEPTION,
WITHOUT VOMITING, OR CONSTIPATION.

To the Editor of THE LANCET.

THE following case is the first of a series which I have noted down for their deviation from the usual course of disease, and which have occurred in the course of several years' practice in the southern portion of the United States. Medicine has been brought to its present state of advancement by the collection of isolated facts, and every new one, however insignificant, being necessary to the construction of a perfect system of the science, bears with it, in my view, a sufficient apology for presenting it to the notice of the profession. If you concur with me in opinion, I shall be glad to see this case presented to the public through so respectable a means as the pages of THE LANCET.

I am very respectfully, &c.
JOHN GARRIE, M.D.
London, August 20th, 1832.

CASE.

In every medical systematic work, we find it laid down, that vomiting is an early and constant symptom of intus-susception, and that an insuperable constipation is an equally invariable attendant. The occurrence of a case of this disease, in which neither of these symptoms was present, nor any one that, so far as my reading or observation extends, could designate it as volvulus, induces me to present it to the readers of THE LANCET.

On the 3d of October, 1830, Ben, a negro slave, seventeen years of age, of middle stature, athletic frame, and healthy constitution, was attacked by a mild intermittent fever. He was ordered a cathartic of calomel (10 grains) and jalap (20 grains), which operated tolerably freely, and without the occurrence of any unusual circumstance. On the afternoon of the following day he said he was better; but as his tongue was somewhat furred, and his stools had the dark appearance said to indicate bilious derangement, fifteen grains of calomel were administered, and directions left, if they produced no effect on the bowels in four hours, to aid their operation by an ounce of castor oil. Early on the next morning I received a message from his master, desiring me "to return in haste, for he thought Ben was dying." Upon my arrival I found my patient lying on his bed, his breathing a little hurried, his pulse quick and feeble, tongue slightly furred and somewhat reddened, totally unable to swallow, though he could make the usual muscular effort with the pharynx, and his voice sunk into a scarcely audible whisper, and which soon became entirely suppressed. I was informed the calomel and oil had operated four times during the night; the fourth time, a few minutes previous to my arrival, and that he had not complained of pain. Muscular strength was not greatly impaired, for he had got up to stool without assistance, and could change his position in bed. Examining the abdomen, I found it slightly distended, and pressure on the right iliac and umbilical regions excited some evidences of pain. The sensorial functions seemed, from his assent or dissent to the various propositions that were made, for a long time perfect, but together with the other functions of life, gradually declined, and he died at twelve o'clock on the succeeding night, the 5th.

As this was not suspected to be a case of intus-susception, it is unnecessary to detail the few remedial means that were resorted to. It is sufficient to say that enemata were frequently given, and always brought away free dejections.

It may be proper to mention, that during the life of the boy I had attempted, in answer to repeated inquiries on the subject, to account for the anomalous symptoms of his case, by referring them to the presence of worms in the stomach or œsophagus.

Twelve hours after death I opened the body (in the presence of a number of persons) by an incision from the thyroid cartilage to the umbilicus, and thence to each anterior superior spinous process. The view of the abdominal viscera thus presented, exhibited nothing unhealthy in appearance or unnatural in arrangement. After removing the sternum, I opened the stomach, and examined the œsophagus thence to the pharynx; but I found nothing to verify the opinion above expressed. Assured, however, that the cause of his death was to be found, if not in those organs, in some of the abdominal viscera, I undertook to trace the course of the intestines from the stomach to their termination. At about the point at which the ilium and jejunum may be supposed to unite, I found a retrograde intus-susception of about twelve inches in length. The received portion of intestine was easily withdrawn, appeared to be in a highly inflamed state, approaching to gangrene, and to contain some hard particles of food, all which the hurry to bury the body prevented my examining thoroughly.

A mere history of a single anomalous case, without the illustration and explanation it admits, offers little instruction, not simply as to its prevention or cure, but even as to its nature. The physician to whose management a peculiar disease falls, will be more apt to inquire into, and to reflect upon, the causes of its deviation from the usual course of nature, than the less interested reader of its description, and it becomes his province to make the deductions that it seems to warrant. It is, therefore, trusted, that indulgence will be granted to an attempt to develop the peculiarities of this case, and that the following considerations will not be considered inappropriate.

The circumstances that press themselves upon our attention in this case, are, the intus-susception, the inability to swallow, and the aphonia. These phenomena are rendered of difficult explanation from the presence of muscular motion, and an undisturbed sensation—indicating a freedom from spinal and cerebral disease. Deglutition is a voluntary exercise of muscular motion, and must be considered as dependent, for the volition on the brain, and for the motion, on the spinal marrow. The inability to speak manifestly arose from an interruption, of function of the recurrent nerves, branches of the par vagum—a cerebral nerve.

These facts being indisputable, how are we on physiological principles to account for the extraordinary symptoms of this case? The following appears to me a probable explanation:—

From the extent of the received portion of intestine, there can be no doubt that it drew with it some branches of the great sympathetic nerve, and, perhaps, with the engorged state of the receiving portion, pressed on some of the abdominal plexuses, or even the semilunar ganglia, so as to suspend or destroy their nervous energy. This would constitute a source from which irritation might radiate throughout the whole course of the sympathetic, and extend to all its connexions, consequently involving intimately the par vagum. The recurrent nerves receiving their impressions from the deranged state of their trunk, there is no difficulty in accounting for the suspension of their functions. The cause of the dysphagia is less easily explicable. From the muscles of the pharynx being under the patient's will, it is apparent that the obstacle to swallowing must have been in the lower portion of the œsophagus. It is well known that this part receives its nervous supply chiefly from the eighth pair, and that its contraction takes place without the participation of the will. Reasoning, then, *à posteriori*, it is evident that disease of the grand sympathetic must have been the cause of the dysphagia. The operations excited by the enemata, must be attributed to the large part of the intestines below the intus-susception, maintaining its peristaltic action; while the absence of vomiting was probably owing to the paralysis of the stomach, and the portion above the inversion.

ON LIGATURE
OF THE
COMMON CAROTID ARTERY IN EPILEPSY.

By J. R. Preston, *Esq., Surgeon.**

Michael Cox, pensioner, ætat. 25, sanguineous temperament, and muscular. This man has for the last five years been subject to very severe epileptic fits, recurring generally about once a fortnight. He had lived, generally, temperately. Since the first seizure, the epileptic fits have generally recurred without any assignable exciting cause, but have been also occasionally induced by intemperance. He had been frequently bled during the paroxysms, but had not, by his own account, been subjected to any other mode of treatment. There was, in this case, great cerebral congestion—a feature which I conceived essential to every attack of epilepsy, and by preventing which (an object I believed attainable by tying one or both carotid arteries) I hoped to cure the disease.

The operation was performed on the 4th of February. It took up a considerable time, owing to the quantity of blood that flowed upon the slightest incision, obscuring the parts, as well as from the livid colour the patient's face assumed, and the feeling of extreme giddiness, induced by the recumbent position in which he remained during the operation, which obliged me to raise him repeatedly, as I feared from these symptoms, the recurrence of another attack. I found it necessary, at last, to bleed him largely from the arm, after which no farther cause of delay occurred. He had been bled freely the preceding day, also, in consequence of one of these fits. I divided the sheath as near the larynx as possible, that there might be less chance of wounding the external jugular vein. A small vein that crossed the artery was divided, and there was a considerable flow of blood, which alarmed me greatly for the moment; it soon, however, subsided.

The artery was very little disturbed from its lateral connexions, and was secured with a single ligature; the edges of the wound were brought together, and kept in contact with three sutures, over which strips of adhesive plaster and simple dressing were applied; above these a pad of folded cotton was placed, to keep the skin as much as possible in contact with the parts beneath; and finally a roller.

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* From the Trans. of the Calcutta Med. and Phys. Soc. vol. 7.

The patient had taken a dose of salts the day before, which had operated moderately.

On the 4th, 5th, and 6th, there were no constitutional symptoms whatever; there was slight uneasiness in swallowing his spittle, or anything of very small bulk, as it required a greater degree of contraction in the muscles concerned in deglutition.

On the 7th he complained of pain over the left temple; tongue white and dry; skin warm; pulse 72; bowels open freely; the wound was dressed for the second time, and was nearly healed, but there was some swelling and hardness about the cervical glands. He had been drinking the previous night.

Seven leeches were applied to the temples, and he took six drachms of sulphate of magnesia, as a precautionary measure. He was discharged from hospital on the 24th, the wound having been for some time completely healed, except at the single point where the ligature hung out. He attended daily to have it dressed.

The ligature came away on the 5th of March.

April 13. Since the operation was performed, there has been no return of the epileptic attacks, nor any tendency to them. He has experienced also, since that time, a great improvement in his general health and feelings. He has also proved the efficacy of the remedy that has been employed, by very hard drinking, which he and his friends considered a test: it required a large quantity of spirits to make him drunk, and he did not afterwards experience the headache, and gloomy and even horrible feelings, which had previously always followed such an excess. He is now as happy as he was before miserable and wretched.

The following opinions by Dr. Clutterbuck upon the nature of epilepsy, are so much in support of this operation, that I have taken the liberty of quoting them; he considers cases similar to the present, the most unfavourable, and under the ordinary modes of treatment the least likely to be cured.

"Epilepsy (he observes) often makes its attack without any obvious cause, and quite unexpectedly; such are generally the most unfavourable cases in regard to prognosis, as they show a stronger disposition to the disease, so that it is brought on by causes too slight for observation." * * *—Lancet, vol. 12, page 355.

"Upon the whole it may be stated, that the prognosis in epilepsy is unfavourable, few cases comparatively, when confirmed by time, so as to become habitual, being cured by art. The reason is, that in a majority of instances the disease is brought on by causes that cannot be removed."—*Ibid.* p. 358.

"Now you are always to consider epilepsy as a brain affection, and upon this simple ground, that it is merely a disturbance of the function performed by the brain, and by this organ alone."—*Ibid.*

I cannot help expressing a hope that tetanus might be cured by tying both common carotid arteries. In hopeless cases we should, I think, be fully justified in trying such a mode of treatment. I must quote Dr. Clutterbuck again upon this subject.

"Tetanus is necessarily to be considered as a disease of the brain, or cranial portion of the nervous system, though it is possible that it may extend to the spinal chord at the same time.* * Upon the whole you must consider the proximate cause of tetanus to be at least doubtful, but you need not hesitate in referring the disease to the brain as its primary seat."—Lancet.

I beg leave to suggest, as a subject for speculation, what would be the effect of tying the carotid arteries in cases of hydrophobia. The symptoms seem all clearly referable to the brain and nervous system, and have much, if not entirely, the character of undue excitement.

CASES OF

CALCULUS IN THE BLADDER,

Treated by W. B. Costello, *Esq.*

I have refrained from publishing isolated cases of stone in the bladder treated by the new method, from the conviction that the ends of science, and the extension of lithotrity, would be best promoted by exhibiting those cases grouped in such a manner as to enable the medical world to draw general inferences respecting an operation, on the superiority of which over lithotomy, great diversity of opinion still exists amongst sincere and well-informed men. Facts given out singly, and narrated *ad captandum*, may answer very well to attract attention; and this perhaps is as good a way as any for the furtherance of personal interest; but the profession want something more than this; it looks for the elements of conviction and precept, and until facts are so arranged as to answer this purpose, it cannot be satisfied. There is vast variety in the class of calculous diseases; in some (the recent cases particularly) the distress is inconsiderable, and the general health is undisturbed; in others life is wasted by throes of anguish unceasingly recurring, by pains which know no remission; in some the boldest proceedings of the surgeon are crowned with triumphant success; in others the worst results can only be prevented by the most unremitting care, and the most extreme caution. To speak usefully therefore on this subject, these differences in cases must be kept in view. It is only by observing and noting them faithfully, that we shall be able to form just conclusions on a subject in which humanity is so deeply interested.

The following case exhibits the application of lithotrity in an aged person who laboured under a sluggishness of the bladder, amounting almost to paralysis.

CASE I.

Stone in the Bladder; Paralysis, Catarrh, and enlarged Prostate.—Simple Method of evacuating the Fragments.

Mr. Rochet, formerly a merchant in the city of London, had retired from active life in 1825, being then 75 years old. Up to this period, and for many years previous, he had suffered from the voidance of small calculi, varying from the size of a grain of millet seed to that of a French-bean. The larger ones often were retained for some time in the bladder, but from experience he became so confident of his power of expelling even these, that he always refused the use of any means of facilitating this occurrence, contenting himself with occasional large potations of the mucilage of barley-water. By degrees, however, the power of expulsion became more and more feeble, the bladder more irritable; small stones were no longer voided; the urine, which he could no longer expel entirely, became fetid and mucose, and the movements of the stone were now distinctly felt by him. It is not necessary to enter more into the history of his sufferings. He bore them for three years. Seeing, at length, that he could make no terms with his internal foe, he applied for relief.

Six sittings sufficed for relieving him of a stone of the size of a large walnut. The stone was friable and easily reduced, but he had no power of voiding the fragments. Some of these however were occasionally carried out, by being enveloped in the flakes of mucus which were voided with his urine. To assist him in this difficulty, the following simple expedient was resorted to: the round vesical extremity of a large-sized silver catheter was cut clean off below the eyes; a flexible wax bougie, of greater length than the tube, was then passed into it, the rounded end being made to pass beyond the cut extremity of the catheter, so as to form a head or olive. The catheter thus headed by the bougie was easily introduced into the bladder; the bougie being then withdrawn, and the patient being in the erect position, or stooping forward, the *detritus* was carried through the open tube, at first by the urine contained in the bladder, and subsequently by injections of tepid water, which were made to flow out quickly by the combined effects of the distention produced in the organ, and pressure on the hypogastrium. The patient was thus delivered of the last particle of the stone.

The paralysis of the bladder still persisted; the catarrh, though lessened, was still distressing, and the prostate was considerably enlarged. To meet these symptoms, the following treatment was adopted: Cold irrigations of the bladder, rendered in the lapse of a little time more stimulant, together with dry friction on the sacrum and insides of the thighs, were first put in use. The catheter used for the irrigations was of considerable size, chosen with the view of making pressure on the prostate; the rectum, which had sympathised with the bladder while it contained the stone, became tranquil; the bowels were regulated, and exercise in the open air was recommended. Under this treatment the bladder recovered its tone, the catarrh disappeared, and the engorgement of the prostate diminished considerably. On a former occasion I dwelt on the resolution of simple engorgements of the prostate; ample experience has since confirmed the opinions I then maintained on this point. I speak of engorgement of the gland, and not of its bony, fibrous, or other alterations. This gentleman is now an octogenarian in perfect health.

CASE II.

Case of Stone in which Cutting ought never to be resorted to.

On my passage through Liverpool, about two years ago, Mr. Bickersteth confided to me a patient of his who had laboured for a considerable time under symptoms of stone in the bladder. Having ascertained by catheterism that the fact was so, the next day was fixed for the operation. On this occasion several of the most eminent medical men of the town were present, amongst whom were Drs. Trail, Formby, Brandreth, Rutter, Mr. Sheppard, Mr. Battey, Mr. Timewell, &c.

Mr. Pix, the patient, aged sixty-nine years, placed himself on a sofa; the orifice of the urethra, being unusually narrow, was opened by means of the urethratomi. He did not seem to be conscious that this small incision was made. The difference in point of sensibility between the surface of the glans penis and the commencement of the urinary passage is remarkable, and well shown in every case where it becomes necessary to make this incision. The lithotrite was now introduced, and a calculus of the size of a pigeon's egg seized. It was reduced by drilling and crushing. A bath was ordered after the operation. On my return I saw this patient again. He had voided the whole of the calculus, together with portions of several others. These portions did not bear any marks of the action of the drill. They appeared to be fragments of calculi, which had been formed of concentric layers. There was no layer whole, and their surfaces, except that which formed the surface of the stone itself, seemed to be coated with a crust of dried crisp or varnish. I saw similar fragments of calculi which had been removed by cutting by my friend Mr. S. Smith, of Leeds. At Glasgow, also, Mr. Buchanan showed me half of a bean-shaped stone which had become engaged in the membranous part of the urethra, from which he cut it out. In neither case had any violence been used capable of fragmenting the stone. I am inclined to think that this disruption of the stone is spontaneous, and depends in all probability on the fermentable nature of the dried viscid substance which lines and insinuates itself into the substance of the layers. At my second visit Mr. Pix felt relieved from his pains; if they should at any time be renewed, he assured me he would repair to me. Very lately, I had news of this patient, in a letter from Dr. Dobbie of Liverpool. The Doctor expresses himself in the following words:—"About a month ago, I gave advice to an old gentleman, who had been operated upon by you in Liverpool. The operation was so far successful, that he passed the fragments of the stone you reduced; but the kidneys go on to form others, and he has collected about twenty-five small calculi, that have passed from the kidney and along the urethra. There are some calculi now in the bladder, and he passes occasionally a small stone, without pain, or with little pain; indeed, to use the words of Dr. Trail, whom he consulted, *his kidneys and bladder form a complete quarry*; and I doubt from his age and infirm state, even if those in the bladder were removed, which you could easily do, whether his disposition to form new ones could be corrected."

Here, then, is a case in which relief from long suffering was obtained by one application of the lithotrite, of a few minutes' duration, and at the expense of very little pain. The same benefit might now be conferred on him, but the occasion for it is far from being so urgent. In cases where the kidneys have so strong a lithic disposition as to be constantly making stone, the cutting operation ought never to be performed. It can only do at a great risk of life, what lithotrity can accomplish with little of either risk or pain. Neither of them can do more than remove the calculi which happen to be in the bladder at the time. The source from which they come, is not exhausted. Here then is one form of the calculous affection in which the cutting operation must be proscribed.

7, Parliament-street, Westminster,
August 20th, 1832.

CASE OF

MALIGNANT CHOLERA.

By W. Dobson, *Esq., Surgeon, Pimlico.*

Mrs. Tyley, ætat. 36, residing in Harrison Street, Vauxhall Road, a strong, healthy, and temperate woman, in comfortable circumstances, not subject to disordered bowels, was attacked on the 1st of August about an hour after a dinner of beans and bacon, with griping of the bowels, diarrhœa, and nausea, but not vomiting. These symptoms continued progressively increasing until the following morning at five o'clock, when spasms of the legs and abdominal muscles supervened, of the most excruciating kind, with coldness of the surface, first commencing in the extremities, then extending over the trunk. I saw the patient at seven a.m., she was prostrate in bed, and unable to move her hands, the debility was so extreme. The countenance was sunken, and the face of a purple hue; the eyes sunk in their orbits, and had a glazy appearance; the fingers were remarkably shrivelled, and of a dark colour; the surface uniformly cold, "as cold as marble," with the exception of the præcordial region. The cramps had ceased. A gruelly-like fluid of a light colour was passing from the bowels involuntarily; the radial arteries were imperceptible, and the beat of the carotids exceedingly feeble; the tongue cold; the voice had the character so peculiar to cholera.

Every symptom indicated that dissolution was at hand, and would occur unless prompt and energetic measures were adopted. Heat was applied to the surface, and the whole body rubbed constantly with warm turpentine. Large quantities of brandy were given, and *calomel* ℈j with *opium* gr. ij every half hour.

Nine a.m. Some warmth on the surface; cramps had recurred slightly; vomited once; pulse at the wrist just perceptible; bowels still much relaxed. An enema was administered, composed of two ounces of lukewarm starch-gruel and *tinct. opii* ℥ss. The breathing being laborious, nitric acid was freely applied to the cervical portion of the spine; the respiration was instantly excited. The *cal. et opii* to be given every hour.

At eleven still improving; less discoloration of the skin; debility intense. *Sp. ammon. arom.* was given frequently.

The patient was seen every two hours during the day, and a gradual amendment was observable each time.

At ten p.m.. Pulse 80 and small; surface much warmer; complains of irritation of the skin from the turpentine; *cal.* gr. x with *opii* gr. j every three hours; farinaceous substances, with small quantities of brandy, to be taken frequently during the night.

3. In a favourable state; pulse much improved; slight ptyalism; surface warm; thirst, with dry tongue; bowels relaxed; evacuations bilious; powders to be omitted. Ordered a cretaceous mixture.

4. Mouth fully affected with the mercury; great debility; quinine and port wine every four hours. Avoiding too much excitement.

5. Decidedly better. 6. Nearly convalescent. 8. Recovered, with the exception of soreness of the mouth.

MALIGNANT CHOLERA.

SANITARY REGULATIONS

ISSUED BY THE

GOVERNMENT BOARD OF HEALTH,

In August, 1832.

(Circular.)—The Central Board of Health have deemed it advisable to re-issue the sanitary circulars relating to spasmodic cholera, dated respectively 14th November, 13th December 1831, and 9th May 1832, with the following alterations and additions, which in their opinion have been rendered necessary by the presence and gradual spread of the disease in this country since October last.

Council-office, Whitehall,
Aug. 9th, 1832.

PUBLIC PRECAUTIONARY MEASURES.

1. In order to ensure the adoption and realize the benefit of any system of sanitary arrangements in a large community, the first essential point is to divide that community into subordinate sections, and to form local Boards of Health; each board to consist, if possible, of one or more resident magistrates and clergymen, and parish authorities, a certain number of substantial householders, of one or more medical men, and an efficient secretary.[*]

2. These Boards should appoint district inspectors to visit daily, and report upon such deficiencies as may be found to exist in the following primary elements of public health; viz. the food, clothing, and bedding of the poor, the ventilation of their dwellings, space, means of cleanliness, their habits as to temperance,—prevention of panic.

3. It would be most desirable that flannel clothing, at least flannel belts and woollen stockings, should be distributed, and an improved diet afforded, to those who may be found most in need of such supplies, and who are generally the first attacked in all communities; in order that they may the better resist infection, and be enabled to oppose improved strength and more comfortable feelings to the influence of the disease, should they be attacked.

4. The subordinate divisions of districts ought to be numbered or lettered, and the names of the medical men attached to, and the visiting inspectors employed in, each division and district, should be placarded in conspicuous places.

5. Efficient arrangements should be made by the local Boards of Health or other authorities to obtain the earliest and most correct intelligence of every case of cholera which may occur within their jurisdiction, and to report the same to the Central Board in London.

6. All unnecessary communication should be avoided as far as possible between the infected and the healthy; and as space, clealiness, and pure air, are of the most vital consequence, both to the recovery of the sick person and to the safety of those about him, the patient labouring under spasmodic cholera should either be placed in a separate, well-ventilated apartment of his own house, if it afford such accommodation, and be attended by as few persons as the circumstances of his case will admit, or be induced to consent to an immediate removal to the temporary hospital; or the healthy members of the family should be removed to such building as may have been provided for the reception of persons whose circumstances will not afford the advantages at home, of space, air, and separation from the sick.

7. But the Central Board strongly deprecates all measures of coercion, which, when tried upon the continent, have invariably been productive of evil. The best inducements to a prompt acknowledgment of the disease having entered a family, as well as to an early and voluntary separation of the

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[*] When a community supporting its own poor wishes to establish a Board of Health, a public meeting of the rate-payers must be called to nominate the members, a list of whom must be transmitted to this office, under cover, "To the Clerk of the Council," for the purpose of being endowed with the necessary powers by an order of the lords of the council.

sick from the healthy, will always be found in the readiness and efficiency with which public charitable institutions attend to the objects noticed in par. 2. (See p. 655.)

8. It is with much satisfaction that the Board feel themselves authorised to declare, and it will no doubt be highly consolatory to the public to learn, that under proper observances of cleanliness and ventilation, this disease seldom spreads in families, and rarely passes to those about the sick, under such favourable circumstances, unless they happen to be particularly predisposed by intemperance, great exhaustion from fatigue or mental anxiety; indisposition of any kind, more especially bowel complaints.

9. When the circumstances of any population render it probable that a temporary cholera hospital will soon be required, such accommodation upon a limited scale should be prepared at once, lest the want of it should add to the panic, which the first cases, when they actually occur, are but too apt to produce.

10. The situations which the board would recommend for temporary cholera hospitals would be, those most detached, insulated, and thoroughly exposed to free and open air; the description of a house, such as would admit of the most perfect ventilation and cleanliness, and the largest space around the sick.

11. When an individual shall have been attacked with this disease, and placed under the most favourable conditions, as already pointed out, both for the recovery of his own and the safety of the public health;—the room or apartment where he may have been attacked, and from which he may have been removed, should be purified by scrubbing, lime-washing, free ventilation, and fumigation by heated sulphuric acid and common salt, with black oxyde of manganese, or the same acid with nitre; or, when these materials cannot be obtained, by strong vinegar thrown upon heated bricks. The bed, bedding, and clothes, should be immersed in water and washed with soap.

12. To correct all offensive smells, chloride of lime may be applied; but great caution is recommended in the use of this material, its fumes, continued for any length of time, having been found highly prejudicial to health, more particularly in delicate persons.

13. A few steady men, proportionate to the extent of the district in which they are to act, should be appointed to lime-wash and purify, as above recommended (under the direction of medical authority), such apartments as may be pointed out by the inspectors of the local board.

14. Those who die of this disease, should be buried within twenty-four hours, wrapped in cere cloth, or in a sheet or blanket saturated with pitch or coal tar, and be attended to the grave by as few persons as possible.

INDIVIDUAL PRECAUTIONS.

15. *Diet.*—No sudden nor extensive alterations should be made in the usual modes of living. All changes of food, to be useful, indeed, not to be absolutely prejudicial, should tend to render it drier, more nutritive and concentrated. Moderately costive bowels, the almost invariable consequence of a regular, invigorating diet, will be found more conducive to exemption from cholera than an opposite habit, everything tending to relax the bowels unduly, being apt to induce a predisposition to the disease.

16. Avoid, above all things, overloading the stomach. Indigestion, however produced, disposes the body to cholera. If in easy circumstances, take for dinner a moderate quantity of roast meat in preference to boiled, with stale bread or good potato, two glasses of wine with water, or an equivalent of good spirits and water, or of sound porter or ale. Eat garden-stuff and fruit sparingly, and avoid fat luscious meats. In short, whilst under apprehension of cholera, use a dry, nutritive diet, sparing rather than abundant, observe great caution as to eating suppers, for cholera most frequently attacks about midnight, or very early in the morning.

17. Temperance should be most rigidly observed in everything. In short, no means should be neglected which may tend to preserve individual health, for although the neglect of any or all of these cautions would not of itself produce the specific disease called spasmodic cholera, yet such neglect would most assuredly dispose an individual living in an infected atmosphere to be attacked by that disease, who most probably might otherwise have escaped.

18. *Exercise.*—Moderate exercise in the open air, in fine weather, is conducive to health; but the greatest care should be observed by all, more especially by the weakly and the aged, not to carry that exercise to fatigue or profuse perspiration, nor to sit down with wet feet or wet clothes. Indeed the most particular attention should be paid to keeping the feet dry and warm.

19. Whenever aperients may become indispensable, those of a warm aromatic kind in moderate doses, should alone be resorted to, such as two of the pills No. 1, or a teaspoonful of the powder No. 4, taken over night, followed in the morning by the aperent draught No. 7. What is generally understood by salts—viz. Glauber's salts and Epsom salts, as well as other cold purgatives, should not be taken on any account, without the express prescription of

a medical man, when cholera is prevailing at the time.

20. The medical members of the board beg to state, that no specific preventive against cholera is known to exist, and that the drugs hitherto offered with this pretension, in places where the greatest ravages have been caused by this disease, not only did not possess the negative virtue of doing no harm, but were found to be absolutely injurious. The true preventives are, a healthy body, and a cheerful, unruffled mind; but habitual drugging, at all times improper, is to be deprecated in the strongest terms when epidemic disease is present or apprehended.

TREATMENT.

21. No remedy at all approaching the nature of a specific has been as yet discovered for this disease in its more aggravated form. In fact, no one mode of cure can be usefully employed under all the circumstances of any disease. The symptoms, and grades of intensity with which spasmodic cholera makes its attacks, vary with the conditions of the subject; its treatment, therefore, must vary with these grades and conditions.

22. The leading premonitory symptoms are, spontaneous looseness of the bowels, with or without griping pains; flatulence, and distention of the abdomen, or slight spasms of the extremities; apoplectic vertigo, with nausea, lassitude, weakness, or various combinations of these symptoms.

23. *When cholera is prevailing, looseness of bowels, however slight, should on no account be neglected. It is by far the most usual forerunner of the disease, as well as the most important, because, in its various degrees, it constitutes that stage in which life may be most easily saved.* When this premonitory symptom affords time for distinct treatment, it may be checked, if mild, by the draught No. 8, to be repeated every second or third hour, if necessary, for three or four times.

24. But if the purging be severe, a moderate bleeding of ten or twelve ounces, or cupping, or leeches, if there be local pain, should be resorted to, and one of the powders No. 3 should be given forthwith, to be washed down with the draught No. 6. This dose to be repeated every second hour for three or four times, if necessary; the patient being *strictly confined to bed*, with a view to perspiration.

25. A warm-bath for half an hour, followed by rubbing with flannel or flesh brushes; warm fomentations to the belly by means of bladders half filled with hot water, or flannels soaked in hot spiced wine, or in hot spirit and water, will afford much relief.

26. After the diarrhœa shall have been fairly arrested, say for twelve hours, a tablespoonful of castor oil, with ten drops of laudanum, should be given, in an ounce of peppermint or cinnamon water,* or the pills No. 1 over night, to be repeated in the morning, if necessary, with the draught No. 7.

27. When there are cramps, a dessert-spoonful or two of the liniment No. 10 should be assiduously rubbed on the part affected.

28. If there be nausea or sickness, without acute pain at the pit of the stomach, give an emetic of twenty-five or thirty grains of ipecacuanha in half a pint of warm water.

29. When giddiness and pain at the pit of the stomach are present, bleed as above, and give a teaspoonful of the aperient powder No. 4.

30. Let the diet during all these premonitory symptoms consist of light farinaceous preparations—sago, tapioca, panada; chicken broth and tepid drinks to promote perspiration.

FIRST STAGE OF THE ATTACK.

Treatment.

31. When the motions have lost the appearance of feculent matter, and have put on that of rice-water or gruel, with vomiting of similar liquids, spasms, intense thirst, irregular, slow, and weak pulse, give an emetic of half a pint of a solution of common salt, milk-warm, and as strong as it can be made, with a teaspoonful of mustard powder. Place a mustard plaster, No. 11, or apply the hot turpentine fomentation, No. 12, over the whole stomach, belly, and front of the short ribs. Give one of the pills No. 2 every alternate half-hour, and in the intervals one tablespoonful of the mixture No. 9. Let the patient drink cold water or iced water if it can be had, allowing no more than two or three tablespoonfuls at a time, or bits of ice the size of a nut may be swallowed whole, to allay the burning sensation at the pit of the stomach. Let bags or stockings filled with heated bran or sand, or bladders half full of warm water, be placed along the patient's spine or sides, and to his feet. Let him be kept still, if possible, wrapt in warm blankets, but not oppressed with heat or coverings, particularly over the chest and neck.

SECOND STAGE OF THE ATTACK.

32. If, notwithstanding these measures, the patient should appear to be sinking, the pulse becoming weaker, the skin colder,

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* A dose of peppermint or cinnamon water, may be made by rubbing down three or four drops of the essential oil, with half a teaspoonful of sugar, adding two tablespoonfuls of water by degrees.

the breathing more laborious, the individual appearing less anxious about his own situation, then, in addition to the steady application of the measures already recommended, let an injection be administered, consisting of two or three pints of water, as warm as the hand can conveniently bear, with the addition of a wine-glassful of spirits, to be repeated, if thought necessary, at intervals of an hour.

THIRD STAGE.

33. When the pulse at the wrist has ceased, or become almost imperceptible, with coldness of the extremities, and perhaps blueness of the surface, particularly of the lips, hands, and feet; irregular breathing, loss of voice, suppression of urine, ghastly countenance: although under these distressing appearances there is but little room for hope, our exertions should not cease.

34. At this stage of the attack the vomiting and purging will generally have ceased, or at least be much diminished; the belly will be drawn in, and pain, sinking, and death-like oppression, will be felt about the heart.

35. Let the hot-water injection be repeated, with three or four drachms of the tincture of assafœtida, and retained for some minutes by means of a napkin.

36. Let mustard plasters be applied to the inside of the thighs and calves of the legs, in addition to that on the belly, which may be removed to the sides of the chest or back; let the limbs be diligently rubbed with warm cloths; let small quantities of light cordials be given at intervals, such as a teaspoonful of compound tincture of cinnamon, or of aromatic spirit of ammonia, in a teaspoonful of water, and let the treatment ordered for the second stage be continued until the pulse becomes distinctly perceptible at the wrist.

STAGE OF RE-ACTION OR FEVER.

37. When the pulse has begun to rise, and the heat and natural colour to return to the surface, keep the patient perfectly quiet, but let him be carefully watched, for a sudden sinking of the powers of life not unfrequently occurs at this period of the disease. Opiates of all kinds must now be withheld; and wine, brandy, and other stimulants, used very sparingly, and withdrawn altogether as soon as the pulse and heat are steadily re-established; when mild tepid drinks are to be substituted, and the powder No. 5 given every hour, instead of the medicines hitherto used, should the bowels be torpid.

38. Under this treatment a warm copious sweat often breaks out, or a more healthy discharge takes place from the bowels, or some urine is passed, which of all others is the most favourable sign. When such is the case, the patient, with proper care, will often pass into a state of convalescence, without further difficulty or danger.

39. It often happens, however, notwithstanding all our care, that the re-establishment of the pulse and heat are closely followed by symptoms of fever, by some degree of stupor, by great oppression of breathing, by distention and tenderness of the belly, all of which indicate danger.

40. The moment such symptoms appear, bleed from the arm, or from the part most affected, by leeches or cupping, to 10, 12, or 16 ounces, according to the effect produced by the bleeding. Reduce the temperature of the patient's room, give cool drinks, and apply cold wet cloths or pounded ice in bladders to the head, and give the powders No. 5 as already ordered.

41. When convalescence has begun, observe the strictest care as to diet. At this period a full meal has in numerous instances brought on a relapse. Indeed animal food, even in a small quantity, under these critical circumstances, has often been attended with dangerous consequences to those just recovering from cholera. To such, even the mildest articles of food should be given in much smaller quantities, and at shorter intervals than to those in health; and their ordinary diet and habits should be very cautiously resumed.

W. Pym, Chairman.

Pills No. 1.—Each to contain

Blue pill,
Toasted rhubarb, } of each, 2½ grains.
Oil of aniseseeds, 1 drop.

Pills No. 2.

Calomel, 2 grains.
Opium, ⅙ grain.
Camphor, 1½ grain.
Capsicum, 1½ grain in each pill.

Powder No. 3.

Compound powder of kino, 15 grains; or compound powder of ipecacuanha, 10 grs.

Aperient Powder No. 4.

Calcined magnesia,
Rhubarb in powder, } each 2 drachms.
Ginger in ditto, 1 drachm.
A teaspoonful for a dose.

Powder No. 5.

Calomel, 1 grain.
James's powder, 2 grains.
Nitre in powder, 3 grains.
The whole for one dose.

Draught No. 6.

Chalk julep, 5 drachms.
Compound tincture of cardamoms, } each 1½ drachm.
Mindererus spirit, }
Ipecacuanha wine, 30 drops.
Oil of aniseseed, 3 drops.

Draught No. 7.

Tinct. of rhubarb and aloes, } of each 3 or 4
Infusion of cloves, } drachms.

Draught No. 8.

Chalk julep, 6 drachms.
Tincture of catechu, 1 drachm.
Tincture of opium, 25 drops.
Oil of peppermint, 3 drops, first rubbed down with two drachms of sugar.

Mixture No. 9.

Mindererus spirit, 2 ounces.
Mint water, 2 ounces.
Carbonate of ammonia, 1 drachm.
Syrup of ginger, 3 drachms.
Sweet spirit of nitre, 3 drachms.
One tablespoonful for a dose.

Liniment No. 10.

Comp. soap liniment with opium, 8 drachms.
Tincture of cantharides, 2 drachms.

Mustard Plaster No. 11.

Two tablespoonfuls of flour of mustard, with a dessertspoonful of common salt, mixed with water into a thin paste, to be spread upon brown paper, and applied to the part until redness, heat, and soreness are produced.

Turpentine Fomentation No. 12.

A piece of flannel, large enough when doubled into three folds to cover the whole front of the abdomen, tightly wrung out of very hot water, and immediately soaked in spirit of turpentine, to be placed on the belly, and covered with a piece of dry flannel to prevent evaporation; to be left on until a burning sensation is felt.

LITHOTOMY

BY A NATIVE OF INDIA.

THE following memorandum relative to a native lithotomist, is drawn up by G. King, Esq., surgeon, Patna, and appears in the last volume of the *Calcutta Medical Transactions*, just published. "At Poonarai, a village, three coss from Patna, resides an Hujam, of the name of Chukkun; who says that he has performed the operation of lithotomy on thirty-five persons, one only of whom died: in that case the stone was of very large size, and broke into several pieces; the patient died on the third day after the operation. He shows twenty calculi, some of which are of considerable bulk. The instrument he uses, is something like a clumsy-made penknife, and he describes his mode of operating as follows:—The patient being put in a convenient posture and held firmly by several persons; an assistant presses the lower part of the abdomen, in the direction of the pelvis. The operator introduces his fore and middle fingers into the rectum, and crooking them so as to obtain a hold of the stone, he brings it towards the perinæum, till it becomes prominent there. He then makes an incision over the stone, (on one side of the rapha, and about an inch from the anus,) and extracts it. The urine passes through the wound for some days; and the cure is generally completed in from twelve to twenty days. Chukkun acquired his art from his father, and has now taught his son to perform the operation. As this man keeps no register of the cases, his statement may be liable to errors, respecting the number of persons on whom he has operated; and probably a liberal deduction should be made from his account of success, which it is of course his interest to magnify. In fact, it is almost an invariable custom with native operators, to assert that their operations are attended with certain success; and that cures in their hands are infallible. At the same time we must acknowledge, that they evince much cunning and dexterity in assigning reasons for not operating on unfavourable cases. They operate with a view to *cure*, and would not hazard their reputation for the prospect of alleviating continued suffering, when they did not expect ultimate recovery." The subjoined note is added by R. Frith, Esq. "The patient in this case was a native boy, 14 years of age, who had been suffering from symptoms of stone in the bladder. The old operation '*by the gripe*' was performed. The patient's belly was first rubbed with oil, and then the stone was brought down by pressure over the pubis; and being secured by two fingers introduced into the rectum, by which it was drawn strongly against the perinæum, an incision was there made over the stone, with an instrument resembling a small razor, and the stone was extracted without the use of any forceps. No fever followed, nor were there any unfavourable symptoms, and the wound was completely healed in twenty days."

THE LANCET.

London, Saturday, August 25th, 1832.

Stimulated to exertion by the rapid and alarming spread of the cholera in the distant parts of the country, the members of the Central or Government Board of Health have considered it judicious to issue another circular explanatory of the salutary measures which should be resorted to, in order to protect the community from the terrible ravages of this alarming disease. The document itself will be found in this day's Lancet, and we give it entire, in order that benevolent individuals residing in distant parts of the kingdom may reprint and distribute, either the whole, or such portions of it as they may consider best calculated to effect the preservation of the public health.

Having examined it, however, with considerable attention, we may confidently state that its contents have been carefully and judiciously selected; that no part of it is irrelevant to the object held in view; and that the whole is entitled to the most serious consideration which can be bestowed on it by medical practitioners and parochial authorities. The defects, if any, are those of omission rather than of commission, and the chief of these is to be found in the absence of a strong recommendation to the public to resort, in cases of attacks of malignant or spasmodic cholera, to medical gentlemen residing in the vicinage of the patients.

The members of the Central Board of Health ought to know, that when the symptoms of a frightful and mortal disease have made their appearance in the rooms of a quiet dwelling, fortitude forsakes her seat, and judgment her habitual occupation. Alarm for the safety of fathers or mothers, sisters or brothers, seizes the mind, and all becomes bewilderment and confusion. Experience having amply warned this official body of such a disastrous result, we are surprised that they did not hold out to the public the policy, nay, the necessity, of resorting instantly to medical aid. Considering the facility with which this malady disposes of its victims, it is not only excusable and justifiable, but even commendable, to publish such precautionary notices as are to be found in the circular before us, and we are far from reprehending the motive for publishing the prescriptions at the close of the document. Nevertheless, the ages and temperaments of patients are so various, their habits of life are so opposite, their opulence and poverty so extreme, the states of firmness and dread so pointedly different, the pathological conditions frequently so anomalous, that nothing can be more unfortunate, at least in conformity with our view of the subject, than that patients should be subjected to domestic treatment, if in any instance medical aid is at all attainable. Let it not be supposed that we condemn the adoption of those precautionary sanitary measures which are recommended in the circular. Nor should it be inferred that we object to the adoption of the domestic remedies prescribed with a view to check the progress of the premonitory symptoms of the malady. But what we particularly wish to enforce is this, that in every instance of an attack of decided spasmodic cholera, in addition to resorting to the measures prescribed by the Central Board of Health, medical skill should be resorted to as the only rock to which the unhappy patient can cling with any hope of temporary redemption or permanent security.

When the project for founding the London University was first promulgated, the Bats were sorely alarmed at hearing that a "medical department" was included in the scheme. Accustomed to intrigue, habituated in defeating the dictates of humanity, skilled in the art of obstructing the noblest of the sciences, they lost not a moment in directing the machinery of their unholy offices to the furtherance of their immediately-conceived dark designs. "A medical department in the London University!" exclaimed they. "What, then, is to become of the recognised schools, and of our own dear hospitals? We will crush this infant monster on its first appearance. Its primitive shall be its final breath." Warned by the prospect of a triumph in injustice, and looking forward with delight to an anticipated increase of gains, the merciless Bats applied to their brothers, fathers, and uncles, in the notorious college in Lincoln's Inn Fields, where, the subject of Battish grievances having been discussed, it was resolved, that no student should be admitted to an examination for the diploma of the college, unless he produced a certificate of having attended a London hospital for a period of not less than one year, or a provincial recognised hospital for a period of not less than four years. "Here," exclaimed the conspirators, "is at once a shield of protection to ourselves, and an instrument of decided defeat to our opponents,"—that is, to those individuals who would throw down the gates of monopoly, and lay open the fields of science to all unworthy, but nicknamed "deserving," intruders. On one of the junior members of the gang inquiring how such a regulation could work so gloriously for the cause of battism, it was replied by one of the seniors in crime, that as the London University was without a hospital, the regulation would bring nearly the whole of the medical students to those schools in which hospitals formed portions of the establishments.

Experience has verified the truth of this malicious prediction. Considering the facilities and excellence of the instruction, the convenience, comfort, and even elegance of accommodation at the medical department of the London University, as compared with the old recognised schools, we are compelled to admit that the former has not embraced so large a number of medical pupils, as there were well-founded reasons for expecting; and can it be disputed that the foregoing regulation of the College of Surgeons was the barrier which successfully opposed the ingress of students? In point of fact, we have stated this restriction in too favourable a light for the College, as the official order required not only a four years' attendance upon a provincial recognised hospital, but an attendance of six months, in addition, at one of the favoured London hospitals,—thus affecting to balance the value of forty-eight months' experience derived from the hospital of a populous town in the country, by opposing to it a certified attendance of only six months duration in one of their own establishments of the metropolis. If the head became dizzy and the heart sickened, while reflecting on this profligate, abandoned, and shameless scheme propounded by corporate monopoly, still, a recovery of the senses indicated to the student, that as compliance was a legal duty, so would resistance be his professional ruin. Instead, therefore, of resorting to the medical department of the London University, those students who were not acquainted with the merits of the private schools, were compelled to proceed, with reluctant steps, to the recognised schools of the old hospitals, knowing that if they attended there during the delivery of the prescribed courses of lectures, they could claim their examination at the College, by producing a certificate of twelve months' attendance at the hospital immediately connected with the establishment.

Is it not extraordinary that the London University, with such a barricade before its door, has made any progress whatever towards success in its medical department? Is it not surprising that students have been found possessing sufficient intelligence and courage to stem the torrent of abuse which was directed against them? Fortunately for the cause of medical science, the new institution started at once into life, in the full vigour of maturity, and has acquired no other aid to sustain it in its brilliant career, than the talents and industry of its professors, and the honest support of an independent medical press.

Every man in the profession who knew anything of the regulations in the school, saw at once that this by-law of the College of Surgeons was a deed of proscription issued against the medical department of the London University. For how could it be supposed that any number of students could be found who had attended, or who could attend, during *four years*, the surgical practice of a provincial hospital? Added to this monstrous stipulation, was the injunction that there should be a six months' additional attendance on the surgical practice of a London hospital, the University having been at the same time deprived of the advantages of such an eleemosynary establishment.

Strongly evident as was the tendency of the College of Surgeons' regulation, yet the Council of the London University appeared to slumber while the yoke of monopoly was fastening upon their necks. They resorted to no measure to avert the threatened evil, and remained silent while the self-elected junto proclaimed throughout the British dominions that the medical department of the London University should contain only just such a number of students as they might permit to enter the theatres of the institution. Experience is always the best monitor in instances of this description. The Council were warned of the dangers by which they were beset, but they turned a deaf ear to the voice of admonition. At length, stimulated by the insult that had been offered to them, and by the partial defeat of one of the main objects with which the University had been founded—namely, the prosperity of a medical school—they resolved some months ago on appropriating a portion of the funds at their command to the erection and support of a hospital. Delighted beyond expression at this resolve of the Council, the medical students throughout England, despising, as must their young and generous hearts, the system of extortion which has been so long in vogue in the old recognised caves of the Bats, look forward with the utmost anxiety to the opening of this University Hospital; a procedure which both they and we regard as nearly allied to the completion of the medical department in the University of London. The anxiety felt on this subject is very great, and the communications which we receive respecting it are extremely numerous. The Council, however, ought to know that the new hospital is regarded by all as *a cure for the extortion practised at the old "recognised" institutions*, and that it is anticipated by the liberal portion of the public that it will be placed on the footing of the hospitals in Paris, and rendered free to students, only such a sum being demanded as the fee of admission as shall be absolutely essential to the present maintenance of the patients; that demand also ceasing when the funds of the charity have accumulated to an extent which may render them sufficient for all the imperative purposes of the institution.

Observations on the Healthy and Diseased Properties of the Blood. By William Stevens, M. D. London: Murray, 1832. 8vo. pp. 504.

Sur le Sang Humain, &c. &c. Par Prosper Sylvain Denis. Paris, 1829. 8vo. pp. 450.

Journal de Pharmacie (Septembre et Octobre 1831) Essais Diverses. Par M. L. R. Lecanu.

The spirit of pure theory has been as powerful a narcotic to the science of medicine, as the spirit of barleycorn has been to the physical constitution of man. Under the intoxicating influence of theories, the rapeutic knowledge has been diverted from the straightforward path which it is her duty to follow, and sent abroad, reeling, like a drunkard, through the sloughs and quagmires of error. Mishaps the most ludicrous, and stumbles the most indecent, have in consequence been made by her during her erratic and unseemly deviations, tempted, as she has been, out of the narrow way, by every will-o'the-wisp that crossed her footsteps, and it is only by the loud laughter of the surrounding multitude that she has at length been aroused, rendered conscious of her error, and has returned into the high road from which she had wandered. Let our readers mark the application of the allegory as we proceed.

The works now before us are as different in kind and conformation as could be imagined in volumes professing to treat of the same subject. In the second and last, we find the authors recording the results of almost innumerable experiments on the vital fluid, describing the experimental means by which these results were obtained, and stating the bearings of these facts upon pathology, physiology, and therapeutics, with a degree of exactitude, and a precision of reasoning, that admit of no controversy of their opinions. With them everything is made subservient to the grand object of ascertaining positive facts, and of making correct and ligitimate inferences therefrom. They strain nothing, warp nothing, to suit their own hypotheses, but toil with might and main at the accumulation of facts, and proceed in this labour with so subdued and modest a tone, that their exertions almost escape attention.

The character of Dr. Stevens' volume is, we have said, of a different kind. We do not mean, however, to imply, that the "Observations on the Blood," &c. should be spoken of in terms exactly antithetical to those we have used in the praise of the labours of MM. Lecanu and Denis. Dr. Stevens' work, in truth, does its author no ordinary degree of credit. But it is more ingenious than sound, and rather establishes for the author a character for a strong and somewhat original power of conceiving theories, than for discovering solid grounds whereon to erect those theories. It is his predominant fault to be over ready at mere assertion. He reasons far too boldly on some things not *fully* proved, and on others not proved at all. Lecanu and Denis, on the other hand, are perfect in all their proofs, never arguing by assertions, but always wholly discarding vague and insecure speculations.

We proceed in the present article to afford our readers an adequate analysis of Dr. Stevens' work, canvassing freely, as we go, its various merits and defects. We shall be aided in our criticism by the data which the French writers afford, and by the experiments of Dr. O'Shaughnessy to which we alluded in a recent number.

Dr. Stevens' book consists of twelve divisions, besides a dedication and a preface. The third portion is entitled "General Observations on the Blood;" the four following sections treat, respectively, of "Animal heat," of "Respiration," of what the author calls the "Latent power of attraction," and, lastly, of the "Vitality of the blood." To these sections, as far as our limits allow, we shall at once direct our attention.

After a cursory description of the mechanical circulation and general functions of the blood, the author rapidly enumerates the chemical constituents, whether proximate or ultimate, of that fluid. In this the only thing requiring especial notice, is the assertion, that the gas (carbonic acid) which the blood contains, exists therein in a *free* or aërial state, and not in a state of solution. This position is to be closely attended to, as we shall subsequently find that upon it the author hinges the remarkable and singular doctrine that the *globular* structure of the blood, about which so much has been written, is an appearance produced by these *free* and spherical particles of gas mixed with blood, and not by the anatomical conformation of the blood itself. We shall not just now inquire into the validity of this opinion, but reserve its discussion till we arrive at the experiments by which the author supports it.

We next have tabular views of the quantities of the constituents of healthy or standard blood, extracted from Lecanu's Essay, as translated in Dr. O'Shaughnessy's Report, and from a paper by Dr. Clanny, published in this journal. The experiments of the latter, Dr. Stevens deems to be incorrect. We next arrive at some more opinions which are peculiar to the author, and which we shall partly give in his own words.

"The fibrin, the albumen, &c., are naturally solid, and with the exception of the colouring matter, the whole of the solid ingredients of the blood owe their fluidity, not to the presence of a free alkali, but, as I believe, to the circumstance of their being held in solution by a *saline fluid*. While in the circulation, vitality and the incessant motion of the blood tend to prevent the occurrence of those chemical changes that would be fatal to life; but when a part of this fluid is drawn from the system, exposed to the air, and allowed to rest, it loses its living principle, and then those chemical changes are no longer prevented, which are, in reality, the cause of coagulation; consequently it is not the living principle, but the loss of its vitality, which is one cause of the sudden change which the blood undergoes when drawn from the body."—p. 6.

The author distinctly states, in the first sentence, that the fluidity of the albumen and fibrin in the blood, depends on *their being dissolved in a saline fluid*. This is roundly and decidedly expressed, and had the opinion not been qualified in the succeeding sentence, we should have been perplexed to understand how Dr. Stevens could overlook the manifest facts—first, that the blood is just as saline after coagulation as before it; and, secondly, that coagulated albumen may contain just as much water and salts as it did before its coagulation. The truth is, that as yet we are in a state of complete ignorance as to the *cause* either of the fluidity of the albumen, or of its coagulation by heat; and we think that a candid profession of such ignorance, is preferable to the author's attempt to explain these occurrences.

We now arrive at the author's first allusions to the cause of the red colour of arterial blood, and to the various phenomena connected with the changes of colour which the blood undergoes in the circulation. Dr. Stevens commences by stating that when blood first coagulates, it is red throughout, but that when the contraction of the clot is complete, its interior becomes black. He then says, that when a portion of the red clot is immersed in *distilled* (saltless) water, it becomes black; that this blackened portion, placed in a saline fluid, again becomes scarlet; that these changes of colour take place in an atmosphere of carbonic acid; that dry saline matter, such as the muriate of soda, sprinkled on black blood, changes the colour to scarlet; that carbonic acid, and all other acids, blacken scarlet blood, even while it contains salts; and, lastly, that blood becomes red when exposed to air or oxygen gas, *only when it contains saline matter*. This latter statement he grounds chiefly on the experiment he describes of scarlet crassamentum becoming *black* when immersed in *distilled* water, and this black clot changing to red in a saline fluid. He states pointedly, and with emphasis, that the immersion in distilled water deprives the crassamentum of all its salts!

Dr. Stevens then repudiates the idea of free carbon existing in venous blood, and he contends for the existence of carbonic acid in that fluid, a fact which we consider is now placed beyond dispute. He then states that oxygen gas possesses a strong power of removing carbonic acid from the blood, by a species of attraction which he afterwards more minutely describes, and to which we may here appropriately allude. It has been for many years known to practical chemists, that gases confined within membranes, in the course of time effect their escape, and mingle with the surrounding air, while a portion of the external air enters the membrane and supplies the place of the expelled gas. The circumstance of experimental balloons of hydrogen gas descending after some weeks elevation in a class-room, without any apparent diminution of bulk, first led to the knowledge of this interchange of gases through membranous partitions. Several years, too, have elapsed since Dr. Andrew Duncan made similar observations on the aërial contents of preparation bottles, half filled with air, and covered with membrane. It seems, however, pretty clear, that Dr. Stevens was the first who distinctly proved that the oxygen of atmospheric air entered a bladder containing carbonic acid, while the acid escaped, or was removed, through the bladder; and he, in our opinion, was indisputably the first who advanced the opinion that the phenomena of respiration were typified by the interchange of gases thus described.

Thus, it must be conceded, that venous blood contains carbonic acid. Well; in the course of circulation, the blood containing this acid arrives at the expansion of the pulmonary mucous membrane. The carbonic acid then escapes through the membrane, and its place is supplied by oxygen from the atmospheric air. Such is Dr. Stevens's theory concerning the *aërial* changes connected with respiration. When we consider the leading facts he has brought forward in support of this view, and when we bear in mind the admirable manner in which these facts have been examined, experimentally, by Messrs. Mitchell and Faust, of Philadelphia, we cannot but express our belief that, in this respect, the author is correct, and that he deserves our thanks for contributing so importantly to the correction of ancient prejudices respecting the respiratory process.

To revert then to the changes in the colour of the blood, effected during respiration, Dr. Stevens holds that the natural colour of the hematosine (or colouring matter of the blood) is *black*—that it becomes *red* when acted on by *saline matter*—that the reddened hematosine is capable of being *darkened* by all *acids*, including the *carbonic*—and that the carbonic is the only acid which, on its removal, permits the restoration of the red colour. To connect then his theories of respiration and coloration, he would state that the venous *black* blood charged with carbonic *acid*, parts with this when it arrives at the lungs, and receives from the air a quantity of oxygen. The colour then becomes *red*, because the carbonic *acid* is removed, and the salts resume their reddening agency on the black hematosine. But the oxygen, as well as removing the carbonic acid, and thus allowing the blood to be reddened (an event which hydrogen or carburetted hydrogen would equally effect), further answers another great object. It enters the circulation, ready to combine with the carbonaceous ingredients perpetually given off by the changing structures of the body. Carbonic *acid* is thus again formed, heat is evolved, the blood becomes black once more, returns to the lungs, and again undergoes the same process.

Admitting, as we fully do, the truth of the author's views regarding the *aërial exchanges* here described, we are by no means prepared to say that he has *proved* the remaining parts of the hypothesis. Let it be remembered that we merely say, "he has not *proved* them," and that we do not mean thereby to deny the probability of many of the opinions in question.

Of one necessary and essential link in the chain of reasoning, namely, the existence of *free oxygen gas* in *arterial* blood, the author *advances no proof whatever.*

Secondly, when he states that the immersion of the crassamentum in distilled water deprives it of *its* SALINE MATTER, *he is completely in error.* We state it as the result of repeated experiments, that such is not the case, and that the clot thus treated does not lose its saline matter. Dr. Stevens merely *presumes* that it does. We have been convinced that it does not, by direct trial, viz. by macerating the clot after immersion in distilled water, and by weighing the saline matter thus obtained. Thus, in one experiment, 1000 grains of crassamentum, immersed in distilled water for four hours, and then incinerated, gave 8.37 saline matter. A second specimen gave 3.93; a third 3.07. In fact, the crassamentum thus treated yields as much saline matter as it does before the immersion in the distilled water.

Thirdly, Dr. Stevens completely overlooks the fact, that when he speaks of the "*red* crassamentum of recently coagulated healthy blood" he speaks of blood which, in its detraction from the vein, has been aërated—partially arterialized. Let him, for example, open a vein in each arm of a healthy man, receive the blood on one side into an exhausted flask, and on the other in the ordinary way; he will find the crassamentum *black* in the blood received *in vacuò*, and *red* in that received in contact with the air. Let him then immerse the *red* clot in distilled water, and place the vessel in the chamber of an air-pump; as the air is exhausted he will perceive bubbles of gas rising through the water, and, as this happens, the crassamentum becomes *black*. Now this clot does not lose its saline matter, but it loses the aërial impregnation it had received.

Fourthly, If *carbonic acid*, as Dr. Stevens assumes, is the cause of the black colour of the blood, by preventing the action of the salts on the hematosine, and if oxygen only influences the production of the red colour by removing the carbonic *acid* (we beg our readers' attention to this), *whatever removes the carbonic acid, leaving the salts, ought to change the colour of red crassamentum to black.* To investigate this point the following experiment was performed. A portion of healthy black crassamentum was placed in the receiver of an air-pump, in a small vessel communicating with another holding lime-water. *On exhausting the receiver the crassamentum gave out* CARBONIC ACID, *and retained its* BLACK *colour.* Dr. Stevens' position is therefore, in this respect, incorrect. We are also bound to say, that in several experiments performed by transmitting a stream of carbonic acid gas through florid colouring matter, the colour was never even once observed to be darkened.

These facts are amply sufficient to bear us out in our refusal to admit Dr. Stevens' views regarding the effect of carbonic acid on the colour of the blood, whether he alludes to its supposed positive agency in *blackening* saline *red* blood, or to the alleged negative circumstance of its absence permitting the restoration of the *red* colour to *black* blood, by the action of the saline matter.

Setting aside then that part of the theory which relates to the effect of carbonic acid on the colouring matter, let us look at the portion which refers to the effect of the *saline* matter. Every nurse in Europe has for a century been aware that the black blood disgorged by leeches after sanguisuction, becomes beautifully bright when common salt, which contains no oxygen, is thrown upon it. From this experiment, with some unimportant modifications thereof, Dr. Stevens concludes that it is saline matter which strikes the red colour in black blood, independent of the agency of oxygen gas, and which thereby causes the change from black to red in the circulation. But having shown above, that the carbonic acid is not concerned in the black colour of the blood, that its presence does not prevent the red colour, and that its absence does not restore the red colour to black saline hematosine, it is evident that *venous blood* ought to be *red*, if the salts were the cause of the red colour, inasmuch as venous blood contains a large proportion of saline matter. Dr. Stevens seems to have apprehended the possibility of this objection, for at page 19 he observes,

"It has been asked, If the saline impregnation be the cause of its red colour, why is it that the blood has a rich scarlet appearance in the arterial circulation, and a dark or Modena red in the venous, which contains as much saline matter as the arterial blood? Now, in the first place, it is not quite true that the venous contains as much saline matter as the arterial; for, with one exception (the bile), the whole of the secretions derive their salts from the arterial circulation, and the venous blood, for example, which is returned from the kidneys or the gastric organs, must naturally contain less saline matter than the arteries, which supply these organs with blood, from which they derive the whole of the saline matter contained in the secretions. We know also that there is a quantity of saline ingredients carried to the muscles and to most of the other solids, and that this is returned, not by the veins, but by the lymphatic vessels. All this, however, is not enough to account for the great difference in the colour of the two fluids; we must therefore look for some other cause.

"Facts, ascertained by experiments, are, or ought to be, the chief foundation of theory; but, unfortunately, from the present imperfect state of animal chemistry, when minute experiments are made, even by the ablest chemists, the results which are obtained are often essentially different, and in some cases diametrically opposite. It is when we find ourselves entangled in such difficulties as these that we are forced to rest our belief less on the result of actual experiments, and more on those facts which are most in accordance with our own observations; but, above all, on the common sense of the argument at issue."—pp. 19, 20.

Dr. Stevens here first denies on *general*, not on *experimental* grounds, that venous blood contains as much salts as arterial. Here he is directly at variance with the experiments of Denis, to whom, at least, the merit of unrivalled accuracy cannot be denied. It surprises us to observe Dr. Stevens's hasty statement, that the *ablest* chemists differ, and that therefore we must distrust experiments, and confide in common sense. Surely Dr. Stevens ought to know that no chemist of common skill can commit an error in the attempt to estimate the quantity of saline matters in a given proportion of blood. The experiment is so simple that a child might perform it.

The nurse's experiment with the leeches, nevertheless, gives sufficient proof that the *addition* of salt to black blood changes it to a red colour, but it by no means proves that it was the salt which gave that colour. There are more facts than one which strongly tend to prove that, under a multitude of circumstances, water is decomposed, giving rise to the formation of the deutoxide of hydrogen * and other less known compounds. Until this subject is fully investigated, it is premature to conclude peremptorily that the salt was more than an indirect or collateral cause of the change in question; neither are we entitled to infer that the agency of saline matter is the *only* way by which the change from black to red can be accomplished.

Requiring then additional and more unequivocal evidence in those parts of the theory to which we have objected, we remit the subject for the present.

Traité Theorique et Pratique de la Ligature des Artères. Par P. J. Manec, Docteur en Médecine, Premier Prosecteur à l'Amphithéatre Général des Hôpitaux Civils de Paris, &c. &c. &c. Paris, Crochard. 1832.

The treatment of injuries and diseases of arteries is a branch of surgery which requires great promptitude, knowledge, and energy of conduct, on the part of the surgeon. The superiority, in this department, of British surgeons over their continental brethren is unequivocal, and now very generally admitted. This superiority, however, has no earlier date than the time of that extraordinary genius John Hunter, the luminous developments of whose scalpel were so strongly contrasted with the Bœotian obscurity of his pen. The experiments of Mr. Hunter to ascertain the functions of arteries, suggested to our distinguished countryman Dr. Jones the inquiries which he instituted in 1804 and 1805, and contributed materially to the fortunate results of his labours. The demonstrations of Dr. Jones as to the influence of the several kinds of ligatures on the arterial coats were so conclusive, that his views were almost universally adopted, and very generally illustrated in practice in this country. But in France, in consequence probably of the restricted intercourse permitted during the war, the practitioners remained entirely ignorant of the discoveries of Dr. Jones. The first knowledge communicated to the French faculty appears to have proceeded from the superficial work of M. Roux, entitled, "*Parallele de la Chirurgie Anglaise avec la Chirurgie Francaise,*" where he speaks in the following terms:—"The English surgeons profess particular views of the mechanism by which nature proceeds in the obliteration of an artery embraced by ligature. These views have led them to adopt a method of making the ligatures themselves act upon the artery, and thus intercept the passage of the blood in a manner strikingly different from ours. They prefer the circular constriction of an artery to its lateral compression (*aplatissement*), and employ not large flat ligatures like ours, but round ones. They apply one only, or two at most, and both are destined to constringe the artery at the moment of the operation, for they do not use ligatures of reserve. They sometimes practise the section of the artery between two ligatures; but this old process recently revived is far from generally adopted." "To determine justly whether the English method or our own is preferable, it is necessary to compare the results of a certain number of operations equally well done according to the respective processes, and under circumstances as similar as possible."

Although the opinion of Roux, as expressed in his work, was manifestly unfavourable to the English method, his less fastidious countrymen, MM. Beclard and Breschet, repeated and modified in every possible way the experiments of Dr. Jones, and the result of their investigations was to the advantage of small round ligatures; so much so, that Beclard immediately adopted them in his practice. In the course of a short time several distinguished surgeons, and amongst others Dupuytren and Larrey, embraced the new plan; but a few surgeons of eminence, holding important offices, held out against the new doctrine, and still continue to resist conviction.

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* See Hudson Gurney's evidence before the Steam Carriage Committee of the House of Commons. *Mechanics' Magazine.* Also papers by Mr. Kemp of Edinburgh, in the late *Edinburgh Journal of Natural and Geographical Science.*

The appearance of Dr. Manec's work is in this point of view not altogether unimportant. It is a work professedly compiled for the use of students and of young practitioners, amongst whom it is one object of the author's labours to imprint a persuasion of the superior eligibility of the small round ligature.

Dr. Manec will be recollected as a hard-working student, not much distinguished by brilliant talents, and subsequently as a demonstrator, at the "Amphitheatre," noted for the vehemence, if not for the emphasis and impressiveness, of his delivery. He opens his work with a rather lengthy introduction, in which a history is given of the several modes of treatment adopted for the injuries and diseases of the arterial system. A rather minute account is given of the anatomy of the several tunics of the vessels, such as must be familiar to every English student that has ever frequented a lecture-room, but which perhaps may not be supererogatory in a work destined for the instruction of French medical students. The various changes produced by ligature on the state of the vessel are well illustrated by plates. Dr. Manec has himself made numerous experiments to establish the truth of the positions laid down by Dr. Jones, and the result of his researches he thinks likely to contribute to render less uncertain and less dangerous the success of these most important operations of surgery.

"We may divide into two classes the facts necessary to be known by the surgeon who wishes to practise the ligature of the arteries. In the first class are to be arranged those general facts which are applicable to all arteries. Thus it is indispensable that the surgeon should know first what change he should effect in the condition of the artery in order to ensure its occlusion; and secondly, what is the preferable means of obtaining the desired result. In the second class we find those particular facts which vary not only in every artery in the body, but at different points of the same artery.

"Our general facts are modified by certain circumstances, which relate to the volume of the artery, and the state of its walls. A knowledge of these can only be acquired by experiments upon animals, the observations upon man being in general so dissimilar, that exact comparison cannot be made. A profound study of descriptive anatomy, and of the anatomy of regions, can alone give to the practitioner that exact and special knowledge of the relation of each artery to the surrounding parts, which is indispensable to success in operating. Thus our general facts all relate to what follows the application of the ligature, our special and topical facts are necessary to the discovery of the artery we wish to tie.

"The reader will, in the first plate of this work, find represented with fidelity the different aspects and transformations which the coats of an artery present, as well as the clot which is formed in its interior, and its changes from the moment when the ligature is closed till the complete occlusion of the vessel, with the variations which these changes exhibit according to the kind of ligature employed. In the twelve last plates I have endeavoured to show, with exactness, the several arteries of the body susceptible of being tied. The relations of these vessels, with the nerves, the veins, and the muscles which surround them, are displayed, and, at the same time, the form and extent of the incision which I have considered most eligible. I have also given to the trunk, and to the limbs of the patient, the respective positions most advantageous for the performance of the operations."

The author proceeds to state, that the first indication to be fulfilled when we wish to produce the obliteration of an artery, is to suspend completely the passage of the blood in its interior. This result is obtained in many ways. 1st, by the simple flattening of the artery, by pressing together its opposed sides, but without lesion of its tunics; 2nd, by the compression of the vessel, with rupture more or less extended of its inner and middle coats; 3rd, by a uniform constriction of the artery, attended with a clear and regular section of the internal and middle coats. Dr. Manec then enters into a voluminous discussion on these several points, which we shall find more satisfactorily handled in the works of Hodgson, Jones, and Hennen: we shall, however, extract his rather interesting account of the vitalization of the coagulum.

"At the end of eight or ten hours, sometimes a little sooner, sometimes a little later, according to the state of the subject and of the artery, there is formed on the surface of the division of the inner and middle coats an exudation of coagulable lymph, according to certain authors, which unites these parts together; by little and little this matter augments in quantity and consistence almost from its first appearance. The column of blood which beats continually against the obstacle which is opposed to its passage, begins to deposit filaments of fibrin, which are the first rudiments of the clot; these two parts combined, the lymph exuded; and the rudiments of the clot unite, but in a manner so slight, that the least movement of the parts suffices to separate them. This, however, is not the case, when a longer time has been allowed to elapse, twenty-eight or thirty hours for example; then this matter of new formation has considerably augmented in quantity, and increased in its organization; it forms at this period filaments sufficiently tough to hold together the lips of the little wound, which is rarely or never the case before the fifteenth or twentieth hour. At the same time that this organization advances, the clot increases in volume. From the first moment of its appearance, which dates generally from the sixth or from the tenth hour, it is represented by two, three, and sometimes by a great number of little nuclei, which unite into one about the fifteenth or twentieth hour. At this epoch the coagulum does not entirely fill the cavity of the artery; the blood can yet insinuate itself between the surface of the coagulum and the side of the vessel, and is impelled against the cul-de-sac formed by the internal and middle membranes, so that the little clot, though fixed by its base, is constantly agitated by the efforts of the blood.

"This state of things does not last long; the blood deposits every instant new molecules upon the surface of the clot, which is thus made to fill the cavity of the artery; but its development always takes place, first in length and then in thickness. This is proved by what has been already shown, and by what happens when the ligature is placed too near a large collateral branch. In the last case the clot does not acquire the volume necessary to fill the vessel, except to a certain extent, so that in the remainder of its length it is very thin. At other times, and this happens when the collateral branch is still nearer, the clot remains free and floating for its whole length.

"In this state, although the blood reaches to the bottom of the artery, in slipping over the clot, it does not possess at this point sufficient force to detach and carry away with it the unctuous matter which exhales habitually from the internal membrane. On the other hand, this matter, in consequence of the same irritation which the ligature has produced, is secreted more abundantly than in the ordinary state, and is accumulated upon the internal surface of the vessel. It is this bed of unctuous matter which assimilates to the coagulable or plastic lymph of which we have already spoken, and which seems to unite the surface of the clot to the artery. The moment of contact between the clot and the artery is various. In young animals, I have seen it take place between the twentieth and thirtieth hour; but commonly it does not take place till between the thirty-sixth and forty-eighth hour, or even later. At whatever epoch, however, this contact takes place, it effects an immediate adhesion between the two surfaces, like that we have seen, formed between the edges of the inner and middle coats. At first this union is very feeble, but it rapidly acquires a marked consistence. From this moment the unctuous matter is deposited upon the clot, but its exhalation ceases soon after; at least I have never been able to perceive any increase in the thickness of this layer, when its consistence indicated that it had existed six or eight hours. We have already seen how the clot is formed, and that it grows from its centre to its circumference, and we have just seen that the walls of the artery are thickened by the accumulation of unctuous matter upon the internal surface; so that these two parts which grow in opposite directions, the clot from the centre to the circumference, and the sides of the vessel from the circumference to the centre, must infallibly meet sooner or later. The period of their junction presents, as we have seen, numberless varieties, dependent upon a crowd of circumstances, sometimes very difficult to appreciate, and which act sometimes on the clot, sometimes on the artery, and sometimes on both. This coincidence points out to us, that the delay in the union is as often dependent upon constitutional as upon local causes. We find that the general circumstances most favourable to the rapid union of the clot with the artery, are youth and adult ages, sanguineous plethoric temperaments, and inflammatory predispositions, whether inherent in the individual, or excited by the atmosphere."

The author has displayed considerable industry, research, and method, in the compilation of his work; he discusses at length, and with much perspicuity and good sense, the several questions of round, small, flat, and large ligatures, and of the advantages, or rather disadvantages, of Scarpa's intermediate compress; but he has adduced nothing on these points that may not be found in popular English works. The section on torsion is interesting, and as the subject matter is in some respects novel to our medical literature, we shall translate it.

"Of late years it has been proposed to arrest the progress of the blood in the arteries by the *torsion* of these vessels, and without leaving any foreign body either in the wound or upon the vessel. We shall not here occupy ourselves in inquiries as to whom the discovery or revival of this process is due; it is for those whom the question personally interests to justify their own rights: we shall merely say that this manner of operating was proposed nearly at the same time by MM. Amussat, Velpeau, and Thierry, and that those three surgeons each perform it in a different manner. The two first have never recommended its employment except for the arrest of the hemorrhages which follow great operations, or those which follow the wounding of an artery. M. Thierry further proposes its application in the treatment of aneurisms—that is to say, upon an artery that has preserved its continuity, and which shall have been denuded for the purpose. In these cases M. Thierry slips under the body of the vessel a solid instrument, which is afterwards made to act in the twisting of the artery exactly in the same manner as the tourniquet acts in compression during amputations. If the artery is wounded or cut across, as happens after an amputation, M. Thierry seizes it with a spring forceps, of which the thickness is proportioned to that of the artery, and he twists the artery by rotating the forceps upon itself, five or six times for small vessels, and ten or twelve for large ones.

"M. Velpeau uses also a grooved pincers, or the ordinary dissection forceps; he seizes the arterial tube by its extremity, isolates it with ease from the surrounding parts, and seizes it with a second forceps, a little distance from the extremity of the vessel, whilst by means of the first forceps he twists the denuded portion from three to eight times successively, according to the magnitude of the artery.

"M. Amussat employs in this operation two pincers sufficiently amplicated, each supplied with a little mechanism which tends to hold them firmly fixed; with the one he seizes the artery, and draws it out of the wound with a view of separating it from the adjoining veins and nerves. That done, he applies the second forceps, of which the mandibles are round and smooth, upon the artery immediately above the grasp of the first pair of forceps; he presses strongly upon the branches, to destroy the continuity of the middle and inner coats, and when he supposes this division is complete, he pushes the second forceps towards the undenuded part of the artery, in order to fold upon themselves the two divided tunics. Arrived at this point, M. Amussat holds fixed and immoveable the second forceps, distant at from four to six lines from the first, and at the same time twists with the first forceps, five or six times successively, the cellular coat which has resisted the crushing action of the other.

"By these different methods of torsion, the flow of the blood from the artery is immediately arrested, and that by a sort of button which the two inner tunics form after their laceration, if the method of Thierry and Velpeau be adopted. If the method of Amussat should be chosen, the effect is produced by the doubling upon themselves of the divided tunics in the area of the artery. In these two cases the cellular coat, the only one truly twisted, forms below the button or refold already described a sort of *cul-de-sac*, which, by compressing lightly the obstacle opposed to the *sortie* of the blood, entirely prevents the escape of that fluid. If the blood should insinuate itself through the broken coats, the resistance of this *cul-de-sac* is quite adequate to prevent its emancipation.

"From what I have just stated, it results that the torsion of the arteries, when it is properly effected, arrests hemorrhage with certainty. But that is not the true point of view in which we ought to regard it, for if the only object were the immediate suspension of the bleeding, nothing so surely performs this as the ligature, of whatever kind, if skilfully applied. The real question is, are the consequences of torsion less likely to produce consecutive hemorrhage than the best ligatures? It is in this respect that the surgeons who recommend torsion have most insisted upon its advantages. Dr. Amussat, particularly, thinks that the torsion has great advantages over ligatures, because in employing it we have no foreign body in the body; thence it results that inflammation and suppuration scarcely ever occur, and, consequently, the cicatrization of the parts is more rapid than in the case of ligatures. Unhappily these favourable consequences have not been always observed to follow torsion of the arteries. Many surgeons declare they have seen inflammation and suppuration extend, and last a longer while after amputation, when torsion has been resorted to, than when, in cases nearly similar, recourse has been had to the ligature. Others cite instances of hemorrhage occurring immediately after the operation, so that the hopes which this process has given rise to ought to be well considered.

"M. Velpeau, who was one of the first surgeons who employed this process, and whose profound medical erudition is universally appreciated, acknowledges now, that the torsion is not eligible in all cases, and that in none has it any real advantage over the ligature. I have never put in practice this hemostatic measure upon the human subject, but I have often made experiments upon dogs and rabbits, in order to compare it with the ligature. In these experiments, I have constantly observed that, after the torsion upon large or medium arteries, the inflammation and suppuration have been stronger, and longer continued, than when the ligature has been placed upon an artery under similar circumstances; and I think it is easy to account for the fact. First, the manipulation is longer, more difficult, and more painful in the use of torsion than in the use of the ligature. If it is desired to practise torsion upon the continuity of an artery, it is necessary to take the same precautions to discover it that we do in applying the ligature. In the last case, however, as soon as the vessel is discovered, we have only to isolate it for a little extent, in order to surround it with a ligature, and the operation is terminated. On the contrary, if it is proposed to make the torsion, the artery being laid bare, there remain several long and painful manœuvres to perform, such as the cutting of the artery with a cutting instrument, its insulation, and the torsion of the two ends which result from the section. It is right to say that no person has ever insisted upon the torsion, in cases where the vessel has preserved its continuity.

"Let us inquire, therefore, whether, after amputations, or the excision of tumours, this method is more easy to practise than that of the ligature. In both processes, it is necessary to seize and to insulate the vessel. For the ligature, the insulation is rendered sufficient by a light traction exerted upon the artery; on the contrary, if it is desired to make torsion, independent of what it is necessary to do for the ligature, it becomes necessary to exert a stronger traction on the artery, in order that it may project five or six lines from the flesh; and as by this effort the veins and nerves are always drawn with the artery, it becomes necessary to separate them, in order to twist the artery only: a precaution which it is scarcely ever necessary to take with the ligature, and which is in no case so long necessary to be continued as for the torsion. When the insulation is complete, it yet remains to make the torsion.

"From what we have just stated it results, that the torsion is an operation more tedious, more difficult, and more painful to perform, than the ligature: there is no exception to this rule, except in the torsion of little arteries. On the other side, if the most accomplished practitioners admit, and, as we think, it has been demonstrated with sufficient detail, that, *cæteris paribus*, the chances of success are greatest when there exists a considerable space between the place where the blood is arrested, and the giving of a large collateral vessel, than when the said distance is short, we must necessarily conclude that the torsion is less admissible than the ligature, since it requires the destruction of a greater length of the coats of the vessel, and that it so far diminishes the strength and length of the coagulum. For example, if the femoral artery were wounded at the distance of two inches below the profunda, a ligature placed above the bleeding point would leave between it and the origin of the great collateral branch, an interval of *twenty* or *twenty-two* lines, which would suffice for the formation of a clot. But if, in the same case, torsion were made, a space of fifteen or eighteen lines would be all that would remain for the development of the clot. If the wound were still nearer the collateral branch, the objection acquires more force, for we have seen that the clot will not form to an extent sufficient to fill the artery, unless there exist a distance of from *eight* to *ten* lines between the ligature and the first branch emanating on the side of the heart.

"Let us examine if the immediate inconveniences we have just mentioned, as attendant upon the torsion of the arteries, are compensated by the advantages which the performance of this operation presents. Far from that. We have already seen that the inflammation and the suppuration are always as long-continued, and sometimes more so, after the torsion than after the ligature. The causes of these accidents are easy of deduction. The first is the length of the operation; the second, one of the most serious, is due to the displacement of the artery in its sheath, in exercising the traction necessary for its insulation; the third is due to the presence of a true *foreign body*, which is more instrumental perhaps in exciting inflammation, than the simple thread which is used in making ligatures. The foreign body is constituted by the tumified and bruised end of the artery, and by the fragments of its inner and middle tunics. These parts of the artery, violently bruised by the torsion, have no vital communication with the tissues which surround them; so that no adhesion can be contracted with them, and they cannot be removed in consequence, except by the double effect of absorption and suppuration. Until this disappearance is complete, the fragments of the arterial walls support an inflammation and a suppuration as complete as the knot of the ligature before its expulsion. And I declare that every time I have placed the ligature upon the principal artery of a limb in any animal, and used the torsion upon the same artery of the opposite member, I have almost always seen the wound cicatrise more promptly upon the limb where the artery had been tied, than upon that where it had been twisted.

"The torsion practised upon little arteries, has not the same inconveniences as when applied to the larger ones; first, because the process is less operose, and, in the second place, because the arteries of this order are not placed in a peculiar distinct sheath, as we have seen the larger ones are. Finally, the term of the artery when twisted, has but small volume; and in order to ensure the stoppage of the hemorrhage, it is only necessary to make three or four twists. It follows that the life of the part may yet exist, and that this part of the vessel, in becoming a foreign body, does not become the cause of a suppurative inflammation, as we have seen it do when torsion has been practised upon a large artery.

"These considerations, as well as those which precede, lead me to the conviction, whilst acknowledging the merit of the surgeons who have proposed and practised the torsion of arteries, and who have enriched science with a new method of arresting hemorrhage, that we ought to resort to this measure with the utmost reserve, and never upon large arteries."

The author makes many judicious observations upon the treatment of hemorrhage from ossified arteries, and cites the practice of Boyer, Dupuytren, Larrey, Scarpa, and Roux. We regret we have not space for a quotation. His remarks on the treatment of wounded arteries in general, evince much good sense, if not originality, and are well worth the perusal of the medical bookworm. At the conclusion of the book, a distinct summary of the author's opinions and corollaries is afforded to the reader. There are twelve lithographic plates tolerably well executed, but they are decidedly inferior, both in drawing and colouring, to those of our countrymen, Dermott and Webb. The effect is, consequently, very frequently deceptive, the arteries appearing much more superficial than they are in the subject. The dissections also are represented as a great deal too clean, no idea being conveyed of the cellular tissue, veins, and glands, which are to be encountered in the operations. Notwithstanding these faults, the work would make a useful companion in the dissecting-room, and its cheapness places it within reach of every student.

MADAME DE BARRY'S BEQUEST OF HER BODY.

It is but a short time since that one of our highest literary characters terminated a long career, devoted to the spread of useful knowledge, and the propagation of liberal principles, by directing that his body should be dissected after death; thus contributing at the same time to the interests of science, and still more by his example, to the extinction of a strong national prejudice. The example thus set by Bentham has just been followed by a French lady. The name of Madame de Barry will be long remembered and respected by all who can duly appreciate her self-devotion, or who can admire the spirit which, rejecting popular prejudice and early impressions, loved to be useful even in death. We have been favoured with a few particulars connected with the disposal of this lady's body, by a gentleman who assisted at the dissection, and think them not unworthy of public attention.

About the month of April last, Dr. Macartney, Professor of Anatomy and Surgery in the University of Dublin, received a communication from Madame de Barry, the substance of which was, that she wished to bequeath her body to him for anatomical examination. The worthy Professor, ever ready to act as high priest on occasions like the present, waited on the lady, and, as we understand, drew up a testamental instrument in such a form, that the body must be delivered to the Professor of Anatomy, or the property of the deceased would pass from the hands of her surviving relatives. On the death of Madame de Barry, this codicil was found fully to answer the purpose intended; for although five or six different wills were discovered, the eccentric, but high-minded woman, had carefully appended the clause of appropriation to each of them, directing therein that her body should be carried to the anatomical theatre by four poor men, to whom she bequeathed ten shillings each; that it should be dissected by the Professor of Anatomy, and that her skeleton should be prepared and preserved in the Anatomical Museum.

We understand that her skeleton is now being prepared, and that Dr. Macartney proposes to place her heart in an urn beside that of Dr. O'Connor, who has disposed of his body in a similar manner.

This, we believe, is the first example of a woman having bequeathed her body for anatomical examination.

LETTER FROM DR. ELLIOTSON.

To the Editor of The Lancet.

Sir,—In your last number, I observe a letter from Mr. Jewel, in which he mentions my name very handsomely, but remarks that, in my recommendation of a remedy, originally proposed by him, "a little more courtesy might have been expected from a gentleman, &c., upon the introduction of the subject to the notice of his clinical class."

Being altogether unconscious of any want of courtesy, I have referred to the report of my lecture, and found my words were, "It was originally recommended by a surgeon of the name of Jewel, and in consequence of his recommendation I have frequently employed it."

I can discover no want of courtesy in this; and, at least, I can assure Mr. Jewel that I intended none. My clinical lectures are seldom premeditated; and frequently, as was the case in the present instance, I do not know what I am to speak of till I open the hospital books on the lecture table. Finding a woman admitted with leucorrhœa had left my wards cured, I spoke of her case; and, lest the pupils should think the remedy successfully employed had not been suggested to me by another practitioner, I informed them that, for my knowledge of its utility, I was indebted to a surgeon, and that his name was Jewel.

Nothing is more likely than that, in addressing *pupils* at an easy, fugitive, clinical lecture,—young gentlemen who have not yet had time to become acquainted with the names and histories of the numerous great authors in our profession, I might, when speaking of membranes, *incidentally* tell them, that the best work upon the subject was by a "French physician of the name of Bichat;" or, when speaking *incidentally* of the discovery of the circulation, that it was made by "a physician of the name of Harvey."

Those of Mr. Jewel's friends who best know me, will feel assured that I could have intended no discourtesy, more especially since, from my never having had the honour of seeing that gentleman, or of having the most indirect communication with him, there could not have been the slightest provocation.

Although I *never* reply to mere attacks, I shall *always* be happy to do my best immediately to remove an *erroneous* impression of intentional incivility or unkindness from any one who complains of me.

I have the honour to be, Sir,
Your obedient humble servant,
John Elliotson.

37, Conduit-street, Hanover-square,
August 18, 1832.

THE ANATOMY BILL.

To the Editor of The Lancet.

Sir,—A clause in the new Anatomy Bill directs that "each body, after undergoing anatomical examination, be decently interred," &c., "and that a certificate of the interment of such a body shall be transmitted to the inspector of the district within six weeks after the day on which such body was received." Now let me observe, Mr. Editor, that if this act be honestly carried into effect (and an act of Parliament is of no use unless it be), the procuration of skeletons will be at once completely prevented, saying nothing about the impossibility of preserving bloodvessel preparations, which by-the-by are certainly not so necessary as skeletons, when there is plenty of dissection. Surely we were all asleep when we allowed this to become law, without objecting to it. There should, at all events, be some clear mode left open for procuring skeletons, which there does not at present seem to be now, and without it the profession, and even the welfare of mankind at large, will be in a desperate condition; for how necessary is it for both students and practitioners to have an abundant collection of bones of their own, not only for the purpose of making and keeping themselves familiar with osteology, but also with the nature and method of reducing fractures and dislocations! I remain, Sir,

Your obedient servant,
G. D. Dermott.

Theatre of Anatomy and Surgery, Westminster
Dispensary, Gerrard Street, Aug. 20th.

*** Whatever may be the defects of the Anatomy Bill, it must be quite obvious to the readers of this Journal, that The Lancet is in no degree accountable for them. We opposed the measure because we discovered its inadequacy for gaining the object in view. The supporters of the Bill were undoubtedly actuated by good motives, but their knowledge of the subject was, from the outset, too evidently, lamentably indifferent.—Ed. L.

TREATMENT OF THE
MALIGNANT CHOLERA
AT LEEDS.

To the Editor of THE LANCET.

SIR,—Having been deputed by the Board of Health of Skipton to visit the towns of Leeds and York, for the purpose of witnessing the effect of the different remedies employed in the treatment of cholera, you will perhaps permit me to occupy a small part of your ensuing number with a summary of my observations and inquiries.

The injection of a saline solution into the veins, as recommended by Dr. Latta, had been extensively tried in Leeds, and I regret to state that it had not been found to answer the sanguine expectations which its first announcement created. In all cases it has certainly afforded wonderful temporary relief, but in none did any permanent benefit accrue. The medical gentlemen had consequently been reluctantly compelled to abandon the plan. Mr. Teale, surgeon of the Holbeck Cholera Hospital, assured me that he should not think himself justified in again having recourse to it, unless it should prove far more successful in the hands of others than it had done in his own. In this opinion I fully concur, and I was therefore not a little surprised to observe in the last LANCET the cases of Dr. Girdwood, and your comments thereon. Allowing these to have assumed a very malignant character, and to have recovered solely from the use of the injection, still I should scarcely think they would warrant the general adoption, or, I should rather say, revival of this remedy, when such a long list of failures can be opposed to them. Very few will, I believe, be able to agree with Dr. Girdwood, that the operation, should it do no good, will not, at least (if properly performed), do any harm. If this were the case, what could induce Dr. Anderson, R.N., to state that he should in future be disposed to abstract blood from one arm at the same time that he introduced the injection by the other, were it not that his experience had taught him the great danger of producing congestion of some internal organ?

The treatment which was then pursued was nearly similar in both towns. When a patient was admitted in the collapsed stage, tins containing hot water, and suitably shaped, were applied to the abdomen, back, and feet, the body being previously surrounded with a blanket. These retain the heat for a long time, and had been found to answer better than any kind of bath in restoring the natural temperature. Should the prostration of strength be extreme, re-action was assisted by the internal administration of strong stimulants, consisting chiefly of ammonia and camphor. A scruple of calomel, uncombined with any other medicine, was then given every hour, until the severity of the symptoms had abated. The quantity was then lessened, and the interval between each dose increased. Six or eight doses had been generally sufficient to arrest the vomiting and purging, and to excite the biliary secretion. It had rarely been necessary to push the mercury to salivation, although I think it would be advisable gently to touch the mouth, with the view of lessening the consecutive fever. Mr. Morley, the resident surgeon of the hospital in Marsh-lane, Leeds, gave in addition to the above the saline draughts and enemas advised by Dr. Stevens; but he had not then had sufficient experience to enable him to form an opinion of their value. This was prior to the exposure of the cases in the Coldbath-fields prison.

At the York Cholera Hospital, re-action was also promoted by the powerful enema of the spirits of turpentine in some thick mucilaginous medium, as much as six ounces, being (in extreme cases) thrown up at once. This was retained in the manner suggested by Dr. Clanny, of Sunderland, for a length of time, proportioned to the symptoms, and had been found to be very serviceable in restraining the excessive and exhausting evacuations. Dr. Goldie and Mr. Anderson, the medical officers of the institution, spoke very favourably of its effects, and I certainly think it worthy of a more extensive trial.

Mr. Teale told me that in some there had been no preliminary diarrhœa, and in a few he had noticed the diabetic state mentioned to him, I understand, by Dr. O'Shaughnessy. He had found also the consecutive fever more difficult to manage than the stage of collapse, and he avoided, therefore, as much as possible giving opium, in consequence of its tendency to produce a determination to the head.

The cases in Leeds had all occurred in low, damp, filthy, and confined situations, where malaria would be expected to prevail, indeed a line might be drawn round the infected district beyond which the disease had not then extended. As many as eight cases had taken place in one house, five of which had terminated fatally. The majority of the medical gentlemen were, consequently, non-contagionists. The same remarks, with a few exceptions, will apply to York. I remain, Sir,

Your very obedient servant,
RICHARD BIRTWHISTLE,
M.R.C.S.L., and As.-SURG. R.N.
Skipton in Craven, August 20, 1832.

ACUPUNCTURATION. INSERTION OF THE NEEDLE IN A JOINT.—Note by Mr. Brett on Acupuncturation, for the cure of chronic rheumatism in natives of India, dated March 22, 1831. I embrace the present opportunity of adding my testimony to the many already on record, of the great advantages to be derived from acupuncturation in cases of *chronic rheumatism,* having employed this remedy with much success in the cases of natives, who had been suffering long and severely from the above disease. The cure or alleviation of the complaint is so general and so prompt, that I cannot hesitate to recommend its frequent trial; knowing as I do, from experiments on my own person, that its application is, comparatively speaking, void of pain: and from having myself obtained by this remedy a cure from severe suffering. In my own case the needles were inserted to the depth of an inch and a quarter, and allowed to remain in for two hours. Dr. Elliotson, in his admirable clinical lecture in the 327th Number of THE LANCET, page 337, observes, "of course you would not think of introducing a needle into a joint." With every respect for so eminent a physician, I think, from a knowledge of the safety with which loose cartilaginous substances are extracted from joints, we need not hesitate to introduce a needle for the purpose of curing protracted chronic rheumatism, adopting the precaution, before introducing the needle, of drawing the skin aside so as to prevent a direct communication with the joint after the needle is withdrawn. In one instance I have successfully treated chronic rheumatism of the knee-joint, by introducing a needle in this manner into the joint, and without any apparent evil consequences — *Trans. of the Calcutta Med. and Phys. Soc., vol.* 7.

⁂ Dr. Elliotson was too cautious a practitioner to recommend the insertion of needles into joints, in a lecture delivered to pupils, especially if he had no facts on which to ground such a proposition, and still more especially since he advised that the needle should always be left in the part for one or two hours,—the operation, too, generally requiring repetition, often several times. The statement of Mr. Brett is not without interest in the treatment of chronic rheumatism by acupuncturation, and, doubtless, when the fact of the innocence and utility of acupuncturing joints is established on the experience of others, and confirmed in his own practice, Dr. Elliotson will take a suitable opportunity to recommend it.—ED. L.

LETTER FROM MR. HART.

To the Editor of THE LANCET.

SIR,—Appended to Dr. Hancock's letter, in your Journal of the 18th inst., is a note containing a most illiberal, and certainly unjust, reflection on the "inconsistencies" (?) of certain medical writers, and of one gentleman in particular, who has lately set forth "experiments," which are termed the results of cholera cases, treated chiefly with corrosive sublimate and acetate of lead. Now, as these remarks evidently apply to me, I beg you will do me the justice to insert the following:—In the first place I may observe, that the cases above alluded to were communicated to the Board of Health not as mere experiments, but as the result of considerable practice, which has established my firm conviction of the decided success to be derived from the treatment of cholera with mercury. To remove the charge of utter ignorance of the action of remedies imputed to me, it is only necessary to say, that with the publishing and consequent heading of the article (upon which Dr. Hancock grounds his charge), I had nothing to do. Furthermore, I will observe that out of forty-seven cases of malignant cholera, treated after the same manner, only nine have died! That in no one instance has excessive ptyalism been produced by the corrosive sublimate, neither have I met with a single case of consecutive fever, after the abatement of the primary disease. These are facts, and, with the exception of Hubbard's case, no one has taken the additional quantity of opium. I have been induced to make the above observations, in the hope that, should any one feel inclined to make a proper trial of them, they will not be found *entirely useless* remedies. I am, &c.,

F. J. HART.

TO CORRESPONDENTS.

A Constant Reader.—Under the present arrangements, a two years' sessional residence at Glasgow is indispensable. A large portion of the lectures is, of course, attended during this period, but certificates of lectures delivered in other universities and recognised schools, are admitted in part, as claims for the examination.

Amicus' letter shall be inserted and noticed next week.

Junius was squeezed out.

Mr. L. S. T. More than sixty such applications have been made to us within the last two months. Each has equal claims on our attention. Thus circumstanced, we are totally precluded from lending the columns of this Journal to the purposes of individual charity.

The Crown has appointed Dr. Traill of Liverpool to the vacant chair of Materia Medica in the University of Edinburgh.

THE LANCET.

Vol. II.] LONDON, SATURDAY, SEPTEMBER 1. [1831-32.

LECTURES ON VETERINARY MEDICINE,

DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XLII.

THE GASTRIC, HEPATIC, AND SPLENIC PLEXUSES OF THE TENTH PAIR OF NERVES.—THE CEREBRO-VISCERAL, THE MOTOR NERVE OF THE STOMACH.—THE STOMACH OF THE HORSE AND THE RUMINANT.—THE SPINAL ACCESSORY NERVE.—THE PHRENIC NERVE.

We had traced the cerebro-visceral nerve to the œsophagus, where it was about to pierce the diaphragm and to enter the abdomen. The right nerve was attached to the under surface of the gullet inclining to the right, and the other branch had found its way to the upper surface inclining to the left. The left branch is the larger of the two. Their fibres of intercommunication here increase in number. I cannot say that they form a plexus, although it has been so described; but there are more frequent anastomoses between their branches, in order, it would seem, that the diffusion and connexion of their influence should be complete before they leave the thoracic cavity. I cannot trace any branches to the diaphragm. The œsophagus passes obliquely through the grand scissure between the crura of the diaphragm, separated from, and not attached to, that muscle by cellular substance, and these nerves accompany it, clinging to its external coat, and enveloped in the same substance.

The Gastric Plexuses.—The œsophagus has no sooner entered the abdomen, than these branches of inter-communication become more numerous, and a real plexus is formed about the base of the œsophagus, through which, however, the two nerves can be traced distinctly. They gradually separate more from each other, and before it actually reaches the stomach the left nerve divides into two branches.

The Ramifications of the Left Nerve.—The one spreads over the upper part of the stomach, sending ramifications in every direction; several reach the cardiac orifice; others pursue their radiating course over the smaller curvature of the stomach; and there they meet and anastomose freely with ramifications from the right nerve, which had been distributed over the same portion of the viscus: so that there is communication of the fibres and blending of the influence of both nerves for some important purpose. The other division of the left nerve crosses above the stomach towards the left; it pursues the course of the gastric artery, and reaches and contributes to form the great semilunar ganglion. I have said that the left nerve, even before its entrance into the abdomen, was larger than the right; the difference of size is more evident within the abdominal cavity; and the reason of this superiority of bulk in the left nerve is now sufficiently plain, for it has an additional and a most important duty to discharge.

The Ramifications of the Right Nerve.—The right nerve runs on to the stomach, and distributes many filaments about the cardiac orifice, and then divides into several branches; some go to the smaller curvature, where, as I have said, they anastomose with ramifications from the left nerve; others supply the greater curvature; they spread over the under part of the stomach; they give branches which can be traced to the spleen, and others which reach and entwine round the pyloric orifice, and they are not lost until they have travelled far down the duodenum. A considerable branch has previously been sent to the liver; and amidst all these ramifications the nerve terminates, and is seen no more. The French might, therefore, with some propriety, call it the pneumo-gastric, for it is given to the stomach and its dependencies as well as to the lungs; it is far more a gastric than a pulmonary nerve: but it is connected directly with all the thoracic viscera, and with those of the abdomen, as far as the duodenum, and with all the rest by means of its communication with the semilunar ganglion.

It is an Organic Nerve.—We do it injustice when we merely term it a *respiratory* nerve, or when we consider all its ramifications, and all the viscera on which it bestows its influence, as merely subsidiary to the function of respiration. I have called it the cerebro-visceral *motor* nerve, and we will now inquire how far this title is applicable to it within the abdomen.

The Progress of Experiments on the Influence of the Cerebro-Visceral Nerve.—The earliest experiments that were made on the influence of the cerebro-visceral nerve had reference to the function of respiration. Whether the mere branch of this nerve, the recurrent or the main trunk of the nerve (and these were generally confounded), was divided, there resulted manifest disturbance in the respiratory function, and, sooner or later, death. This, as the most evident and palpable effect of the division of the nerve, engrossed almost the sole attention of physiologists. Its influence on the action of the heart (circulation and respiration are functions nearly connected) was next inquired into; but this was an organ *sui generis;* it was endowed with an inherent principle of contractility: if supplied with blood or even with warm fluid, it would beat on, although this nerve was divided; nay, although the organ itself was removed from the body. At length the influence of this nerve on the process of digestion became the subject of inquiry. This is a slow process; there is nothing about it that at once arrests the attention; it is merely the gradual change of the food into a uniform pulpy mass. I will not detain you, Gentlemen, with a narration of the numerous experiments instituted by physiologists with reference to this function, many of them ingenious; others without rhyme or reason: some apparently leading us on to a satisfactory conclusion; and others strangely inconclusive and contradictory: some conducted with a spirit of philosophic humanity; and others with a disgraceful recklessness of animal suffering: but I will very briefly give you the ultimate result.

The Influence of the Cerebro Visceral Nerve in the Process of Digestion.—When the cerebro-visceral nerve is divided, and not only so, but a portion of it excised, so that the nervous influence shall be completely cut off from the stomach, digestion, or the solution or chymification of the food is not suspended but only delayed. Those portions which were in contact with the villous coat of the stomach become dissolved; but those that are a little within the mass of ingesta, and out of the reach of the gastric juice, are unchanged. M. Magendie divided the cerebro-visceral nerves in the thorax and immediately above the diaphragm. All disturbance in respiration which might possibly produce derangement of digestion was thus avoided, and the true influence of the nerve on the functions of the stomach would be seen. He says, that after having allowed a proper period to elapse, the substances were *chymified*, and they furnished afterwards an abundance of *chyle*.

MM. Breschet and Edwards instituted numerous experiments to ascertain the kind and degree of nervous influence in the process of digestion, and they arrived at this conclusion, which may now, I believe, be taken as a physiological fact; that the gastric juice is the solvent principle or agent, and that the function of digestion is performed and completed by the different portions of food being successively brought into contact with this fluid. We are not yet prepared for the inquiry into the power or influence by means of which the gastric juice is secreted, but we can no longer doubt that it is by the stimulus of the par vagum on the muscles of the stomach, that the elementary mass is brought into repeated and sufficient contact with this juice.

The Muscular Coat of the Stomach.—I need not tell you that the second or central coat of the stomach is a muscular one, and of singular construction, consisting of two layers of fibres, intersecting each other; the one small and few in number, running longitudinally, and the other large and numumerous, running circularly. You may plainly perceive how these fibres are multiplied and strengthened around the cardiac orifice, so as to form not precisely a sphincter muscle, nor a valve, but a strong, and, in the horse, an almost insuperable obstacle to the regurgitation of the food. At the pyloric orifice they are also increased in number and strength, constituting to the required extent, and, as it regards solid matter, a true sphincter muscle. These muscles effect some important purpose, or they would not be placed around the stomach and so curiously arranged there, and they are supplied by the gastric portion of the cerebro-visceral nerve alone. While the filaments from the semilunar ganglion, diffused likewise over the stomach, pass with the arteries through the coats of this viscus, and are lost in its interior surface, the cerebro-visceral is seen pursuing its course between the peritoneal and muscular parietes, and evidently expending and losing itself in the muscular coat.

Illustrations of the Influence of the Cerebro-Visceral Nerve on the Stomach. It prevents Vomiting.—The œsophagus is not inserted into the stomach of the horse vertically, nor does it at once penetrate the parietes; but it runs obliquely between the muscular and cuticular coats for some distance, so that the contraction of the muscles at the base of the œsophagus, or around the cardiac orifice, will obliterate, as it were, the canal, and render the regurgitation of the food almost impossible. Every contraction also of the circular fibres of the stomach generally, and particularly of those about the cardiac region, unite in producing the same effect, and preventing the act of vomiting, a liability to which would be inconvenient in a slave, the utmost exertion of whose powers are required at every call of necessity, or freak of caprice, in sickness or in health, or whether the stomach is full or void.

The difficulty of vomiting in the horse, depends not upon any construction of the velum palati, or posterior part of the mouth, but simply upon the ureter-like manner in which the œsophagus enters the stomach, and the power of the muscular fibres at the base of the œsophagus, or in the parietes of the stomach, to diminish or obliterate the canal.

It forces the Food from the Cuticular Division of the Stomach.—The left portion of the stomach of the horse—the lesser curvature, is lined by a cuticular coat. No gastric juice is secreted there; but the food is detained as in a kind of reservoir, and undergoes a process of maceration, by which it is better prepared for the action of the true solvent fluid. It is by the contraction of the circular and longitudinal fibres of the muscular coat of this part of the viscus, that the food, as soon as it is thus prepared, is forced out of the reservoir into the true digestive stomach, and the cerebro-visceral nerve is the agent in producing this contraction.

Keeps it in Contact with the Mucous Coat.—When the food is brought into the larger curvature, be it in a greater or less quantity, all the muscular fibres contract, and keep it in approximation with the mucous coat, and consequently in contact with, and imbibing the gastric juice; and that as perfectly when it is nearly empty, as when distended to the utmost.

Exposes every Portion of it to the Action of the Gastric Juice.—The separate parts of the muscular coat have the power of successively contracting and relaxing, and thus in the language of Dr. Bostock, in his invaluable "System of Physiology," and which should find a place in every veterinary, as well as medical student's library. "The successive contraction of each part of the stomach, by producing a series of folds and wrinkles, serves to agitate the alimentary mass, and by bringing every part of it in its turn to the surface, to expose it to the influence of the gastric juice; while, at the same time, the whole of the contents are gradually propelled forwards from the orifice which is connected with the œsophagus, to that by which they are discharged."

Office of the Pylorus.—Last of all, the food, more or less dissolved, reaches the pyloric orifice, which is furnished not only with a valvular fold, but with a sphincter muscle, in order to resist, when occasion requires, this onward pressure, and to return the alimentary mass until the chymification is complete. The mechanism of this outlet is not perfect, nor its function thoroughly performed in the horse; for his stomach, compared with his size, is strangely small. It was designedly made so, that it might not press too much on the diaphragm, or painfully or fatally interfere with the process of respiration, when his utmost energies were occasionally taxed immediately after he had been fed. The food, therefore, remains in his stomach but a little while; and it passes through the pylorus very imperfectly digested; while liquids flow through it as soon as they are drunk.

The Function of the Duodenum.—This, however, is fully counterbalanced by the increased size, and muscular and villous coats of the *duodenum*. In other animals, this portion of intestine has been considered as devoted entirely to chylification; but the previous process—the conversion of the food into chyme—has been very imperfectly performed in the stomach of the horse; and therefore we want, in the upper part of the duodenum, a kind of second stomach,—a similar muscular apparatus to compress and to mix up and dissolve the food. That apparatus is evident enough, and also the branches of this nerve, ramifying over the duodenum, and ceasing before we arrive at the pancreatic and biliary orifices. Its mucous coat also is far more vascular than that of any of the other intestines. I might go further than this, and assert with Magendie, that the phenomena of digestion may be observed in the whole intestinal canal, and even in the great intestine of the horse; but this belongs to the digestive system, and will afford a beautiful illustration of the adaptation of the animal to the purposes for which he was destined.

The Function of the Cerebro-Visceral Nerve as it regards the Stomach.—Then, observation of the course of this nerve, and the mechanism of digestion, unite with the result of experiment in determining its proper function. It is the *motor* nerve; it has to do with the mechanical part of the affair; it has to keep the parietes of the stomach in contact with the food, and the food in contact with the gastric juice; it has to bring the different parts of the food in successive contact with the food, and to propel the food through this portion of the alimentary canal, in order that it may be discharged into the duodenum. The other part of the business,—the production of the solvent menstruum,—is referable to another influence.

The Influence of the Cerebro-Visceral Nerve on the Stomachs of Ruminants.—Let us turn to another of our patients, and the case is yet stronger. The food hastily gathered by *the ox*, passes into the *rumen*, and there it is retained in an immense reservoir, macerating in the mucous fluid secreted from the cuticular coat of the paunch, and a little liquid which occasionally breaks through the floor of this œsophagean canal. Soon after this viscus is filled, the process of rumination commences. The food is returned to undergo a second mastication. But, how returned? Why, in the precise order in which it was swallowed, and has been prepared by this maceration for its return. This is effected by a slow revolution of the food through the various compartments of the rumen, and that is accomplished by the alternate contraction of the longitudinal, and circular, and oblique muscles, for they are not only strong, but they run in every direction. Here too the successive contraction of the different parts of the stomach agitates the alimentary mass, and brings every portion of it, in its turn, into contact with the mucous secretion; and all this is effected by no spinal motor nerve, for no one can be traced to the stomach, but by this cerebro-visceral one, which is seen ramifying in every direction under the peritoneal coat.

A portion of the food which was first received is now, by some voluntary effort (the will is not excluded from every part of the process), brought over the valve-like projection which separates the rumen from the reticulum—the first stomach from the second. This pellet being received into the honeycomb, all voluntary influence and power for a while cease, and the cerebro-visceral takes up the work; and it compresses the pellet, and forms it into the proper shape to reascend the gullet; and it squeezes from its beautifully arranged cells a viscid fluid with which the pellet is enveloped; and then by an act partly voluntary or in which, and that is the essence of our system, many voluntary muscles are brought to lend their aid to the involuntary, or organic ones—the pellet breaks through the floor of the canal, and by the same mingled influence reascends the œsophagus, and is subjected to a second mastication, and is once more swallowed; and, being reduced to a semifluid form, has no longer sufficient momentum to break through the œsophagean canal, but passes on to the third stomach.

There again the cerebro-visceral acts, and acts exclusively. The food has been twice masticated, but there may remain some hard fibres which would resist the solvent power of the gastric juice. These are taken up by the leaves of the manyplus, and a new action commences—a triturating grinding motion; the animal is not conscious of it; it is a purely organic affair; and these fibres are mechanically rubbed down between the roughened papillated leaves of the manyplus. In accordance with this, there are here, compared with the size of the stomach, muscular fibres ten times stronger than in the paunch, and there are far more abundant ramifications from the cerebro-visceral nerve.

The food is at length prepared for digestion, and enters the fourth or true stomach; and there is the muscular action of the parietes of the stomach, and all its effects, as I have just described in the horse. I have dwelled at considerable, I trust not tedious, length on this part of my subject, for the stomachs of the ruminant afford the most varied and satisfactory illustration of the function of the cerebro-visceral nerve, and prove it to be an organic motor nerve.

The Sensitive Influence of the Nerve.—I have not forgotten the ganglion curiously belonging to this motor nerve, nor the possible or probable sensitive influence which it bestows—namely, that kind of sensation which belongs to organic life—a consciousness of healthy, or of deranged action; nor have I forgotten the wide and perfect sympathy which must exist between the various parts of so complicated a machine, that it may act orderly and beneficially: and I can conceive, that by anastomoses with other parts of this nerve, and more particularly with the great organic nerve with which it is already so closely united, and with which, by-and-by, it becomes intermingled—I say that I can readily conceive how, by anastomoses between these and fibres from the sensitive nerves of the spinal column, an indescribable but real feeling of pleasure should accompany the discharge of many of the organic functions, and pain, and sometimes to an acute degree, should mark the departure from healthy action.

The Hepatic and Splenal Plexuses.—As to the fibres, which can be plainly traced to the liver and spleen, we know little of the minute structure of either; and the function of the latter is not yet determined: but there are nutritive vessels in both; there are vessels conveying the fluid, from which the biliary secretion is formed, or which is to be retained in the reservoirs of the spleen; there are vessels to carry away the secretion or the residuum; there are important functions to be performed; and therefore I can easily conceive of the agency both of the motor and sensitive fibrils of this organic nerve: the motor ones concerned with the mechanism of the part, and the sensitive ones with peculiar or sympathetic organic feeling and co-operation. But it is time to return to the medulla oblongata and its lateral column.

The Eleventh or Spinal Accessory Nerve.—There is another nerve, not intermingling with the glosso-pharyngeus or the cerebro-visceral, which, within the cranium, is included in the same sheath with the latter, and passes with it through the foramen lacerum. The origin of this nerve is very singular. As low down as the fifth cervical nerve we observe some minute threads; they are smaller than the fibriculi which go to form any of the other spinal motor nerves; and they arise evidently from the centre of the lateral column. They unite and they form a larger thread, which takes a direction up the neck. It passes between the motor and sensitive roots of the nerves above it—lying under or penetrating through the ligamentum denticulatum; and as it climbs it is augmented by fresh threads. It reaches the foramen magnum—it enters it—it continues to climb until it has reached the ninth and tenth nerves, and then it turns round and escapes again from the cranium, as I have described, through the same foramen with them. It has scarcely quitted the cranium when it separates from these nerves, and, after that, its own trunk divides. Although separated from the sheath of the cerebro-visceral, it sends a branch to unite with the main trunk of that nerve, and also with the glosso-pharyngeus; it sends many branches to blend with the guttural plexus, and thus to form anastomosis with all the neighbouring nerves; a large and main branch, proceeding anteriorly, reaches the sterno-maxillaris, and afterwards buries and loses itself in that muscle; another large division winds round the transverse process of the atlas, proceeds between the levator humeri and the splenius, and, becoming superficial, reaches so far as the top of the shoulder, and then ramifies on the rhomboideus brevis, and vanishes in the substance of that muscle.

An organic Motor Nerve.—The minute fibriculi from which it rises would create a suspicion that it is a motor nerve; the muscles on which it expends itself prove that it is so; and the lateral column whence it springs would indicate that it is employed in the performance of some organic function. The muscles to which it is directed are voluntary muscles; they are concerned in the motions of the head, the neck, and the fore extremities; they are supplied with nerves from the spinal chord, and which excite them to sufficiently powerful action, for the purposes which they are to accomplish. They are voluntary muscles, but they are also concerned, some of them at all times, and that involuntarily and unconsciously, in assisting in the function of respiration, and, when the breathing is hurried, they are all called into action consciously, yet, in a great measure, involuntarily. This is plainly a respiratory nerve, it is concerned with the function of respiration alone, but respiration is one of the organic functions. I class it with the others as *an organic motor nerve.*

The Twelfth or Phrenic Nerve.—We now leave the cranium and the spinal chord, and, observing the numerous ramifications and anastomoses of the cervical nerves, we find a very small one from the fifth (fourth) which takes its course down the neck, seemingly unconnected with any other nerve; we follow it, and it is joined by a branch from the sixth (fifth) cervical, and these, united, pursue the course which the first little filament had taken; and, by-and-by, these are associated with a branch from the seventh (according to the common nomenclature, the sixth) cervical nerve; and I have met with a filament from the lowest cervical nerve. The nerve thus formed, pursues its singular and solitary course down the remainder of the neck, and penetrates into the thorax, above the root of the axillary artery; it enters the mediastinum, and proceeds on, attached to the outer side of the pericardium, a beautiful object to the young and even the experienced anatomist; and it continues its lonely path until it reaches the diaphragm, which is its ultimate destination. It penetrates the pleura—its branches beautifully radiate for some distance over the diaphragm, and are then lost in the substance of that muscle. One branch, however, goes on from the right nerve,—the ramus anastomoticus—and contributes to form the semilunar ganglion.

An organic Motor Nerve.—This is evidently an organic nerve, for it supplies the muscle mainly concerned in respiration. In its natural or relaxed state, the diaphragm bellies into the thorax. It is stimulated by nervous influence derived from the phrenic nerve, and it contracts. In the act of contraction it is flattened, and the cavity of the thorax is enlarged, and the air rushes in through the nostrils and mouth, and the act of *inspiration* is performed. The nervous influence is then withdrawn, and the muscle relaxes, and once more bellies into the thorax; and the cavity is diminished and the air is expelled, or, in other words, the act of *expiration* is performed. There are other auxiliaries of which I have spoken when treating of the function of respiration, but this is the main agent. The division of the phrenic nerve will not at once suspend respiration, for that will be carried on, painfully and laboriously, by the other muscles, which assist in raising and depressing the ribs; but, by degrees, they and the animal generally will be exhausted by their unaccustomed and violent action, and death will ensue. If the spinal chord is divided above the origin of the phrenic nerve respiration immediately ceases, for all the nerves concerned in the dilatation and contraction of the chest are cut off. The cerebro-visceral may continue to influence the glottis and the lung, but that will be of little service when the parietes of the chest are fixed.

Whether a pure Organic Nerve.—It has been taken for granted by physiologists generally that the phrenic nerve is one of voluntary motion, and that the diaphragm is under the control of the will. In its usual action the phrenic is a pure organic nerve, and uninfluenced by the will. The function of respiration,—the arterialization of the blood,—is necessary to the animal from the moment of his birth; it is identified with the continuance of life; and the continued contraction and relaxation of the diaphragm, the main agent in the discharge of this function, is co-existent with the commencement of life, *and ceases only when the vital principle* is extinct. In its natural action then it is perfectly independent of the will. But is not the diaphragm occasionally a voluntary muscle? Can we not increase or suspend its action when and how we please? Indirectly we can; we can call in other muscles to the aid of the diaphragm; we can give to them a certain velocity and power of action, with which the diaphragm from its relative situation must correspond. We can more fully and rapidly than usual expand the chest by the action of the intercostals, and also by that of the spinal accessory; we can rapidly and violently contract it by the power of the abdominal muscles, or we can give fixidity to the chest; we can excite that kind of opposed or antagonist action between the muscles of inspiration and expiration, which the power of the diaphragm shall be insufficient to destroy, and between which it is in a manner paralyzed. We simplify the matter by considering the phrenic nerve as one of involuntary action,—*a pure organic one*;—and a diligent observation of the mechanism of respiration, will render it highly probable that it is so.

Difficulty as to its Origin.—How then shall we account for its origin as a *true spinal nerve,* curiously made up of fibrils from different nerves, but all to be traced to the columns of animal sensation and voluntary motion? This is a difficulty which no anatomical fact will enable me to surmount; and to relieve myself from it, I am obliged to have recourse to that which is unphilosophical, and rarely, indeed, admissible,—the supposition of that which I cannot prove, that when from a better process of maceration or hardening, we are enabled to unravel the complicated fibres of the spinal chord, it may hereafter appear that certain portions of the fifth, the sixth, and the seventh cervical nerve are derived from the lateral column, or that which is connected with the functions of organic life. Until then we must rest content with the undeniable fact, that the phrenic nerve is, in the ordinary distribution of its influence, a purely organic one; and the strong inclination of my belief is, that it is only indirectly that it can be ever brought under the power of the will. It acted before a volition could be formed; it continues to act when the mind is passive; and it is only by opposing a superior power, by the combination of many a muscle, that the will has any control over it. There are stages, and states, and modifications of the respiratory function which are necessarily to a great degree subjected to the influence of the will. If I were to select alone the faculty which every animal possesses of communicating his ideas, or expressing the pleasure or the pain which he feels through the medium of the voice, I should have a sufficient illustration of the association of voluntary power with the organic principle, in a way which neither the function of circulation, or digestion, and, in fact, which no other function can require. Hence arises the difficulty, but a difficulty which is resolved by the observation, that the discharge of the function, so far as essential to life, depends on the organic principle; and that the intervention of the will is permitted only when our occasional "comfort or accommodation" requires. But, Gentlemen, I am here again theorizing somewhat too much. We have one more nerve, and that a most important one to describe, and then neither you nor I shall be sorry to return to that which ought to be the main object of our anatomical and physiological researches, and which gives them their highest, and, comparatively speaking, their only value, a consideration of the nature and treatment of disease.

STONE IN THE BLADDER.

To the Editor of THE LANCET.

SIR,—In the last number of your valuable journal, you published a case of lithotrity, in which the patient obtained relief from long and continued sufferings, by a single application of the lithotrite. Allow me to transmit the particulars of another case, in which a perfect cure was effected by one application of the instrument. I have the honour to be, Sir, your obedient servant,

WM. B. COSTELLO.
7, Parliament Street, Westminster,
August 20th, 1832.

CASE 1 OF LITHOTRITY, PERFORMED AT THE SUNDERLAND INFIRMARY.

William Turnbull, aged 43, a glass-blower of Sunderland, had been for six or seven years in the habit of passing gravel, sometimes without the consciousness of, sometimes with, very considerable pain. The quality of the urine varied with the intensity of the attacks—at one time of a healthy straw colour, at another turbid and mucose. His habit of body, like most of the glass-house workmen, was thin; he was pale, and in appearance emaciated. He collected some of the small concretions which he had formerly voided. They were of various sizes, from a pin-head to a pea. He had not for several months passed any at all, though his sufferings had increased much.

My friend Dr. Clanny being aware of my intended journey to Edinburgh, informed me that Mr. Smithson had this patient under his care, and requested that I would take Sunderland in my way. To this I gladly consented, as I felt anxious to afford the medical gentlemen of that neighbourhood an opportunity of forming an opinion of the new method, by an actual inspection of the operative process. Upon my arrival in Sunderland, Mr. Smithson immediately placed the patient under my care, and invitations were sent to the members of the profession in Newcastle, Durham, and the neighbouring towns, to be present at the operation. In consequence, on the appointed day, the attendance at the Sunderland Infirmary was numerous beyond all precedent. There could be not less than one hundred medical practitioners present on the occasion; amongst whom were, Drs. Clanny, Brown, Ogden, M'Whirter, and Smith; Messrs. Moore, Green, Greenhow, Baird, Smithson, M'Intyre, &c.

The following account of the operation was given by one of the gentlemen present in a local paper:—"On Tuesday, 27th inst. (July 1830), Mr. Costello of London performed the operation of lithotrity upon William Turnbull, a patient of Mr. Smithson's at the Sunderland Infirmary, in the presence of the faculty of that and the neighbouring towns. Before Mr. Costello proceeded to the operation, he delivered a lecture on the subject to the gentlemen assembled, at the same time demonstrating the action of his truly beautiful and simple instruments. As soon as the lecture was terminated, the patient was introduced and the operation performed. The time employed in it did not exceed three minutes, and did not seem to cause any pain to the patient. A vote of thanks to Mr. Costello was passed by acclamation, in which the patient was observed to join very cordially, *tundans terram pede.* After this brilliant operation, Turnbull returned to his home on foot."

The bladder in this case was capacious and healthy. The calculus was no bigger than a peach-stone; it was easily reduced. The *debris* were voided within forty-eight hours, and he returned to his work.

A success of this kind—and lithotrity may now boast of a good number of such cases—is calculated to give rise to cheering reflections in the minds of persons affected with this cruel malady. Lithotrity, which, in its infancy, it was affirmed, could be only applicable to simple cases, has extended its beneficent influence over the disease even in its severest forms. But the case just narrated shows in a strong light its efficacy, and, if I may so speak, its innocuity, under similar circumstances. Indeed it cannot be too strongly impressed upon the minds of sufferers of this class, that it is only when they hasten wisely and in time to have recourse to this operation, that we shall cease to see those alterations of organs, that breaking up of general health, which renders doubtful the success, not only of lithotrity, but of every other curative method.

CASE 2 OF LITHOTRITY,

SHOWING ITS GENERAL APPLICABILITY.

In contrast with the above case, I shall place another, which, for the intensity of symptoms and sufferings, I have rarely seen equalled. On the 19th of January 1829, I was requested by Mr. O'Grady, surgeon to the British Embassy at Paris, to see a compatriot who was in extreme torture. All his symptoms were indicative of the existence of stone in the bladder. We sounded him, and ascertained that such was the fact. The patient, Mr. J. V. Batley, a man of keen sensibility, high moral courage, a poet and painter of talent, was then in the twenty-eighth year of his age, and the tenth of his sufferings. He gave the following history of his case. His habits at school were different from those of other boys; he could not join in their youthful sports; his inability arose from his liability upon active exertion to pain in the region of the loins, in the pelvis, and upper part of the thighs. After he was taken from school, his sufferings became more acute and frequent. He then lived at Camden Town, and could rarely walk beyond the next street. A sedentary contemplative life was forced on him. His general health suffered, and to this circumstance he attributed his remaining in ignorance of the nature of his complaint; for whenever he sought advice, he spoke of his debilitated stomach, his palpitating heart, or his affected lungs, &c. The issue of one of those consultations excited some suspicion of the real nature of the case. After detailing his symptoms, and stating his horror of exertion, his medical adviser told him he must master his disinclination in this respect, adding, if he was a hewer of wood or a drawer of water, he would be relieved from his complaints. On this he took courage, provided himself with an axe, and took to hewing wood. Placing a block before him, he aimed a blow at it; but with whatever violence it fell upon the block, the blow seemed to tell with tenfold force within his pelvis. This seemed very odd. He renounced his new calling, and took to the easel. The air of France was recommended, and thither he went. The journey increased his miseries, and for several months after he could scarcely crawl out of his room. When I first saw him, his appearance was haggard; his sleep broken by frequent micturition, accompanied by the most intense pain in the neck of the bladder, and in the passage, and a burning sensation at its orifice; the urine was muco-purulent, and excessively offensive; the fever was almost constant, with evening exacerbations, and sweatings towards morning; the digestive functions were disordered, and the bowels constipated. He complained greatly of a sense of weight in the fundament, and of the descent of the gut when he made water; acute pains were also felt in the abdomen, pressure on this region or on the hypogaster was insupportable. Cystitis and peritonitis were present. No operation could be safely attempted under the circumstances. The patient was declining into a state of marasm. These symptoms were combated by local bleeding and bathing, emollient cataplasms, opiate suppositories, mucilaginous drinks, jellies, conium, ext. humuli, &c. &c. In the course of two months, a degree of amelioration had been obtained, which allowed him to be removed a few miles into the country. He was, however, still far from being in a favourable state for the operation of lithotrity; and although my friend M. Civiale encouraged him to it, he would by no means submit to the cutting operation. I was much embarrassed by his refusal, for, on the one hand, I felt loath to compromise the new method by a failure, while on the other, I was unwilling to abandon to despair, and to a certain and painful death, a fellow-being, for whom there still existed a chance of recovery, even though slight. He, therefore, agreed to persist in the use of the means already beneficially employed, to wait for greater improvement, and then to proceed in our attempts with all the prudence and precaution possible. A mild and more nourishing diet was permitted, and the quinine and camphor mixture was ordered. By these means strength was in some degree restored; the slow fever disappeared, and although the urine was still muco-purulent, and voided nine or ten times in a night, the patient seemed to have obtained all the advantage we could expect from dietetic and medical care.

Towards the latter end of April, at the ardent desire of the patient, the operation was begun. I used a large-sized lithotrite, armed with a divided drill capable of attacking the stone on a surface of eight or nine lines in diameter. The stone was fully the size of a hen's egg, and required the entire expansion of the branches to seize it. The pain of its application was less than might have been expected. The utmost gentleness was used, but the sitting could not be prolonged beyond three minutes. M. Civiale, whom I had so often served in that capacity, was my assistant on this occasion. Both he and the patient were delighted at so happy a commencement. I, however, relaxed nothing of the caution with which I had hitherto proceeded. A second sitting was held ten days after. On this occasion I employed my shoulder-drill with the best effect. My reason for not making use of the divided-drill on a large and friable stone like this was, that as the time of sitting was necessarily short for the patient's sake, I had found that too much of it must be consumed in the adjustment of its mechanism. This consideration, joined to the want of solidity on those drills, notwithstanding certain improvements I had afterwards made in them, has since induced me to renounce the use of them altogether. After another interval of six days, during which the patient was able to walk a little in the garden, another sitting took place, which was at his own desire prolonged to five minutes; the stone was now in fragments, and the portions voided amounted to somewhat more than half an ounce. From this time forward he slept better, his appetite improved, the urine was less ammoniacal and voided at longer invervals. A few days after this sitting, pain was felt in the spermatic chord, and the left testicle began to swell. I ordered leeches, suspensory bandage, and confinement on a sofa, for he had been imprudent in the newly-recovered use of his legs, and had walked too much. In a fortnight these symptoms had disappeared. Another sitting now took place. Its result was most abundant; there was no reaction of any kind, either on the testis or chord. It was now evident that the stone no longer acted with any degree of irritation on the bladder, and that in the direct ratio of the comminution of the stone, did the mitigation of all the symptoms follow. He could now walk about with ease, and even run, which he used to do in an exulting manner; his flesh and colour returned; he could make water without pain, but still the flow of it was subject to interruption. Two more sittings were held, the labour of which consisted principally in crushing the fragments. The patient had now become so accustomed to the action of the instrument that he seemed rather to desire than dread its application. He returned to town, and resumed his occupations. At this juncture I came to London, and the remaining fragments were removed by my esteemed colleague M. Civiale. Shortly after Mr. Batley wrote to me in these words: "I am relieved from my encumbrance. Since your departure, Dr. Civiale has removed the remaining fragments. He sounded me a few days since, and declared me to be cured; indeed I feel so. The unpleasant sensations and pain that I have daily felt during the space of ten years have ceased. I might sing *Nunc dimittis*. I ought to be on my knees thanking Heaven for the enjoyment I feel in not suffering. It is indeed to me a new existence," &c. Most of the English medical practitioners of Paris were present at this operation. Drs. M'Loghlin, Boyton, O'Grady, Grayson of New York, Jones, Yonge, Morgan, Roche, &c. &c.

Remarks.

These two cases, which I have designedly placed together, will suggest many useful reflections. The first will offer itself as a type of the facility and freedom from pain with which a small stone is removed from a healthy bladder, and as an incentive to an early application for relief; the second will serve to show the almost indefinite extent of the applicability of the new method. From what I have since seen of the good effects of patience, and a well-directed medical treatment in triumphing over difficulties presumed insurmountable, I am satisfied that in the infancy of lithotrity many cases were considered unfit for its application, in which, had it been cautiously tried, it would have been successful. I yielded to this gentleman's entreaties to employ lithotrity, first because he resolutely refused to submit to the cutting operation, which in his case presented but slender chances of success; and secondly, because, although I was not sanguine as to the success, I was satisfied that, with proper precautions, the application of the instrument, even supposing that it had not the desired result, would not aggravate his position.

REPORT
OF THE
RISE, PROGRESS, AND DECLINE,
OF THE
MALIGNANT CHOLERA
AMONGST THE SOLDIERS OF THE
SIXTY-EIGHTH REGIMENT,
AT CLARE CASTLE, IRELAND.

To the Editor of THE LANCET.

SIR,—I take the liberty of transmitting to you a series of reports illustrative of the rise, progress, and decline of the cholera in the 68th regiment.

The reports are voluminous, but the facts which they authenticate are so important, and are detailed with so much intelligence, accuracy, and spirit, that I could not deny myself the gratification of sending you the originals for your perusal, and leaving to you (who can so well perform it) the task of compressing them into such a compass as may enable you to give them publicity through the medium of your valuable journal.

I have the honour, to be, Sir,
Your obedient servant,
PHILIP CRAMPTON,
Surgeon-General to the Forces in Ireland.
Merrion Square, Dublin,
Aug. 25th, 1832.

INTRODUCTORY REMARKS BY THE EDITOR.

The reports are 22 in number, commencing on the 9th of June, and ending on the 3rd of July, 1832, extending over about 80 folio pages of paper. They are, with the exception of one report (made by Mr. Reid, the surgeon of the regiment), drawn up by Dr. Richard Williams, the assistant-surgeon of the 68th, on whom the duty devolved in consequence of the illness and subsequent death of Surgeon Reid, Dr. Williams being afterwards promoted to the vacant post. The first report is dated from the "half-billet station," Ennis. At this place a detachment of the regiment was quartered, and here the disease first appeared. The remainder of the reports were written at Clare Castle, close by, in which the barracks of the regiment are situated, and where the disease appeared in two or three days after its irruption among the civilians and troops at Ennis. The care of the Clare Castle Hospital devolved on Dr. Williams, and the present history relates to the progress of the disease among the troops therein residing. An occasional table of the disease as it prevailed around this spot, accompanies the other documents, but it will be unnecessary to notice them further than to say, that the report from the troops at Ennis on the 12th day, gives 159 cases and 73 deaths, 23 cases still remaining under treatment. The contrast between the results of the practice there and at Clare Castle, will ultimately be seen to be very considerable; but there can be little surprise at the fact when we find it stated in one of the earlier reports, that out of nine physicians in Ennis, Dr. Silver (attached to the regiment) was the only one who would attend a cholera patient. In fact, out of 72 village cases, 70 died. Several of the physicians left the town from fear, and others decidedly refused their attendance either on the military or civilians, for any remuneration whatever. So great was the dread of cholera, too, amongst the people, that the contractors for removing the contents of the dirt or ash-holes at Clare Castle, could not obtain men to go near it.

Premising thus much, we proceed to make an abstract of the reports.

ABSTRACT.

The first case of cholera which appeared in the 68th, occurred on the 8th of June 1832, in a soldier, who was attacked suddenly, and fell on the ground, while on parade. He was instantly removed to a small room in the half-billet station house in Ennis, and placed in bed. He recovered under the following treatment:—An emetic of grs. xl of ipecacuanha with 1 gr. tart. antim. was first administered. In fifteen minutes a second grain of tart. antim. was given, and hot flannels were applied to the stomach and extremities. Full vomiting was induced. The turpentine enema was then thrown up, and cal. ℈j with grs. x conf. opii were mixed and given. Full dejections followed; the heat and pulse were increased, and the patient was convalescent next day.

June 9. Five cases among the men and two among the women occurred this day. These cases were reported by Surgeon Reid, who, while writing down the details, added to his letter, "I have been very unwell for the last fortnight, and had Dr. Williams to assist me." On the following day Mr. Reid died of cholera, and the superintendance of the future cases thence fell on Dr. Williams. The cholera now appeared among the troops in Clare Castle, where a regimental cholera hospital was established, and from which place Dr. Williams had on the 12th of June to report 15 cases.

Clare Castle is situated upon a small island in the Fergus, an arm of the Shannon, and is surrounded on all sides by mud, the tide running underneath the Castle. A more prejudicial locality could hardly be found. Even water fit to drink is not to be obtained for the soldiers residing in it, without sending half a mile from the Castle. In the present instance the men, "in many instances reckless of consequences, drank the water of the Fergus when the tide was out." It is further to be observed, that the barracks were completely filled with troops, and that the rooms are very small, and incapable of being well ventilated. Even the room appropriated for the hospital would only contain 24 persons, and the windows of it opened on the worst part of the swamp around the Castle; on mentioning which, Dr. Williams observes on the 18th June, "I really observe a tendency to collapse when the tide is out."

"It is worthy of consideration, that cholera made its appearance in this castle a short time after an immense quantity of old, filthy, dirty, and execrable stores were removed from the store-rooms for the purpose of survey and sale." Amongst these Dr. Williams particularly noticed "a quantity of mops and handles, which when worn out had been placed in store, impregnated with everything that was dirty, got dry, and thus became a concentrated focus of the most filthy kind, and which at the moment of writing the report (13th of June), contaminates the atmosphere. The only plan that occurs to me (continues Dr. Williams) to stop this awful pestilence, is to evacuate the Castle *in toto*, and to put the men in houses in the vicinity. The village of Ennis, separated only by a bridge, has not a single case in it. I propose the evacuation of the barrack, therefore, as I would suggest the quitting a house that was on fire. Amongst the old stores, I notice a vast collection of old blankets, rags, and sheets, in a state of decay from remaining so long in store, and from long use previous to their condemnation. The soldiers in the Castle were employed in cutting and tearing these old articles into pieces; and, altogether, I am inclined to attribute the great number of cases proportionate to the number of individuals, to the rest of the rubbish which has been so recently unkennelled. The mass is of three years collection. I have recommended that every species of dirt be instantly thrown into the Fergus, to be carried away by the tide."*

The practice of Dr. Williams is thus described by him in the report of the 12th of June. "It consists, principally, with occasional variation, in giving an emetic of 50 grs. of ipecac. immediately on admission. If circumstances admit I bleed. After the feet are warmed by dry heat, sinapisms of mustard and ol. terebinth are applied to the stomach, spine, and feet. When spasms attack the calves of the legs, sinapisms are applied to them after they have been freely rubbed with strong volatile liniment in which the tinct. lyttæ is an ingredient. I next proceed to give calomel in scruple doses every hour at *quatuor vices*. In the interim an enema with ol. teribinth. ℥ij and sulph. sodæ ℥ij is thrown up. The extremities are rubbed perseveringly with the vol. liniment, and when spasm and cold are urgent, with ol. terebinth. made hot. As patients are apt to throw their hands out of the bed, I bandage them with flannel, and friction is continued over the bandages. In irritability of stomach, I give soda water with opium, and in collapse, brandy is added to these. In collapse, stimulants of various kinds are employed, capsicum, spts. ammon. aromat., brandy, &c.

June 17. By this time 81 cases had occurred, when an order arrived for the evacuation of the Castle, and the encampment of the regiment on the highest and driest piece of ground in the neighbourhood. Directions were accordingly issued that the troops should remove with every article of dress, &c. in a perfectly clean state, taking none of the bedding hitherto in use, new or clean blankets being issued. The hospital at Clare Castle, however, was still preserved for the admission and treatment of all cases that might occur. The effect of the removal was very salutary on the troops, amongst whom great despondency prevailed. The promptitude and rapidity with which the tents were conveyed, and the whole migration was effected, inspired them all with courage and good spirits.

In his report of the 17th June, Dr. Williams writes,—"I have abandoned the use of the lancet, the collapse being so rapid, and attended with such a total loss of pulse, arising probably from our low situation. A serjeant *admitted* at nine this morning, collapsed in *less than an hour*. When the spasms continue, I remove the sinapisms from the spine, and apply blisters. The plan of injecting *saline solutions* into the veins, has been tried at Limerick (whither Dr. Williams went some short time previous to study the disease), and has proved a total failure in every case, so that I do not propose to attempt it. I have given the sulphate of quinine in good old sherry to convalescents labouring under debility, particularly to old drunkards, of whom we have a tolerably large share, the result of our residence in Canada. As I only take three teaspoonfuls of sherry per diem myself, and that only during periods of incessant fatigue (the duties of Dr. Williams were excessive, preventing opportunities for sleep for nights together), I am able, with a good conscience, to express my abhorrence to the men of that execrable mixture called "whiskey punch," at this period of danger. * * * The hospital nurse has just complained, and has had her emetic. I almost attribute her attack to a deviation from my directions on the subject of liquor. I allowed her a glass of sherry twice a day, but yesterday, suffering from fatigue, she took a tumbler of my abhorrence, hot whiskey punch. I saw cholera in her eye, and mentioned it.

I hear of no recovery at the village of Clare, the collapse there is so immediate after the first appearance of purging and vomiting. Of thirteen cases twelve are already dead. We exploded a good deal of *gunpowder* in the small and confined quadrangle of the castle, where, from being surrounded by high building, no circulation of air can take place. I advised this, as the explosions, if they have no other effect, at least change the atmosphere, and the men and women, moreover, have a great opinion of the plan."

18. All the men slept under canvass last night. The ground was so well selected, that they were all perfectly dry. The whole ground beneath the tents, however, was boarded, as a precautionary measure, and an additional blanket was given out in lieu of sheets. A car and horse are kept at the camp to be in momentary readiness to convey cases to the hospital at the Castle. At the same time, all convalescents from the hospital are ordered to perform a quarantine of ten days at a short distance from the healthy tents, before rejoining them. Tents are appropriated for that purpose.

19. No deaths amongst the men yesterday. The cases at present do not present so malignant an aspect as they have done. "I am just called to Colonel Cross (the colonel of the regiment), whose exertions have predisposed him to an attack. He has taken an emetic, his stomach is tranquil. I have commenced with the calomel, and at present I see nothing unfavourable about

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* While the clerk was recording these facts at the dictation of Dr. Williams, he was seized with diarrhœa, and was obliged to be conveyed to the cholera hospital.

him." The cases up to this day are 104; deaths 18; recovered and discharged, 47.

20. The disease on the decline. Only two soldiers admitted from the camp, and only nineteen soldiers now in the hospital. But one death the day previous, and he a great drunkard,—one who, in Canada, drank upon the average, probably, about a quart of ardent spirits a day. " In the tent the cleanliness of the people is strictly attended to, particularly as regards the feet of men, women, and children. I have advised, on the score of diet, that the coffee shall have more milk than usual, and that the milk be boiled for the morning and evening repasts, making, in fact, *café au lait*, which is highly nutritious, the more especially as the cows are milked in the adjoining field. Breakfasts to be partaken of at seven. I have also strongly advocated that excellent dish called 'Irish stew.' Colonel Cross much better. The calomel has touched his mouth. His death would have paralysed us. As it is, I think the effect will be good, by showing the men that their officers are equally exposed to this sad malady with themselves. I look with dread to the admission of detachments of troops into the camp, who have passed through infected places. Sentries are open to bribery like other men, and a single man, woman, or child, straying into our camp from the quarantine ground (which is in the same field) might be fatal to us. No subject hangs so much upon my mind as this."

" In cases of alarm, or depression, I find immense advantage from giving three or four grains of quinine in a glass of sherry."

21. Amongst all the convalescents, men, women, and children, who have remained ten days in quarantine before joining the general camp, not a single instance of relapse has occurred. " One woman (the wife of a serjeant) having violated the quarantine regulations, which confine the convalescents within a certain limit, has been expelled the camp altogether. Without an example of this sort, we should have the wives and children of the soldiers straying in every direction. Two children, whom I met on the road out of the limit, I have punished by putting with their parents into a part of the quarantine limits separate from the rest for four days." Colonel Cross is free from all symptoms of cholera, though labouring apparently under disease of the lungs. " As for the camp, it has changed a scene of the greatest depression, melancholy, and despair, into one of cheerfulness and happiness. A hospital tent has been prepared, with boards and tressels, for the reception of common cases. The occurrence of two days of brilliant weather, has enabled the women to wash every article of the men's linen, a most important consideration, for which we have every advantage." Total cases to this date, 114.

22.* No case of cholera during the whole of yesterday. Diarrhœa, from damp, came on in three cases, but the dejections were yellow; they occurred in persons who had been convalescents in quarantine; the day was one of continued heavy rain. " In two cases, typhus has succeeded to cholera, with delirium. One of these cases is doing well; of the other I cannot speak favourably. A fresh detachment arriving, more tents have become necessary; but new ground for them must be had, or there will be no room in the present field for exercise or amusement." The adjoining field should be hired, or we shall have to use the lower part of the hill, which gradually terminates in wet ground, from the overflowing of a spring of water. The great object appears to me to be to keep the tents upon the crown of the hill, and not to let them be too crowded. As we descend, the situation for tents proportionately deteriorates." The report of the 25th of June says, " I fear we shall not be able to obtain another field, the Rev. Mr. Young having refused the request of the Major General. In this case we shall be rather crowded." The terror from which this refusal probably arose, was, as before intimated, universal amongst the civilians, as may be further judged from the following report.

24. A case occurred yesterday at Six-Mile-Bridge (a short distance from the camp), in a detachment, two fatal cases having happened among the inhabitants five days before. The remaining men of the detachment were all removed to quarantine ground, leaving one man only to tend on the cholera patient, " to whom the inhabitants refused all supplies of food; tea, &c., having to be made for him at the inn." Even the medical practitioner, " afraid of coming in contact with the disease, positively refused to attend the case." One case of cholera was admitted yesterday in the Castle Hospital from the camp, as also were two cases from the Six-Mile-Bridge quarantine tents. " We have three very severe cases of typhus, one of which I fear will prove fatal." All the cases of consecutive fever have been exceedingly severe, the head being much affected.

25. Yesterday one case from the general camp, and one from the Six-Mile-Bridge ground. The detachment from the latter place is now gone to the general camp. The house at the Six-Mile-Bridge where the sick case was left, " was yesterday sur-

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* Dr. Williams, in the report of this date, acknowledges his appointment as *surgeon* to the regiment; the notification to him of which, was accompanied by an assurance of " entire approbation of his conduct during a most trying period."

rounded by 100 men with straw, probably intending to burn the dwelling; but they were overawed by the police. The medical practitioner, from his great alarm, still refuses to go to any case of cholera."

27. All the cases doing well. "It is worthy of observation, that the servants of officers have particularly suffered.[*] Servants generally live in offensively dirty apartments. They are allowed to be out of mess, and are in consequence badly and irregularly dieted, and the money which should be appropriated to wholesome nutrition is often expended in administering to their vices. In this way they are more particularly predisposed to disease."

28. But two cases in bed. The whole barrack to be well scoured and white-washed. "The generous diet, particularly the *café-au-lait*, at breakfast and supper, has contributed to the well-being of the men, and left less money for ardent spirits."

29. The car for conveying corpses is dismissed from the camp. Only a simple case of disorder brought to the hospital, and that was a case of colica, from eating unripe crab-apples, which abound around the camp. Two or three more such cases occurred within the week. Several beds, which were saturated with the alvine dejections of the patients, have been burned.

July 1. The men never looked better.

3. Not a case left. The disease not yet terminated in Clare itself. About 90 persons went from Clare a few days since to a large house in the neighbourhood, where they attacked a puncheon of whiskey, and have since lived in a most disorderly manner. Cholera in a very malignant form has in consequence appeared amongst them. "The disease having left the troops, I need no longer trouble the Director-General with a daily report."

Total from the Commencement.

	Cases.	Deaths.
Officers	2	1
Soldiers	96	18
Women	17	1
Children	7	2
Total	122	22

Richard Williams,
Surgeon, 68th Regiment.

INTERNAL ADMINISTRATION OF

COLD WATER, EXCLUSIVELY,

IN THE

THIRD, OR COLLAPSE STAGE,

OF

MALIGNANT CHOLERA.

By Hardwicke Shute, M.D., *Gloucester.*

Being an Abstract of Two Memorials addressed to the Central Board of Health, London, August 21st and 25th, 1832.[*]

Gentlemen,—It is stated in your circular, dated 9th August 1832, that in the third stage of cholera, there is but little reason for hope. I request your particular attention to a plan of treatment which I have found succeed in twelve consecutive cases in which my directions were strictly adhered to.

My attention was very early directed to the fact, that most decided injury was done by the administration of brandy or alcohol in any form, or even of stimulant emetics, where they were retained, as frequently happened in the advanced stage of collapse. I was most forcibly struck with the marked analogy which exists between the collapse of cholera and the impaired vital energy which results from starvation or excessive cold. Now it is an established fact in therapeutics, that the administration of a stimulus, disproportioned in strength or extent to the impaired vital energy of the system, is certain death. The treatment which I have adopted in the second and third stages of cholera, is founded on the analogy already mentioned, and on the general principle, that the greater the sinking of the vital powers, the greater is the necessity of withdrawing all kind of stimulus. The circumstance which particularly directed my attention to the remedy I employ was the thirst, the inordinate desire for cold water almost exclusively. I shall therefore state, that the allowance of cold water which, in the most marked cases of recovery, was taken to the extent of some gallons in a few hours, is the circumstance to which I desire to call your attention. I add to this the abstraction of all kinds of stimulus, external and internal, even to the exclusion of friction, or the application of heat in any form.

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[*] "The following statement will show the number of those attacked and the result.

The servant of Surgeon Reid	..	Died.
The orderly of Surgeon Reid	..	Died.
The servant of Major North	..	Recovered.
Ditto Lieut. Flint	..	Recovered.
Ditto Lieut. Graham	..	Died.
Ditto the Quarter-Master,		Recovered.
The female-servant of ditto	..	Recovered.
The servant of the Adjutant	..	Recovered.
Ditto Lieut. Hill	..	In Hosp.
The first mess waiter	.. ..	Died.
The mess cook	.. .. ..	Recovered.
The second mess waiter	..	Recovered."

[*] These documents reached us too late this week to admit of our publishing them entire in our present No. We may state, however, for the satisfaction of our readers, that not a single line has been omitted in the abstract which is material to the purpose of the ingenious, patient, and humane author.—Ed. L.

The number of cases, I think, is not too few to justify a more extensive trial of the plan proposed. My observations apply to the second, and particularly to the third stage of cholera, "when the pulse at the wrist has ceased, or become almost imperceptible."

The windows of the apartments, at the Cholera Hospital in Gloucester, are large and numerous in proportion to the size of the room, and the door which opens immediately into the garden, is seldom shut. The windows are open day and night, so that the patient may be considered as living in the open air, and the fire is kept so low as not to influence the temperature of the room. The covering of the patient is confined to a light blanket or rug; and it seldom happens that some part of the patient, particularly the breast and shoulders, is not constantly exposed. Under these circumstances, a pint of cold water is offered to the patient, and very frequently two-thirds of this are taken at a draught. In what I consider the most favourable cases, vomiting is almost immediately produced, and the patient in two or three minutes again calls for, and eagerly drinks, the same quantity, with the same results. This is often continued for hours, until gallons of water have been taken, and the greatest proportion, but I conceive not all, is rejected. In other cases the patient is too insensible to ask for water; and under these circumstances, it is offered every ten minutes or quarter of an hour, and most commonly drunk with avidity. If gruel or tea be offered, the patient most frequently refuses it; and, generally speaking, no kind of nutriment is taken in any form until the period of convalescence. In the first six or eight hours no amendment can be observed. In the next six or eight hours there is some diminution of intensity in the purple hue of the extremities. In the next six or eight hours there is a manifest improvement in the countenance of the patient, and increased disposition to sleep. In some cases the pulse has not been perceptible for twenty-four or thirty-six hours. From this period the pulse, the animal heat, and the secretions, are very gradually restored; and at the end of forty-eight hours, on the third day from the commencement of the plan of treatment proposed, the patient is convalescent; and in all cases without consecutive fever. I mention these circumstances particularly, in order that the practitioner may not be impatient: he should make not the least alteration in the plan laid down, as long as the patient is merely not getting worse.

I cannot now refrain from briefly suggesting, 1st, the importance of knowing, that in the collapsed stage of cholera, cold may be extensively applied to the coats of the stomach without diminution (to use the most cautious term) of the vital energy; 2d, that cold so applied has a manifest tendency to check the exudation of serum, which characterizes the disease; 3d, the acknowledged good effects of vomiting; 4th, the probable good effect of the fluid absorbed from the stomach in restoring the fluidity of the blood; 5th, the great importance of the arterial circulation being restored as gradually as possible, and the fact of convalescence taking place without the consecutive fever.

August 21st.

The first case in which the water practice was tried, and proved successful, occurred about six weeks ago, soon after the appearance of the disease in this city. In the four first cases which came under my notice, internal and external stimulants were liberally administered. The mustard emetic and bleeding were also had recourse to, and every patient died. In these four cases I had observed that thirst was a prominent symptom; and in one instance, when the patient was unconscious of everything, I was much struck with an earnestly expressed desire that no fire should be made in the room; directions to that effect having been given in her hearing.

Soon after this, I was requested by a medical gentleman of this city to visit one of his patients, a female, aged 15. It was a marked case of collapse. (Many remedial means were tried in vain.) I left the house with the impression that our patient would not survive many hours. Having reflected upon the case, and feeling anxious, if possible, to give some relief, I returned in the course of an hour, and ordered some very strong green tea to be made, with the intention of giving it to an unlimited extent. Whilst the tea was in preparation I presented about a pint of cold water to relieve the thirst, which was urgent. There was at that time no improvement in the symptoms. The greater portion of the water was drunk with avidity, and almost immediately rejected. A few minutes after, the water was asked for, and drunk with the same result. Having witnessed a continued repetition of this process for half an hour, without any symptoms of increased collapse, and with the satisfactory circumstance of a change in the voice, which had been strongly characteristic of the disease, I left the patient, with directions that cold water should be given to any extent which she might desire. The tea was refused as soon as tasted. I was informed the next morning that she had taken the water to the extent, as it was imagined, of some gallons, and had been sick after each draught; but had not asked for the water, or been sick, for the last hour. The window which opened immediately upon the bed, was unclosed, and had been so, I was informed, the whole of the night. At this time there was no perceptible pulse, or increase of animal temperature, but the countenance was, I thought, somewhat improved. The same plan was continued. In the evening the pulse was perceptible. On the following morning reaction was established, and in two days from that period the patient was convalescent without fever.

Soon after this, I met another medical gentleman, who was quite in despair; he said he had lost *seven* cases of cholera in succession, and had another who would probably die before night; and that he had done nothing, merely ordering small doses of capsicum with camphor mixture. I found this patient (a female, æt. 21) in a very similar state to the last, and recommended the unrestrained use of cold water. The next day she was better, and on the following day I found her restored, but with too much excitement; beer and cider, as well as cold water, having been, through the prejudice of the attendant, very freely administered. She was perfectly restored in a few days.

This practice has been adopted by other medical gentlemen, in one instance to my knowledge; and I have reason to think in another; but in some modified form; in both instances originating in my suggestion. The gentleman with whom I attended the second case already mentioned, has favoured me with the following letter:—

"My dear Sir,—It is with much pleasure I here enumerate the cases of cholera which have come under my notice since you suggested the cold-water treatment; upon the success of which I beg to offer you my most sincere congratulations, and doubt not but the same beneficial effects which I have witnessed will be generally experienced when the practice has become more frequently adopted. I remain, your sincere and obliged friend,

"CHARLES CLUTTERBUCK.

"Gloucester, Aug. 25, 1832."

A. B. (the names are given in Mr. Clutterbuck's letter), female, age 20 (the young woman you saw with me), having drunk freely of cold water, recovered in a few days; able to go to her usual employment.

B. C., male, boot-maker, age 21, a very hard drinker; took one dose of an aromatic mixture; drank freely of cold water, and could not be prevailed upon to taste his mixture afterwards; perfectly recovered in a few days, to the great astonishment of his friends.

C. D., male, age 5 years. A very severe case. This child drank, during the night, nearly *three gallons* of cold water, and was convalescent, but is now suffering from a severe attack of fever.

D. E., female, age 34. The same plan of treatment, and was convalescent in a few days.

E. F., male, age 32. Treated upon the same plan, and convalescent in a few days.

My experience, subsequent to the cases mentioned, has been almost confined to the Cholera Hospital here, which was opened a fortnight since under my superintendance, with the assistance of a resident apothecary. The four patients sent to the hospital on the first day; not then under my care, were all corpses the next morning. Since that period, of 48 cases there have been 20 discharged cured; eight I consider convalescent; eleven deaths, and nine under treatment. Of the deaths, two were obviously referable to previous treatment; three were in progress towards recovery, and relapsed from over exertion while in a state of extreme debility; two were under the age of three years, and died from cerebral congestion; one was more than fifty years of age; one sixty (the former extremely emaciated, from vomiting or disease); one was so far advanced as almost to have lost the power of swallowing; and one, a notorious drunkard, died with a gin-bottle concealed about her person, a few hours after admission into the hospital. The deaths cannot, I think, throw any discredit on the practice. Any suggestion from those who are much more conversant with the disease than myself, will be received with gratitude, or any question answered to the utmost of my ability. I am, Gentlemen, your obedient humble servant,

HARDWICKE SHUTE, M.D.

Gloucester, Aug. 25, 1832.

MODE OF EMPLOYING FRICTION.—When my wife faints, which is miserably often, I do not apply friction in the usual absurd way,—rubbing backwards and forwards, which must alternately check and accelerate the passage of the blood,—but taking hold of one of her hands with my left-hand, I place the thumb and finger of my right-hand tight round her wrist, and then pass them them firmly up towards her elbow. Having brought them back loosely to the wrist, I pass them firmly up again, and when I have repeated the operation two or three times, *sometimes* on both arms, I have the pleasure of hearing the ejaculation, "I feel better now." When I first had recourse to this means of resuscitating my lady, she exclaimed *instinctively* on two different occasions, without being at all aware that I had had any particular intention, "Oh! that is what I seem to want." From the invariable and immediate effect of this mode of friction, I flatter myself that it is not unworthy the notice of the medical practitioner in the friction of cholera patients, a process which should be much oftener resorted to, and more energetically persisted in, than it generally is. N. G.—*Extract from a letter to the Editor.*

CASE OF

MALIGNANT CHOLERA,

IN WHICH

FOUR HUNDRED AND EIGHTY OUNCES OF FLUID WERE INJECTED INTO THE VEINS WITH SUCCESS.

By THOMAS WEATHERILL, *M.D.*, *Liverpool.*

JOHN STEVENSON, Village Place, Clayton Street, Liverpool, aged 29 years, blacksmith; works in an iron-foundry; is rather tall and well made, of general good health, and not addicted to any kind of intemperance, but has been, for the last few months, occasionally troubled with slight pain and irregularity of the bowels, headache, and sleepiness, with a continual sense of mental and bodily lassitude; appetite and strength considerably impaired. Was seized with the malignant cholera when at work on Saturday morning, 18th inst. The disease was promptly and vigorously met; its virulence was subdued in a few hours, and on the 21st the patient came down-stairs, and took dinner with his family—eating, among other things, a fried egg.

In the evening of the 21st inst. he was again seized, occasioned no doubt by his imprudence, with the complaint, in a most sudden and violent manner; the most powerful and energetic treatment was immediately, as before, had recourse to, and assiduously kept up during the night, but, notwithstanding, the patient continued to get worse, and at seven next morning, thirteen hours from the second attack, it was quite evident he had not long to live if something could not be instantly done to arrest the progress of the disease. My experience in the treatment of this disorder has convinced me that the last stage of it, the stage of collapse, is not to be restrained or controlled by medicine, and I had determined to secure the aid of venous injection, as performed by Dr. Latta and others, in the first case that might offer. In this instance I dissolved ʒij *muriat. sodæ*, and ℈ij *carb. sodæ*, in half a gallon of warm water of the temperature of 112° Fahr.; this mixture was gradually and cautiously thrown into the left basilic vein, instanter, with the most happy effect. Relief first showed itself more particularly in the respiration; it was then speedily communicated to the circulation, heat was evolved, and the whole system soon after responded to the operation in a manner which clearly proved that the patient had obtained signal benefit. It was indeed truly delightful to witness the progress, the rapidity, and the extent of this relief, as it was developed in the different stages of the operation! The relief, however, was transient; in half an hour he was fast sinking into his former condition. The same quantity of the mixture, mentioned above, was again introduced into the arm, and the patient quickly revived—he was "himself again;" the same rapid general improvement was evinced in this as took place in the previous operation.

At ten a.m. (nearly two hours had elapsed since the second operation) I found my patient in a profuse sweat, extremities cold, and in a fearful collapse. Another half-gallon of the mixture was had recourse to, and followed by similar benefit as before. Soon after (one gallon and a half had now been injected into the circulation) the patient passed *per anum* about a quart of pale yellowish-coloured watery fluid, of a very offensive odour, which contained apparently some fæces. In ten minutes he passed another of like quantity and character: again he sweated, everything about him was quickly drenched; he was restless, cold, and nearly pulseless. A fourth half-gallon was let into the system; in a moment he rallied, again became cheerful, and expressed himself quite relieved. I remarked here that the sweating was suddenly checked after the process of injection had commenced. He parted with another offensive dejection, in all other respects as formerly. An enema, composed of one quart of warm water, half an ounce of alum, and two ounces of the spirits of turpentine, was given him; this came away in a few minutes, leaving a smarting sensation in the bowels, which continued for upwards of two hours, and annoyed him exceedingly.

It was necessary to perform venous injection at 2, at 5, and at 9 o'clock p.m., using the same quantity of saline mixture as before, at each operation, except at the fifth, which contained six pints, making in all seven operations, and introducing thirty pints of fluid into the system in the space of thirteen hours, the pulse becoming steady at 10 p.m., beating 100 per minute, a natural warmth pervading the whole body, with other favourable symptoms which indicated pretty clearly that further injection into the veins would not be called for, at least for the present.

I ought to state that, at 7 p.m., he took grs. v of calomel, and gr. ss. of opium, and this dose was repeated every two hours until seven o'clock next morning; besides, nearly two ounces of the strong mercurial ointment were rubbed on his lower extremities. Also, he had an enema of common table salt and warm water. He took freely of beef-tea and arrow-root prepared in water, had three copious dark-coloured alvine discharges, and for the first time since his second attack passed a considerable quantity of urine during the night, perspired, and slept at intervals.

The calomel and mercurial friction were stopped as soon as his mouth began to exhibit the usual effects of this medicine, substituting a wineglass of the following mixture, to one tablespoon of lemon juice, taken every three hours in a state of effervescence. *Tinct. cascarillæ,* ʒij ; *Carb. ammon.* ʒij ; *Aquæ* ℥x.

He took a little port-wine occasionally, and drank plentifully of beef-tea, and weak mutton-broth. His convalescence has been attended with nothing worthy of remark, except much harassing with a hiccough; but this is subsiding, and the only thing which now remains an obstacle to his complete recovery, is debility.

It will readily be perceived, that much time and attention were given to this case. Had there been less, it must have terminated in another way. In return, I possess the proud feelings of having been the means of rescuing a human being from death.

The result of this case ought to be taken as a fair specimen of what venous injection is capable of accomplishing, and in what degree this operation should claim the attention of the profession and the public. My own opinion of it is exalted, and I shall hereafter consider myself as not doing my duty, if ever again I suffer a patient to die of the cholera, without first giving it a fair and full trial. Any medical man who does not, perhaps will excuse me for passing severe judgment upon his conduct.

Liverpool, Aug. 26, 1832.

NOTE FROM DR. HANCOCK.

To the Editor of The Lancet.

Sir,—I am aware that your Journal is devoted to objects of higher interest and utility than the feuds of polemical controversy; but I beg leave to say, in answer to the remark of Mr. Hart, in your last Number, that nothing could be more opposed to my intention than to offend, or in any way to hurt the feelings of, any member of the medical profession; and it appears to me that gentleman cannot, with propriety, stigmatise my remarks as *illiberal or unjust*, since, *as the statement appeared in the* Lancet, he could not but see that the same was liable to animadversion; this, indeed, is implied by his own *explanation*, which appears sufficiently satisfactory, and while it exonerates him from the imputation of ignorance or inconsistency, it will also show, I presume, that my strictures were not altogether unfounded. I am, Sir, yours, &c.

J. Hancock.

CASES OF DROPSY.

By John Whitehead Macnee, *Esq. Surgeon,*
Member of the Med. and Phys. Soc. of Calcutta.

CASE I.

Two cases of this disease, which may possibly possess some interest, came under my care on board the H. C. S. ship *Duke of Northumberland*, while on her passage to England during the early part of the present year.

Mrs. R., a soldier's wife, first applied for assistance on the 3d of February. Is aged 33, and has been ten years resident in India. She is now sent home at the expense of government, on account of continued irregular conduct. Has been in the habit of drinking freely, and has long suffered from pain in the right side, for which leeches and blisters have been repeatedly applied. The employment of these means afforded her occasional relief, but it was only temporary. Has likewise, on several occasions, become subject to effusion into the abdomen and lower extremities, which disappeared while under medical treatment, but returned again on being sent out of hospital, and resuming her abandoned habits.

At present there is continued dull pain in the right hypochondrium, and on any attempt at examination, so much uneasiness is produced as to render it impossible to ascertain with any degree of certainty whether the liver is enlarged or not. The pain extends to the back and shoulder, and is increased by inspiration. The abdomen is swelled, and a fluctuating fluid easily detected. The legs are of their natural size, and the urine scanty and high-coloured. It coagulates on being exposed to heat. The action of the heart is regular, but she suffers severely from oppression in the chest, and frequently starts during the night, dreading the approach of suffocation. For the last three months her strength has sensibly declined, which she attributes to diarrhœa, which has continued during that period without any remission, the evacuations being occasionally of a natural colour, but loose, and at other times tinged with blood, and always voided with more or less uneasiness. Catamenia suppressed; countenance anxious; tongue white, and pulse feeble, and 120.

A blister was applied to the right side; took 10 grains of calomel, and 2 of opium, immediately. In the evening she had 3 drachms of castor oil. She was put upon spare diet. On the following day she was by no means improved. The pain in the right side continued unrelieved. The blister had risen well. She was directed to take four grains of hydrargyrus cum creta with one of opium three times daily.

Feb. 9th. Had an attack of intermittent fever this forenoon, for which it was necessary to give her six grains of the sulphate of quinine during the cold stage. It had the effect of stopping the paroxysms. The diarrhœa continues, but the pain in the side is somewhat easier, and she breathes with greater freedom.

14th. Has had no return of ague. The diarrhœa has ceased, and the pain in the right side is nearly gone; breathes with greater freedom, and sleeps more composedly; the liver appears free from any enlargement; urine more copious; skin moist; tongue cleansing; pulse 95. The medicines were continued, with the addition of ten drops of the tincture of digitalis, three times daily.

21. Is improving; sleeps well during the night; mouth slightly affected; tongue clean; bowels regular, and motions of a natural colour and consistency. Continue the digitalis, and omit the hydrar. c. creta.

25. Her medicines were this day discontinued. Her breathing is free, and urine passed in natural quantity. The swelling of the abdomen has nearly disappeared. General health good.

CASE II.

Mrs. E. came under my care on the 21st of February. She was aged 30, had been seven years in India, and, like the former patient, was sent home by the company on account of continued bad conduct. She had been in the habit of drinking freely, and exposing herself to the rays of the sun without any covering. She had had repeated attacks of dysentery and intermittent fever, and had likewise suffered from inflammation of the liver. She was upwards of three weeks on board before she applied for any assistance, being prevented reporting herself sick from the dread, I suppose, of being deprived of her allowance of liquor.

She was of a pale and flabby habit of body. The abdomen and lower extremities were much swollen, pitted, and were tender on pressure. She had a sense of dull obtuse pain in the right hypochondrium, which was aggravated by the hand being applied to the part, or on attempting a deep inspiration. When she reclined on the left side uneasy feelings were likewise produced. She suffered severely from effusion in the chest, more particularly during the night. Had not menstruated for several months, and since her embarkation she had repeatedly been intoxicated on, I am afraid, the produce of her prostitution. The pulse was small and frequent, tongue furred, and breathing hurried.

Ten ounces of blood were taken from the arm; bandages applied to the legs; spare diet, and abstinence from spirituous liquors. The following every night at bedtime.

℞ *Pil. hydrargyr.*;
Ex. aloe, aa gr. vi;
Colocynth. co. gr. iij. M. *Fiat mass. et divide in pil.* iij;
Olii tiglii ♏ iij.

This treatment was continued to the 26th. The swelling by that time had left the abdomen and extremities; the pain in the side was less, and respiration performed with greater freedom. Copious watery discharges had taken place from the bowels, and on the 5th of March she was quite well. Both of these patients continued under my observation till the beginning of June, and remained free from any complaint.

FLOODING FROM IRRITATION OF THE RECTUM.

I am induced to append to the foregoing cases one of a different nature, but likewise somewhat interesting in its details.

Mrs. Captain ——, aged 32. This lady became pregnant in May 1831, and in the beginning of the following July, after some exertion, was seized with regular labour pains. Flooding took place to a considerable extent, but the practitioner who attended her declared that abortion had not taken place. The discharge of blood, however, still continued in a diminished quantity, although the recumbent position had been strictly adhered to. When she came under my care, the patient's strength was very much reduced, the discharge still continued copious, and the finger could easily be introduced into the os uteri.

In this case there was much obscurity. The surgeon who had attended the lady in Gosport had given no written statement, and all that could be learned of its previous history was from the imperfect accounts of the patient and her husband. Dr. James Johnstone, surgeon of the 23rd regiment of Bengal Native Infantry, attended the case along with me, and at first we were both inclined to believe, that expulsion of the fœtus had taken place, with retention of the placenta; still, however, as the previous medical attendant had positively declared that no miscarriage had taken place, we deemed it prudent to follow his opinion for a time, until we could more decidedly come to one of our own. Accordingly, rest was enjoined; a plug, wet with an astringent injection, was introduced into the vagina; opening medicines were given, and small doses of the sulphate of quinine to support the strength.

This treatment was continued for ten

days without any improvement. The strength had declined, and the discharge, if anything, had increased. The os uteri was lax and open. An injection was given, which removed a large quantity of hardened fæces from the rectum. This afforded almost immediate relief, and it was determined to repeat the enema every second day. Under this treatment the discharge every day became sensibly less, and in twelve days had entirely ceased.

In this case the discharge was without doubt maintained by the irritation of the hardened fæces in the rectum, and strikingly manifests the sympathy which exists between it and the uterus.

Glasgow, August 25th, 1832.

MANEC'S SURGICAL COROLLARIES.

Extracted from his work entitled "Traité Théorique et Pratique de la Ligature des Artères."

After the general considerations which have recently been advanced (*vide* Lancet, No. 469), and which have for their basis the results obtained by a great number of experiments made upon living animals, and accurate observation of the human frame, we may propose, as general rules to be complied with in the ligation of arteries, the following principles :—

1. To obtain the obliteration of an artery, it is necessary the passage of the blood through its area should be entirely suspended.

2. This obliteration is effected more promptly, and with less danger, when the internal and middle tunics have been divided, than when the same membranes have preserved their integrity.

3. The clear and regular division of the internal and middle membranes, such as is obtained by the use of the small round ligature, favours more signally the occlusion of an artery, than the broken and irregular laceration of these tunics, such as occurs when broad ligatures are employed, whether large or small.

4. The means which nature employs to close definitively a tied vessel, are of two kinds : first, the adhesion of the walls of the vessel to each other ; second, the formation of a clot of blood, which, as soon as it fills the vessel, becomes adherent to its internal surface.

5. The phenomena which ought to appear in a tied artery immediately after the operation, and to continue until its entire obliteration, being *ceteris paribus* in the direct ratio of the vitality of its wall, it is of the utmost importance that the surgeon do not insulate its walls more than is absolutely necessary for the application of the ligature, in order not to diminish the vital properties of the vessel.

6. The adhesion of the walls of an artery together cannot take place unless the tunics possess their vitality unimpaired. No union at all takes place of the parts killed, and but a feeble adhesion if there be but little life.

7. The coagulum is not formed in a tied artery when the ligature is placed very near a large collateral branch given off on the side of the heart.

8. The first means employed by nature in resisting hemorrhage, after the fall of the ligature, such as the adhesion of the opposite walls of the artery, or the agglutination of the lips of the divided middle and internal coats, are always destroyed entirely or in part by the suppuration which occurs when the ligature comes away.

9. The clot, the formation of which is the second process, has almost always its base in contact with, and adheres more or less tenaciously to the agglutination of the arterial walls, disorganised and destroyed by the same suppuration ; so that if it have not a certain length, or if it have not contracted an adhesion sufficiently strong with the sides of the artery to resist the impulsion of the blood, it will be displaced, and hemorrhage will take place immediately. The clot is too small, or does not form a wall, because the ligature is too near a collateral branch, or does not unite, or unites but feebly with the walls of the artery when the tunics are diseased, or when the vessel has been irritated and insulated to too great an extent during the operation.

10. The adhesion of which we have spoken once established, may be destroyed by the suppuration which is propagated from the adjacent parts, and from the surface of the artery towards its centre.

11. The coagulum itself may be destroyed entirely when the inflammation is too strong, and that may occur even before the separation of the ligature.

12. In these cases, if there does not exist a sufficient distance between the apex of the coagulum, of which the base has just been destroyed, and the origin of a large collateral artery, hemorrhage will inevitably occur ; in the contrary case, a new clot will be formed, and will follow the same transformations as the first, if not arrested in its development.

13. The principal causes of the suppuration, of which the consequences may be so unfortunate, are the long continuance of the

operation, the meddling of the surgeon to ascertain the various organs he encounters, the laceration or contusion of these organs, the insulation of the artery to a great extent, or its too great extension in its own sheath, and finally the nature and size of the body allowed to remain on the surface of the artery, or in the wound. Independently of these physical and material causes, we occasionally meet others in the permanent or temporary condition of the individual who has submitted to the operation, and which it is often impossible to appreciate.

14. From what has been just said of the causes of suppuration, and from the statement in corollary 3, it must be inferred that round and small ligatures ought to be used before all others.

15. Whatever may be the nature of the material employed for the purpose, it is never absorbed; if the external wound should cicatrize before its suppuration, subsequent abscess always takes place to admit of its escape.

16. The secretion of the innermost and middle tunics by a small ligature, does not suffice for the obliteration of an artery; it is necessary it should remain applied on the artery to keep in contact the lips of the section, and to support the first rudiments of the coagulum.

17. After the division of the innermost and middle coats, the continuance of the ligature upon the artery for some hours, does not suffice in man to produce its occlusion, that only takes place in animals whose blood has great plasticity—dogs, for example. In the first place, the adherence of the walls of the artery is so feeble, during the five or six first hours which follow the operation, and in the second the clot is so small, that the impetus of the blood, and the disturbance produced by the extraction of the thread, is sufficient to destroy the one and displace the other.

18. A clot displaced and moved about by the column of blood, soon loses its volume in rolling in the artery; and when it is stopped in its interior, if a portion of blood can glide between it and the artery, it is quickly worn away by repeated attritions. On the contrary, if it completely stop up the artery, or should get entangled in a collateral branch, these vessels become obliterated.

19. It requires a longer time, and is more difficult, to make *torsion* well upon a large vessel than to apply a ligature.

20. In the performance of torsion, the surgeon is obliged to make a strong traction upon the artery, in order to insulate it to an extent of six or eight lines. This displacement extends at least as far as the first large collateral branch, so that the continuity of the vessel with its proper sheath is disturbed in its whole extent, an event which may be attended with unpleasant consequences. This displacement is not to be feared in small vessels, because they have not distinct sheaths, and because further, they give off more branches in a given length of vessel.

21. By the disturbance and the sort of trituration which the torsion produces in the coats of arteries, it results that some of the fragments produced are deprived of vitality. These fragments become a cause of inflammation, which joined to the long and painful manipulations of the torsion, terminate in the production of a suppuration at least as long and troublesome as that produced by ligature.

22. When an artery is divided at a little distance from a great collateral branch on the side of the heart, torsion is less advantageous than the ligature, because it requires a greater length of artery, and consequently diminishes the space necessary for the formation of clot.

23. Torsion practised on small arteries is not followed by the heavy inconveniences which are presented when it is performed on the large ones.

24. When an artery is ossified or cartilaginous, the disease cannot be stopped except by the formation of a clot. If the point acted upon is near a collateral branch, the operation will not succeed. We must, in this case, attempt it above, or in this branch, at the same time with the trunk. This rule applies with equal advantage to ligatures made on sound arteries.

25. When after the complete section of an artery the hemorrhage stops of itself, the surgeon will endeavour to discover the causes likely to disturb the formation and organization of the coagulum, and abstain from all operation, unless, however, by the affinity of the wound with a great branch, he thinks its neighbourhood may obstruct the development of the clot in this case. The artery must be formed, and a ligature placed above the collateral branch.

26. After a wound which has only opened the artery, whether it be longitudinal or transverse, although the hemorrhage stops spontaneously, the surgeon ought always to lay the vessel bare and tie it, because in this case the clot forming without the artery can only produce a temporary cure.

NON-MEDICAL CORONERS.

To the Editor of The Lancet.

Sir,—If it meet with your approbation, I request the favour of the insertion of the following in your next Lancet. On Wednesday, July 25th, a boy named Abrahams was killed in Portsea, and on the coroner's inquest the following particulars were elicited. The first witness deposed to his having seen the boy knocked down by a cart (the horse of which had taken fright), but could not state whether the wheel had passed over the body or not. The next witness, a woman, swore that she saw the horse running away and the boy near; and on again turning round, she saw the boy on the pavement, apparently dead, and the horse and cart quiet. The surgeon, who first saw the boy after the accident, stated there was a wound laying bare the bone on the left side of the os frontis, but no visible fracture; the abdomen was also swelled, and there was the appearance of a contusion over the right hypochondrium; he further stated, that the boy gave one or two convulsive inspirations after he saw him, and then expired. The surgeon, moreover, stated that he could not swear to the cause of death, but requested that an examination of the body might take place. Would you believe it, Mr. Editor, that the coroner stated to the jury, that he had no power to order an inspection; and is it to be believed that the jury, under the coroner's direction, returned the following verdict (although none of the medical gentlemen, amounting to four or five, who saw the body, believed the existence of a fracture), viz., that the deceased died of a fracture of the skull, produced by the wheel of a cart passing over the head ! ! ! !

These facts, for which I can vouch, need no comments; it is scarcely possible to believe, that in these times a coroner should be found, who would thus state to the jury that he had no power to order an inspection of the body of a person whose death may have been occasioned under suspicious circumstances. Moreover it has since appeared, that the boy was knocked down by one of the shafts, and that the contusion over the region of the liver was produced by the ball of the wheel coming in contact with him, and pressing him against the wall. Query, Whence came the evidence of fracture of the brain, and of the wheel passing over the head?

By inserting the above you will much oblige your obedient servant,

A Medical Reformer.

Portsea, August 3, 1832.

PETITION OF DR. HARRISON AND OTHERS TO THE HOUSE OF COMMONS,

COMPLAINING OF ABUSES WHICH EXIST IN THE ROYAL COLLEGE OF PHYSICIANS, LONDON.

To the Editor of The Lancet.

Sir,—I write to desire the favour of you to publish in your next journal the subjoined Petition for the information of your numerous readers.

It was presented to the House of Commons on the 6th instant by Joseph Hume, Esq., and is already printed by order of the House. I am, Sir, yours, very truly,

E. Harrison.

Holles Street, Cavendish Square,
August 21st, 1832.

To the Honourable the Commons of the United Kingdom of Great Britain and Ireland in Parliament assembled.

The humble Petition of the undersigned Physicians

Showeth,—That a College of Physicians exists in London, assuming powers which your Petitioners are prepared to prove have an injurious effect upon the subjects of these realms and the practice of medicine.

That the attention of medical men in every department of the profession has, at length, been roused to the manifold evils proceeding from its defective arrangement, and the sources from which they respectively spring. The great body of British physicians who are excluded from their inherent rights in the College of Physicians, most respectfully submit that it is fitting to inquire into the grounds of their rejection, and to enable them to assert their just claims to participate in its honours and emoluments, and to promote the general interests of the profession.

Your Petitioners beg to state, that the "College or commonalty of the faculty of Physic," sometimes denominated the Royal College of Physicians, was founded by Act of Parliament more than three hundred years ago, for several highly important purposes.

At first its affairs were managed agreeably to the charter, and it then fulfilled the wise and benevolent intentions of the founder. Medical science was assiduously cultivated, and meritorious practitioners liberally encouraged; quacks were punished, and the public health was carefully protected.

In the course of time abuses crept in, by the *rulers* being more anxious to promote private objects, than to enlarge the sphere of professional usefulness. A new policy was then introduced; the most accomplished and best-educated physicians were, to the detriment of medical science, without any just cause, refused admission into the College, and subjected to the most refined and persevering opposition.

Your Petitioners beg to mention, that among the proscribed were many of the most celebrated men, as Doctors Bonham, Levet, West, Schomberg, Askew, Fothergill, &c. Even Sydenham, though an Oxford graduate, was excluded by the College, so indifferent were they to the most sacred duties, and the obligations confided to them for public and private purposes. But the measure of injustice and selfishness was not complete till the middle of last century. At that time the physicians were in quite an arbitrary manner divided into fellows and licentiates, under a by-law still acted upon, though declared to be illegal by Lord Mansfield, chief justice, and his associates on the bench in 1767, in the case of the College against Levet, and in 1768 against Fothergill.

The fellows of the College have thus verified the words of holy writ, "Very truly there was never anything by the wit of man so well devised or so sure established, which in continuance of time hath not been corrupted."

Your Petitioners submit that the College was established to promote medical science, and discharge and accomplish great and highly beneficial objects; but the institution has entirely failed in effecting those desirable results, and the fellows continue to the present time to be so unmindful of their engagements to the public and the great body of physicians, that a country possessing many of the most learned, able, and honourable physicians, is so entirely overrun with rapacious, ignorant, and extortionate quacks, as to have become the jest and scorn of other nations. Even to this hour, and in this enlightened country, their presumptuous audacity fills the newspapers with mendacious advertisements, and their names are daily paraded in the crowded streets, and under the very windows of the College, unresisted and unnoticed.

Your honourable House will hardly believe, that while these enormous abuses pass unredressed, the fellows seek every opportunity to persecute all physicians, although regularly educated and properly qualified in the best schools of medicine in the United Kingdom and in foreign countries, who refuse to submit to their unwarrantable impositions, whatever may be their qualifications and claims to public estimation.

Your petitioners complain that the possession of a University degree, however celebrated the school, which ought to procure confidence, is not only rejected as an incompetent qualification, but considered in the British capital to be the only bar to medical practice.

Your Petitioners submit that the truth of these complaints will more clearly appear on a careful analysis of the charter under which they claim to act, but from which they have in principle and practice wholly departed, insomuch that the Charter is not even recognised in any of their acts.

Your Petitioners hereby declare, that the College of Physicians was instituted by Act of Parliament in 1523 and 24 for three specific purposes, which have for a long time been wholly disregarded by the fellows.

The Charter was granted by King Henry VIII, "to put a seasonable check to the practices of the wicked, and repress the audacity of bad men, whereby divers injuries are done to the ignorant and credulous people. And that the fellows for their own credit and the public good will take care, as well, by their own weight and example, to discountenance the ignorance and the rashness of the before-mentioned persons, as to punish them by our laws lately promulgated, and by the regulations to be made by the same College."

2dly. "To provide for the superintendence and scrutiny, correction and government of all and singular the *physicians practising the faculty of physic* in the said city, or within seven miles round the same; likewise the punishment of them for their faults in the improper exercise and practice of the said faculty."

Also to provide for "the superintendence and examination of all sorts of medicines, to be given, administered, or applied to the liege subjects for the healing and curing of their infirmities, for the advantage and use of the said lieges, so that the punishment to be inflicted on any of the said practitioners in medicine offending in the premises, shall be by fine, imprisonment of their bodies, and other reasonable and proper means."

3dly. "To provide that no person *not being a physician* shall practise the faculty of medicine within the said city, or seven miles round the same, unless admitted by the president and commonalty, or their successors for the time being, under the penalty of five pounds for every month in which he shall have practised the same faculty without such admission."

Your petitioners submit that these were the principal inducements which led King

Henry the Eighth and the legislature to establish the College of Physicians. While its affairs were well conducted, and the objects which were contemplated were effected, harmony prevailed throughout the profession, until a dominant party converted the college into an engine to oppress the great body of the faculty, and to promote their own views. From that time the constitution was entirely changed, and the college ceased to manage its concerns according to the charter, and instead of complying with the wishes and directions of the legislature, the fellows either entirely overlooked, or grossly abused, all the trusts reposed in them.

So long as they were suffered on their own authority to fine and imprison empirics and untitled practitioners, they went on prosperously enough for their own interests, and seized upon every opportunity to increase their revenues by arbitrary mulcts.

These mulcts at first were levied upon *quacks* only, but the spirit of monopoly having found a ready admission into the college, were subsequently levied upon respectable physicians, who were unjustly excluded by the unlawful and arbitrary limitation of the number of fellows, and they, so treated, became, not only the unhappy subjects of the jealousy of their privileged brethren of the college, but degraded in public estimation as far as the college could effect it.

The increase of the metropolis requiring a larger number of physicians than the narrow spirit of the College of Physicians would allow, the original number of twenty fellows was increased to forty, and afterwards to sixty, thus establishing an odious and injurious monopoly, equally prejudicial with the former, and wholly inadequate to the demands of society. All above that number, whatever might be their attainments or qualifications, not only being interdicted practice, but subjected to the most vexatious and unfeeling persecutions.

Towards the middle of last century, however, the number of excluded physicians still increasing, and their favour with the public likewise augmenting, the College of Physicians discovered the necessity of relying on some surer aid to preserve their power.

It was at this period that they devised the scheme of separating the physicians of London into two classes, one of whom, the graduates of Oxford and Cambridge, were admitted as matter of right to the privileges of fellows, while the other class, consisting of the physicians of all other Universities were as unreasonably excluded. Hence the illegal and arbitrary distinctions of "fellow" and "licentiate," which subsists to the present day.

Your petitioners submit, that the better to understand and expose these offensive and injurious proceedings, a reference to the charter will show, that King Henry granted to six physicians, selected out of a company in the city of London, namely, to John Chambre, Thomas Linacre, Ferdinand de Victoria, Nicholas Halsewell, John Francis and Robert Yaxley, that they and all other resident physicians "of and in the aforesaid city, shall become in fact and in name, one body or perpetual commonalty or college, and that the said commonalty or college may for ever elect and make out of that commonalty some man prudent and skilful in the faculty of medicine, for president of the same college or commonalty, to superintend, take cognizance of, and govern, for that year, the aforesaid college or commonalty, and all men of the same faculty and their business, and that the same president and college or commonalty, shall have perpetual succession, and a common seal to be used in the affairs of the said commonalty and president for ever."

It is hardly credible, and yet it is strictly true, that these six physicians had all taken their degrees at Padua or other Italian universities. The College of Physicians was therefore committed, both in fact and in words, by the royal founder, indiscriminately to physicians of every university. Accordingly none had any preference one over another for many of the first years. With respect to Oxford and Cambridge graduates, who have arrogated to themselves all the advantages of the charter, and subsequently contrived, by a series of encroachments, to confine the college to their own body, they are not even mentioned in the charter or act of parliament confirming it. Their usurpation is, therefore, wholly illegal. Indeed, as medical instructions had never been delivered at either university when the college was founded, it would have been preposterous in the highest degree to have endowed them with greater privileges than the universities enjoyed where medicine was carefully and regularly taught.

Your petitioners submit, that it would not have been more preposterous than the practice of the college now is to admit them.

Your petitioners having brought to the notice of your honourable House, that the fellows have entirely disregarded the charter and acts of parliament for many years, as your petitioners allege, and broken every condition under which they were incorporated, beg also further to observe, that it yet remains to point out some of the illegal and vexatious proceedings still in full operation, and of which your petitioners as well as the other licentiates and independent physicians have just cause to complain.

Your petitioners are ready to prove at the bar of your honourable House, that it is not possible at this time, for any person to be regularly and properly educated, either at Oxford or Cambridge University for the Medical Profession.

1st. That while it can be proved, that the fellows have in so many ways violated their duty to the public and the faculty, they have been ever ready and forward to promote their own emoluments and aggrandizements.

Your petitioners allege, that a careful attention to the whole of their proceedings for the last two hundred years, will satisfy your honourable House, that their conduct has been more for the selfish views and profit of the fellows, than the just and important interests of the community.

Your petitioners submit further, that these objects have been artfully promoted by dividing physicians into fellows and licentiates, in direct opposition to the acts of parliament and charter of incorporation.

By this palpable infringement, Oxford and Cambridge graduates have acquired a preponderating influence over the rest. Not satisfied with engrossing all the honours and advantages of the college, the fellows have contrived to burden the degraded physicians with extravagant fines, and a most humiliating confession upon their bended knees, which both parties know to be false, of their comparative inferiority and unfitness to manage the cure of various diseases.

Upon the distribution of physicians into fellows and licentiates, it is worthy of remark, that Lord Mansfield observed in 1768, "the licenses probably took their rise from that illegal by-law (now at an end) which restrained the number of fellows to twenty. This was arbitrary and unjustifiable. They were obliged to admit all such as came within the terms of the charter, yet it is probable that the practice of licensing was in consequence of their having made that illegal by-law."

Your petitioners submit, secondly, that by another absurd and vexatious regulation the college has actually superseded the law of the land. In an act of the legislature, dated the 32d of the same reign, it is provided, that "And forasmuch as the science of physic doth comprehend, include, and contain, the knowledge of surgery as a special member and part of the same, therefore be it enacted, that any of the said company or fellowship of physicians, being able, chosen, and admitted by the said president and fellowship of physicians, may from time to time, as well within the city of London as elsewhere within this realm, practise and exercise the said science of physic in all its members and parts, any statute or provision made to the contrary notwithstanding."

It would be foreign to the objects of your petitioners to discuss the advantages and the inconveniences which physic and surgery received or would now receive from combining the practice of both in the physician. It is enough for your petitioners to show, that physic and surgery were so cultivated, from the foundation of the college, till the above-recited act of the legislature was superseded and defeated by a private regulation of the fellows, which requires that its members shall wholly abstain from the practice of surgery.

It is well known, that the greatest ornament of the college, the excellent Dr. Harvey, practised physic, surgery, and midwifery, for many years. He also gave a course of instruction in anatomy and surgery annually at the college. These lectures are dearer to Englishmen, because through them the circulation of the blood was first discovered by our immortal countryman.

Lord Lumley and Dr. Caldwell had each of them left a sum of money to the college for this purpose, and Dr. Harvey was one of the early professors appointed to deliver these lectures. It is therefore desirable to know who now receives the sinecure emolument, and why the lectures have been abandoned. It is enough for your petitioners to have shown that in preventing physicians from exercising surgery along with physic, the fellows have virtually repealed an important statute of the realm.

Your petitioners have, they presume, clearly proved to your honourable House in the foregoing detail, that the charter is in itself admirably fitted to protect the metropolis against dangerous and unprincipled pretenders, and to provide the apothecaries' shops with genuine drugs.

It would greatly afflict your petitioners, and distress your honourable House to add to this statement, the probable amount of lives prematurely destroyed, and the aggregate mass of affliction, which during three hundred years have proceeded from the culpable inactivity and carelessness of the college. But recent calamities, fresh in the minds of all who pay the slightest attention to passing events, sufficiently declare the grievous extent and great frequency of evil arising from this source alone.

As the anxiety of your petitioners is directed more to provide for the future than to condemn the past, they willingly leave the charter without further comment, to consider the legislative care bestowed upon the country in the same reign. They, therefore, desire to call the attention of your honourable House to another enactment, which seems to contain matter for grave inquiry, if not for severe reprehension.

In the concluding clause of the act of 14th and 15th of King Henry the Eighth, it is provided, "And whereas in dioceses of England, out of London, it is not light to find always men able sufficiently to examine (after the statute) such as shall be admitted to exercise physic in them, that it may be enacted in this present parliament, that *no person* from henceforth be suffered to exercise or practise in physic through England, until such time as he be examined at London by the said president and three of the said elects, and to have from the said president or elects, letters, testimonials of their approving and examination, except he be a graduate of Oxford or Cambridge, which hath accomplished all things for his form without any grace."

Your petitioners cannot repress their astonishment that a provision essential to the public safety should have remained to this time a dead letter in the statute-book, and affirm that the fellows dare not affect to conceal their criminal indifference under the garb of ignorance, because they have repeatedly boasted, that they possess under it an effective dominion over all the practitioners in England. With such exultations, and the act of parliament constantly in view, your petitioners may reasonably inquire what difficulty hath hitherto prevented them during so long a period from enforcing their authority? Have they been too busily engaged in subduing the great body of London physicians to cast a thought upon the country? Or do they entertain so mean an opinion of the provincial inhabitants of the kingdom, as to think them unworthy of protection? Whatever may have led them to overlook their sacred obligations to the people, and to neglect their most valuable concerns, it is to be hoped.

And your petitioners humbly pray, that your hon. House will take into consideration the constitution and conduct of the College of Physicians, and investigate the nature of the charter and acts of parliament under which they have claimed to act and provide, in justice to the community at large, and the British physicians in particular, in such manner as shall in the wisdom of your honourable House be deemed best.

And your petitioners as in duty bound will ever pray, &c.

LONDON COLLEGE OF MEDICINE.

To the Editor of The Lancet.

Sir,—The proposals for the establishment of the London College of Medicine were hailed by myself, and I am sure by all enlightened members of the profession, as a new era in the annals of medical legislation. It is now about fifteen months since the regulations of the proposed college were published in a number of your scientific journal, and I blush for the profession's disregard of its rights and interests, when I feel that the time when those salutary regulations should be put into operation, and the London College of Medicine permanently established, is nearly as distant as when you first suggested its institution.* What has retarded its progress I cannot conceive. Medical men individually admire its regulations and extol its principles, but the members of the profession collectively, though they cannot condemn a single iota of the plan, have betrayed a lamentable want of zeal and co-operation, not having awarded all their exertions, and used all their interests, towards the completion of so noble a design. I am aware that there are a *select few* whom we must expect to oppose an institution such as the London College of Medicine, but their motives are known as also are their principles, and whatever they may do in their endeavour to crush the friend of liberty and consequent subduer of monopoly, will only be in character with what you have so frequently and so ably exposed. I have waited patiently, hoping to hear through your journal that the machinery was in action, and regret that you have said so little on the subject. I therefore call, through the medium of your columns, on every one who may read this letter, to give his most strenuous and immediate support to the College, not, however, until he shall have taken the trouble to peruse the principles upon which it is founded, as I feel convinced that no man possessing the least discernment can fail to see their perfections; if, after perusing, he disagree with them, and consider certificates of having paid a certain quantum of *money to certain men* a better mode of examination than proof of possessing a certain quantum of medical and general knowledge, he must be divested of common sense, or influenced by party spirit,—he must, in fact, be either in intellect a fool, or in principle a bat.—I am, Sir, your most obedient servant,

A. M. A. B.

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* Our correspondent is deceived. The College is established, and the state of its funds most prosperous.—Ed. L.

THE LANCET.

London, Saturday, September 1st, 1832.

Of all the disgusting specimens of imbecile legislation, the *Apothecaries Act* of 1815 stands probably the most prominent. Divested of every comprehensive view of the subjects embraced in its details, that odious document left the portals of the legislature one mass of unsightly confusion. The promoters of the bill, however, had *their* objects in view, and, unfortunately for the profession and the public, succeeded, through the influence of their dupes in Parliament, in accomplishing their scandalous purposes. If the members of the legislature had understood the question, the bill, on its very first reading, would have been trampled on the floor of the House. Medical legislation, however, was a new occupation; the heads and hands of our senators were unused to that description of labour, and hence the deplorable botch which they made of their work. But it does appear passing strange that the members of the profession generally did not interfere to prevent such a bill from becoming a portion of the law of the land, though the indifference displayed on that occasion may in some respects be accounted for when it is considered that regularly-educated practitioners were so unceasingly exposed to the impudent pretensions of quacks and other impostors, that the general body of the profession were induced, possibly, to believe that *any* change in the law would operate to their advantage.

Independently of the objections which may be offered against the extraordinary powers thrown into the hands of a company of retail traders,—independently of the imperative stipulation for an apprenticeship of five years,—exclusive of the commanding position which the retail drug company was allowed at once to assume over the Colleges of Physicians and Surgeons,—independently of the *veto* which was contained in this Act against regularly-qualified practitioners who had obtained their diplomas from the medical and surgical colleges of Dublin, Edinburgh, and the celebrated Universities of the continent,—we are compelled nevertheless to regard the *fourteenth* and *twentieth* sections of the Act as the most odious of the whole of its features.

The lying "preamble" of the hated measure, alleges that the Act was "for the better regulating the practice of apothecaries throughout England and Wales;" whereas it was, in reality, designed to *prevent* the most competent individuals from discharging the functions of apothecaries. Extensive and perplexing has been the misconception with respect to the legal construction which ought to be put on the clauses just mentioned. Here then we will recite portions of those clauses verbatim, but not without first placing before the eye of the reader a note which has been addressed to us by an intelligent correspondent.

"APOTHECARIES HALL.—ARMY AND NAVY.

"*To the Editor of* The Lancet.

"Sir,—In your last Lancet you state, in reply to a correspondent, that the medical officers of the public service before 1815, are not entitled to practise as apothecaries, an assertion calculated to do great injury to a great body of general practitioners, which I am sure you do not intend, and which you will be ready to correct in your next Journal. The Court of Examiners promulgated a resolution to the following effect:—That the medical officers of the navy and army, whose appointments took place prior to the 1st of August 1815, would be considered as exempt from the operation of the Act, and that a certificate of qualification was unnecessary. The 'Declaratory Act' was to remove all doubt as to those who came after August 1815, and disgraceful is it to the Government to expose their officers to doubt, by allowing the act to expire.

"Allow me to ask, Mr. Editor, and I do it in the name of many, who see reform necessary, and wish the LONDON COLLEGE OF MEDICINE success—in what light the medical officers of the King's service are viewed, and what rank will be, or is, intended for them in that institution? It is remarked with surprise that you have observed solemn since on that head.

"AMICUS.

"Pimlico, Aug. 20, 1832."

Now, then, for the extracts from the fourteenth and twentieth sections of the Act.

Clause 14. "That from and after the 1st day of August 1815, it shall *not be lawful* for *any person or persons, except persons already in practice as such*, to practise as an apothecary or apothecaries in any part of England or Wales, unless he or they shall have been examined by the said Court of Examiners, or the major part of them, and have received a certificate of their being duly qualified to practise."

Clause 20. "And be it further enacted, that if any person, *except such as are then actually practising as such*, shall, after the said 1st day of August 1815, act or practise as an apothecary in any part of England or Wales without having obtained such certificate as aforesaid, every person so offending shall, for *every such offence*, forfeit and pay the sum of *twenty pounds*."

There is yet another section which we must quote; viz.—

Clause 21. "And be it further enacted, that no apothecary shall be allowed *to recover any charges claimed by him in any court of law*, unless such apothecary *shall prove on the trial* that he was in practice as an apothecary *prior to*, or *on*, the said 1st day of August 1815, or that he has obtained a certificate to practise as an apothecary from the said master, wardens, and society of apothecaries, as aforesaid."

After a careful perusal of these extracts from the Act itself, our correspondent, we believe, will be quite willing to admit that we did not fall into any error when we offered it as our opinion that medical officers who had received their appointments in the navy before August 1815, could not legally act as apothecaries in England or Wales. Our opinion on this subject having been called for, we were bound to state it *truly*, no matter how indignant were our feelings against the framers of such a law—no matter how strong was our dislike of the enactment itself. But our correspondent informs us that "the company of apothecaries promulgated a *resolution* to the effect that naval and military officers who received their appointment prior to the 1st day of August 1815, would be exempt from the operation of the Act." No information of the existence of such a document ever before reached us, and we now believe that "*Amicus*" labours under a mistake when he supposes that any "resolution" bearing that purport was ever promulgated by the Company. Undoubtedly there are sufficiently strong grounds for inducing us to yield a ready credence to reports tending to prove the extreme ignorance of the Court of Examiners, but it is next to impossible that any men who are permitted to walk outside the walls of a lunatic asylum can have published such a resolution as the one described by our correspondent. Surely the examiners know that it is their duty to *administer* the law with respect to examinations, and to prosecute, upon information, unqualified practitioners, and that they have no greater power to alter the provisions of the Act itself, whether the restrictions operate against naval or military surgeons, or against the fellows or licentiates of all the medical colleges, than has the beadle who stands as sentinel at the door of their hall. If an action were to be instituted agaist a gentleman who acted as a surgeon in the army or navy previous to the 1st of August 1815, for practising as an apothecary—which, according to the definition of Mr. Baron Bailey, and several others of the Judges, consists in visiting and prescribing in medical cases, and supplying the medicines—would this "resolution" of the Company have the effect of non-suiting the plaintiff, or of inflicting the full penalty of 20*l.* on the defendant? Again, if a naval or military surgeon, having no other authority for practising as an apothecary, were to bring his action in one of the courts of law for recovery of a debt on account of services performed by him *as* an apothecary, would the "resolution" of the Court of Examiners supersede the necessity of proving, on the trial, that such apothecary was in practice *as* an apothecary prior to, or on, the 1st of August 1815?

All that can be said on the subject is this, that if the company have really issued such a "resolution," they are about the most incompetent of all living men to discharge the duties of their offices. The law must be taken as it exists, notwithstanding the gross, flagrant, cruel injustice of its operation. Not only did that Act, by raising up an insurmountable barrier, prevent naval, military, and all other surgeons,—not actually in practice as apothecaries before, or on, the 1st of August 1815,—from practising as apothecaries after that period, but it presented an obstacle of not less extent or difficulty to the entire body of physicians practising in this metropolis. And if it be admitted that physicians who may have been in practice twenty years before 1815, cannot *now* practise as apothecaries without subjecting themselves to the penalties of the Act, is it possible that gentlemen who received their appointments as *surgeons*, before that period, are exempt, on the performance of *medical* duties, from penalties which may to a certainty be imposed on physicians? But the words of the Act are too clear to admit of dispute, or to call for further discussion. On perusing the foregoing extracts, "*Amicus*" will acknowledge that the error existed in the statute, and not in the opinion published in this journal.

Our correspondent, however, believes, that the "Declaratory Act" was intended "to remove all doubt as to the rights of those who came *after* August 1815." The words of the fourth clause of that document must convince him that the limit of disqualification was not fixed at so recent a date as 1815. The words of the Act are these: "And whereas many persons who have *heretofore* held, and now do or who hereafter shall hold, commissions in his Majesty's service as surgeons, either in his Majesty's navy, or as surgeons or *apothecaries* in his Majesty's army, *cannot commence practice as apothecaries* in any part of England or Wales, without being examined by the Court of Examiners, or being *liable to the penalties* imposed on persons who, not having been *in practice as apothecaries* on the first day of August 1815, &c. &c." Thus it is quite evident, that this "Declaratory Act" passed in 1823, was intended as much to relieve naval and military sugeons who had received their appointments *previous* to 1815, as subsequent to that period. That the existing state of the law is most disgraceful to the government and the legislature generally, must be conceded by every member of our profession.

On quitting this branch of the subject, "*Amicus*" asks in what light medical officers of the King's service are viewed, and what rank is intended for them in the London College of Medicine, and he proceeds to say—"It is observed with surprise you have maintained a solemn silence on that head." Our efforts in defending the rights of naval and military surgeons, and our conduct generally in attempting to rescue the officers of one of the departments of public service from unmerited degradation and insult, must, we should think, exonerate us from any charge of either in-ended or implied neglect as respects those gentlemen. The LONDON COLLEGE OF MEDICINE being an institution founded upon a just basis, and the whole of the details of its arrangements pointing to professional honour and public utility, the question of rank relating to any branch of the profession is best furnished with an answer by that clause which secures to the whole of the admitted fellows equal rights and privileges. And surely it was not necessary to make an especial declaration in favour of naval and military surgeons, when the very first of the laws of the College stipulates that every gentleman legally-qualified to practise, in *any* branch of the profession, is deemed an eligible candidate for the diploma. "*Amicus*" will be pleased to learn, that the laws of the College are already so well understood and appreciated by the medical officers of the army and navy, that the names of several highly-respectable gentlemen, belonging to both departments, are included in the list of Fellows. Our correspondent could not, we are quite certain, labour under an impression to the effect that the committee of management had passed slightingly over the prominent professional claims of military and naval surgeons. The LONDON COLLEGE OF MEDICINE boasts of a constitution which may challenge the utmost scrutiny of the whole medical public. Its laws are framed with an undeviating regard to the successful promotion of medical science, and the consequent health of the community. Hence it stands pre-eminently entitled not only to the encouraging support of the independent or liberal branches of the profession, but also to the immediate attention and protection of the legisture.

THE petition presented to the House of Commons from Dr. HARRISON, and other physicians, will be found in this week's LANCET. The petitioners have trodden upon a debateable, if not an unsound, foundation, and we think it not difficult to prove that to have acted consistently with the opinions which he entertains of the efficiency of the charter held by the College of Physicians, Dr. HARRISON and his co-petitioners should have applied to a court of law, rather than to the legislature.

AN abstract of two papers on the treatment of the worst stage of cholera, by Dr. HARDWICK SHUTE, will be found in this day's Journal.

In forwarding papers to the medical press, the CENTRAL BOARD OF HEALTH do not, of course, consider themselves in any way responsible for the theories or plans of treatment adopted by the authors. In giving publicity to the documents transmitted to them, the Board, we apprehend, consider that they are rendering an essential service to the profession and the community, in making known as widely as possible the effects of the different curative means resorted to, by probably some of the most experienced and best-qualified practitioners in the world.

WE are sorry to say, and we do so without feeling called upon to enter into any particulars on the subject, that the cholera is slowly, but surely, on the increase in this metropolis.

Surgical Pathology, a Thesis. By JULES CLOQUET, &c. Translated from the French by J. W. GARLICK, M.R.C.S., and W. COPPERTHWAITE, M.R.C.S., With Plates. 8vo. pp. 223. S. Highley. London. 1832.

CONVINCED as we are that instruction in pathology and morbid anatomy should form a most essential part of medical education, we are prepared to give every encouragement in our power to works devoted to the useful purpose of rendering these branches of the science more accessible to the student. It has been very generally acknowledged, that neglect of pathological anatomy in this country has contributed in a great degree to retard the improvement of medicine, and to substitute a false and uncertain basis, for the unerring and satisfactory grounds afforded by an acquaintance with the organic lesions produced by disease, and by observation of the symptoms or signs preceding or accompanying such deviation of structure. For many years, the only regular course of lectures delivered in the country upon pathology and morbid anatomy was that which was, and is still, given by the professor of anatomy in the University of Dublin. This example was followed, though not till lately, by the University of Edinburgh; and the University of London has recently appointed to the chair of pathology, a gentleman fully qualified to do justice to the subject which he has undertaken to teach.

In the little volume now before us, we expected to find at least a succinct account of the most prominent changes produced by diseased action in the different structures of the human body; in this we have been disappointed; the work is, in fact, nothing but the translation of a thesis written by M J. Cloquet, apparently as a candidate for one of the chairs in the faculty of medicine; it might be aptly termed a discourse upon surgical pathology, and would form a good article for the *Quarterly* or *Edinburgh*, but has no pretensions to the character of a work designed for conveying preliminary knowledge to the student.

Indeed it is evident from the brief preface of the author, that he has not directed his aim at this object. "In the first part," says he, "I have endeavoured to determine the rank which pathology ought to hold among the other branches of medicine, and to point out the divisions which the science has received. In the second part of the thesis, I have shown the arrangement of a course of lectures. The plan which the Professor should adopt in teaching the science, and the means which are best calculated to ensure a successful result for his labour."

It is ever more pleasing to us to speak favourably than otherwise of the productions of a young author; in the present instance we must declare the translator seems to have mistaken altogether the object of Cloquet's thesis, and feel it incumbent on us to explain the real nature of the work, lest the student experience the disappointment arising from the purchase of a work supposed to be upon one subject, while he makes the discovery that it has been written upon another.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

BY

MR. BRODIE,

Delivered August 6, 1832.

ABSCESS IN THE AXILLA.

IN a case of this nature, where the tumour had pointed anteriorly near the edge of the pectoralis major muscle, and had been opened, Mr. Brodie ordered that the man's arm should be hung in a sling, and fastened to his side; "For," remarked Mr. Brodie, "the abscess is so situated, that if any motion be allowed, it will not heal."

FUNGUS OF THE TESTICLE.

There is a patient in the hospital now under the care of Mr. Brodie with this fungus. Mr. Brodie ordered him to be kept in the horizontal position; "For," said he, "the erect position only tends to fill the vessels of the part with blood, which I think the horizontal position will prevent."

DISEASED URINARY ORGANS.

In a case of stricture of the urethra, diseased prostate gland, and, (as Mr. Brodie believes) disease of the kidneys also, in which the urine is alkaline, Mr. Brodie ordered,

Acid. nitric. dilut. ʒiij;
Tinct. cinnamomi ʒiv;
Aq. puræ Oss; to be taken every day in three doses.

Mr. Brodie hesitated as to what tincture he should order in conjunction with the acid, and asked Mr. Pollard (whose turn it now is to take charge of Mr. Brodie's patients) what tinctures they kept in the shop, specifying the tinct. cardam. comp. Mr. Pollard's answer was, that *they did not keep it*.—What a perfect Augean stable is this shop!

GLANDULAR TUMOUR.—IODINE.

We saw a few days since the patient, whose enlargement of the thyroid gland, in which Mr. Brodie tied the thyroid artery, we noticed in a former Number. The tumour has greatly diminished in size, and the patient's appearance is in every respect much improved since the operation. Mr. Brodie is now giving him large doses of the liquor potassæ; he now takes ʒi daily, and Mr. Brodie means to increase it to as great a quantity as his stomach will bear. In answer to a question from Mr. Fernandez respecting the renewed growths of these glandular tumours after the iodine has been discontinued, Mr. Brodie said that they had not a fair chance of judging if such were the case or not, with patients in a hospitol, as when they left it they were lost sight of. Mr. Brodie's opinion is very strong against using iodine so generally and indiscriminately as he says it is used, as, for instance, in inflammation of the synovial membrane of the knee joint; it is tampering, he says, with a very violent medicine, for it has been known to produce paralysis, and he considers the practice "highly disgraceful."

OPERATIONS.

On Thursday, Aug. 8, there were three operations performed. One was a case of stone in the bladder in a child (boy) four years old. The stone was about the size of a kidney-bean of the mulberry kind, with an internal rough coating of lithic acid. During the performance of the operation, the child was held by the surgery-man, but Mr. Brodie remarked afterwards, that the plan was not so good as binding the patient.

The next case was one of necrosis of the tibia. Mr. Brodie cut down upon the bone, and extracted a few dead fragments, and applied the trephine twice. After the operation, Mr. Brodie addressed the pupils. "This case, Gentlemen, is one of disease of the tibia, from which I have extracted some dead portions; and I should suppose that, at a former period, there must have been also great effusion of fluid into the knee-joint (caused, no doubt, by this disease of the tibia), from the present very lax state of the parts. In extracting the dead portions of bone, I discovered two sinuses in the bone, the one leading upward—the other downward. I have applied the trephine over each of these, and thus laid them open, and I do not feel myself warranted in proceeding any further with the disease to-day."

There are at present some cases of violent secondary symptoms of syphilis in the hospital, under the care of Mr. Brodie and Mr. Babington, in which they are using the concentrated syrup of sarsaparilla; that which is prepared at the Hall was first used as being the cheapest (7*s*. 6*d*. per pint), but it was not found to succeed. Mr. Hudson's concentrated syrup was then tried, and was found to succeed much better. It is, however, a very dear success for the hospital funds, Mr. Hudson's preparation costing 18*s*. 6*d*. per pint. Mr. Brodie is using it in a case of syphilitic ulcer of the tarsal cartilage.*

LONDON HOSPITAL.

RETENTION OF URINE ARISING FROM ENLARGEMENT OF THE PROSTATE GLAND.

J. O., labourer, ætat. 72, admitted on the night of the 22nd of July, suffering under retention of urine; he stated he had been suffering for several years from this complaint, and had been several times a patient in the hospital for this disease; he had not passed any water for thirty hours previous to his applying here. On examination the bladder was found to be much distended; no stricture could be discovered; the retention arising from the very great enlargement of the prostate gland, particularly the middle lobe. The catheter was introduced night and morning, and the urine drawn off, which was mixed with a quantity of blood; his general health appeared to be very good until the morning of the 27th, when he complained of being unable to move himself, particularly to the left side; he appeared rather low; pulse weak and tremulous; was ordered eight ounces of wine every hour at night; his mind became wandering, was restless, throwing his arms about, and at eleven o'clock on the morning of the 29th died.

The body was examined the following day (without any notice) during the time we were in the wards; consequently we cannot give any account of the appearances on dissection; it is scandalous that no notice is given to the pupils when a body is about to be examined; if the surgery beadle should have orders to put up one when a post-mortem examination is to be held, the only parties who know anything about holding them, are the assistant-surgeon and the dresser of the patient.

RETENTION OF URINE.

The following case, although it admitted of speedy relief, is interesting.

A boy, about three years and a half old, was brought to this hospital on the 20th of July, under the following circumstances:—

The mother of the child stated that he

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* Many anecdotes are in circulation respecting Mr. Hudson's paying a per-centage upon prescriptions, &c.

had not passed any urine for nearly forty-eight hours. Upon examination, the bladder was found to be very much distended, giving considerable pain to the patient when pressed upon. In the lower part of the perineum, on the right side, were found considerable redness and swelling, which at first led to the belief of extravasation of urine. A catheter was with some little difficulty passed, and no stricture was found; nearly a pint and a half of high-coloured urine was drawn off. The mother refusing to allow the child to remain in the hospital, a dose of castor oil was given to it, and eight leeches were applied to the inflamed part in the perineum, and the mother was directed to apply fomentations.

The child was again brought here the following day, when a fluctuation was evident in the perineum; it was conjectured that an abscess had formed, and that the pressure of matter in the urethra occasioned the retention of urine. Upon passing a lancet into the tumour, a quantity of matter was discharged; a poultice was ordered to be applied. The mother took the child away with her, still refusing to allow it to remain in the house. Two or three days afterwards she brought the child again, when it was able to pass its urine freely; the wound in perinæo had healed up.

CUT THROAT.

W. A., ætat. 66, admitted 16th of June under the care of Mr. Scott. He was a tolerably healthy man until about two years since, when he began to labour under great depression of spirits, but was unable to assign any cause for the infirmity. He has lived rather abstemiously for a man in his situation of life. During one of his melancholy paroxysms, he drew a razor across his throat. On his admission into this hospital, it was found he had made a deep incision between the os hyoides and thyroid cartilages; the ligaments and muscles between the two had been cut, and the finger could be passed into the wound as far as the mucous lining of the larynx, which, when touched, caused a distressing sense of suffocation. Deglutition was performed without much difficulty, but the patient spoke with reluctance, on account of the alarm it excited in his mind. The wound was covered by a coagulum, but this being removed, a branch of the superior thyroidal artery was found to be divided, and required to be secured.

The edges of the wound were approximated, and retained, as nearly as possible, in apposition, by bringing the head forwards, and confining it in that position by bandages; he was ordered to be kept perfectly quiet, and to take the following:—

℞ *Tinct. opii,* ♏x;
Sp. ammon. comp. xx;
Mist. camphoræ ℥iss *omni 2nd hor.*

He continued without any bad symptoms until the 21st, when he became feverish, and complained of pain in the head, for which twelve leeches were applied to the forehead; the saline effervescing mixture to be taken every four hours. The following day he felt much relieved by the leeches, &c. The wound in the throat looks very well. To continue the mixture.

23. Is very low, with a weak pulse; tremulous tongue; was ordered

℞ *Ammon. carb.* gr. v;
Mist. camphoræ ℥iss *ft. haustus 6tis hor. sumend.*

From this time he gradually improved; he occasionally complained of pain in his head, which was removed by the application of a blister, and he was discharged on the 26th of July with the wound quite healed.

LONDON HOSPITAL—MR. ANDREWS.

Sir,—A very serious misunderstanding took place in the wards of the London Hospital on Monday last, between Mr. Andrews and a gentleman who passed the college on the last examination night. It appeared that Mr. Andrews on the day following the examination in question, came to the hospital, and made some severe and sarcastic observations before the class, against the gentleman alluded to, and even went so far as to say, that the examiners rejected two candidates on the preceding night, and *that if they had done their duty, the gentleman alluded to ought to have been amongst the number.* I really think that this is "too bad." After taking from his pocket 22*l.*, and sending him forth as a fit and capable surgeon, no remarks ought to have been made. I send you my name with this letter, and remain, Sir, your obedient servant,

Junius.

August 15th.

TO CORRESPONDENTS.

Chirurgicus.—The cost of the advertisement will be 9s.,—the customary charge.

Mr. Spilsbury's case is not drawn up in a form adapted for publication.

We have read the articles in the *Sunderland Herald*, *Cork Advertiser*, *Shrewsbury Chronicle*, *Bolton Chronicle*, *Tyne Mercury*, and *Leeds Patriot*, which were intended for our perusal.

THE LANCET.

VOL. II.] LONDON, SATURDAY, SEPTEMBER 8. [1831-32.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

LECTURE XLIII.

THE SEVENTH PAIR OF NERVES — THEIR DOUBLE ORIGIN AND FUNCTION — THE SYMPATHETIC OR GREAT ORGANIC NERVE —ITS ORIGIN — UNIVERSAL CONNEXION —PRESIDING OVER THE FUNCTIONS OF NUTRITION, SECRETION, &c.

WE are now prepared for the consideration of the seventh pair of nerves, or, as it has been strangely called, the *portio dura* of the seventh, as if it were not essentially distinct from the auditory nerve in origin, structure, course, and function.

Its Double Origin.—There needs only a moment's inspection to convince you that this nerve arises from two distinct roots;—one from the central column of the medulla oblongata or inferior surface of the spinal chord,—and the other from the side of the compressed corpus olivare, or what we should have termed the corpus restiforme or lateral column in the human being. We can plainly trace the superficial course of the inferior portion across the medulla oblongata, and parallel with the pons varolii, and separated from it by a little sulcus; and the nerve or the inferior portion of the nerve is the evident prolongation of this medullary chord. With a little trouble we can dissect away and turn over this inferior portion of the nerve, and then we have other fibres as plainly springing from the side of the medulla oblongata, running parallel with the first, comprised with them in one common sheath, and combining to form the seventh nerve.

Structure of the Nerve.—Minute anatomists have spoken, and with perfect truth, of the difference in the internal structure of different nerves. This is sufficiently plain in the larger nerves. The continuation, as it were, of the pulpy matter of the brain in the nerves of pure sensation—the distinct filaments of the fifth pair—the single furrowed chord of the cerebro-visceral, are well known; so in this seventh pair, and chiefly in the inferior portion of it, the chords of which it is composed can be traced without the aid of a lens, and also the minute filaments running from one to another, and giving to the whole a plexus-like appearance. This is a division of minute anatomy which has been hitherto neglected. Considering the greater development of the nerves, both animal and organic, in our patients, we have superior opportunities for inquiring into it; and, when we have a little emerged from that obscurity or disrepute under which the students of veterinary medicine have hitherto laboured, the possible connexion between the structure and the function of the nerves, may be one subject, and far from being the most important one that may be entrusted to our research; and, if it should hereafter appear that our practice is based on anatomical facts, and strict physiological deductions, and close observance of their bearing on disease, the pathology of domesticated animals will be acknowledged to be one of the most useful auxiliaries in the study of human medicine.

The Course of the Seventh Nerve.—This nerve emerges from the skull through the meatus auditorius internus, inclosed in the same sheath with the eighth pair (the portio mollis or auditory nerve). The eighth soon runs its course, as already described; but the seventh, when passing through the spiral canal in the temporal bone, and at the bottom of the meatus, gives off two important branches somewhat differing from those described by human anatomists. The first answers nearly to the *chorda tympani* or nerve of the tympanum, for it passes across that cavity, and along the handle of the malleus, or rather, between it and the incus; it then escapes from the tympanum through a small foramen appropriated to this purpose, descends towards the tongue, and unites with the lingual nerve. The other branch also passes through the extremity of the tympanum—it follows the direction of the eustachian tube—anastomoses with the vidian nerve of the fifth pair, traverses the suboccipital ganglion, and, deeply seated beneath the temporal bone after its escape through a foramen in the sphenoid bone, it distributes its ramifications over the neighbouring parts.

The main trunk of the nerve, passing from the spiral canal, is found behind the condyle of the lower jaw; and there it first assumes the character of a kind of gangliform plexus, and ramifies into various branches. Three go to the different parts of the ear,—the *anterior* branch supplies the muscles of the anterior part of the ear, constitutes another plexus before and at the base of the scutiform cartilage, anastomoses with the lacrymal branch of the ophthalmic nerve, and loses itself in the neighbouring integument and muscles;—the *posterior* auricular goes to the back of the ear, and supplies the muscles and integument there; while the *internal* auricular enters the conch, and ramifies over it. There are various parotideal branches which lose themselves in the upper part of the parotid gland, and the integument over it. And now, the main trunk of the eighth nerve, joined by a branch from the posterior maxillary nerve, emerges from the parotid gland, turns round the cervix of the jaw, and appears upon the outside of the temporal artery. This is the place where it has usually been divided for experiments on it as the motor nerve of the face. The beating of the artery will always guide you to it. It now becomes the proper facial nerve. Its branches reach to every part of the face, from the forehead to the extremity of the muzzle; while some of them, taking a descending as well as posterior course, unite with the cerebro-visceral, the spinal accessory, the great organic, and the first and second cervical nerves.

Difference of Distribution.—There is a considerable difference in the distribution of the facial branches in different animals, and that depending upon the variety of the motions of the lips, or the powerful action of the muscles of the jaw and neck. Observe in the horse the profuse distribution of the nerve about the nostrils and the muzzle, accounting for the beautiful play of the nostril in the blood-horse, and the varied action of his lips and muzzle—the principal seat of the sense of touch in him. Next observe the far fewer branches in the dog, whose lips and the alæ of whose nostrils have comparatively little motion, and the plexus of ramifications going to the muscles of the jaw and even of the neck, which are concerned in the various and powerful action of the jaws, and the motions of the head generally, and of the neck. Again, observe the multiplicity of fibrils directed to the ear of the horse, compared with the same part of other animals, illustrating the varied play and the speaking action of the ear in this noble animal.

The Function of the Nerve.—I know that the seventh is the voluntary motor nerve of the face—of that there can be no doubt. The motions of the head—the action of almost every part of the face—the expression of the countenance, depend partly or entirely on the influence of the seventh nerve; and all the muscles of the face, the head, and the external part of the neck, are perfectly under the command of the will. A great many of the muscles of the face, however, are connected with the discharge of the organic functions—voluntarily so far as mastication is concerned—voluntarily in many an occasional act connected with respiration, as coughing, sneezing, sighing, talking; but involuntary and unconsciously in the common process of breathing; and involuntary and violently, yet consciously, in the deep and rapid breathing of exercise or disease.

The double origin of the seventh nerve, from the central inferior, and lateral column of the medulla oblongata, enables me to account for all this with tolerable satisfaction. From the motor column I derive the power of voluntary action; from the lateral column the involuntary action of some of the muscles in the performance of an organic function, and that varying to a considerable degree, according as circumstances may require; and from the union of both, I have the power of exciting those muscles to more energetic and more widely combined action, for the discharge of this function when I please, or under circumstances of difficulty or disease. It is like the combination of the spinal accessory with the nerves of voluntary motion from the spinal chord, and which causes certain muscles of the neck and shoulder to be either subservient to the will, or employed in respiration, or both, as circumstances may demand; but the two roots, or sources of nervous influence, are here brought nearer together. I acknowledge, Gentlemen, that this is somewhat a new view of the character and function of the seventh nerve. The double origin you will not deny; the function of the nerves, from the inferior central and from the lateral column, seems to be tolerably well established; and it will be for you to inquire what better explanation you can give of the office of a nerve springing from both these columns.

The Upper Cervical Ganglion.—At the base of the cranium, beneath, and in front of the atlas, we find a pyriform reddish body, of a somewhat soft consistence, and differing in bulk in various animals. It gradually contracts as it proceeds down the neck, and at length terminates in a chord or nerve. In the horse it is scarcely an inch in length before it has diminished to a mere chord—in the human being it is said to extend from the skull to the transverse process of the third vertebra. Before it takes on the character of a chord or nerve, it either sends numerous filaments to neighbouring and even distant parts, or it receives them, or both, and particularly as it regards the cranial and spinal nerves.

There is a communication with the sixth pair, but not of that important character that some have represented. A branch of the great organic was ramifying, or forming a nervous plexus over the carotid artery within the cavernous sinus—the sixth nerve passes over the artery at the same place,—the two nerves come in contact with each other—they exchange several fibres, and then both of them continue their course. Professor Mayo has given a very satisfactory representation of this in his outlines of human physiology. There are two communications with the fifth pair, and of the same character, and accompanied by an exchange of fibres;—one through the medium of the suboccipital ganglion, and the other within the foramen lacerum.

It is the Head or Source of the Nerve.—I cannot then bring myself to believe that it springs from the fifth and sixth pairs, or that it is a collection of branches from them, and, by-and-by, from almost every nerve of the frame. A minute dissection of these communications will prove, that there was a nerve previously existing with which the others anastomose, and that it is not one nerve giving origin to the other, but a fair interchange of fibrils and of influence for some important purpose. Both the fifth and the sixth nerves receive as much as they give. I am therefore disposed to regard this ganglion as the commencement of the great organic nerve. I feel a difficulty about it. Ganglions are generally found in the course of nerves, and do not form their head or source. This is a unique instance of a ganglion constituting the origin, although perhaps we have something like it in the origins of the organic motor nerves from that rounded, ganglion-shaped portion of medullary matter the corpus olivare. There needs some master mind to give us a comprehensive and satisfactory view of this portion of the organic system. It would be worthy of that great physiologist to whom we already owe so much, and a noble completion of his labours.

The Course and Ramifications of the Great Organic Nerve.—I will then suppose that this ganglion is the head or commencement of the great organic nerve. There are first its ramifications to the suboccipital ganglion, and thence to all its wide connexions; there is the branch which enters the cranium through the foramen lacerum. As it passes through, a filament is given to the cavity of the tympanum, and so to the seventh pair (the portio dura), and also to the cerebro-visceral; and, still climbing up, I have said that it forms a plexus or ganglion or both; it is a firm body, but enveloped with a meshwork of nervous fibrils, a *rete mirabile*, covering the carotid artery, and where it has the communication which I have described with the sixth, and whence proceed several other little filaments to the fifth. We no sooner begin to trace the ganglion, and the nerve in which it terminates, than we are made aware of that which gave it the name of the great sympathetic; it is connecting itself with everything; we can trace it to the laryngeal branch of the spino-cerebral, the glosso-pharyngeus, the spinal accessory, the first and the second cervical. In fact, we shall find that it communicates with every nerve by which it passes, and exchanges fibres with each—it gives fibres to receive them from both roots, but chiefly the motor root.

This Nerve given especially to the Arteries.—But now appears a new feature in nervous ramifications. We have seen anastomoses between different nerves, or branches of the same nerve; we have seen the nerves ramify upon and lose themselves in the substance of muscles; but the fibriculi of this nerve are principally distributed over the arteries. We see it here beautifully. When they reach the arteries, they form plexuses, net works, and, combining with some interposed cellular substance, perfect nervous tunics around the vessels. The substance of the nerve is pulpy—its neurilema or sheath is delicate—there is a kind of transparency about it, and therefore although these nerves are visible enough on the larger vessels, and can be traced even on the smaller, we at length lose them, not because they cease to exist, but because, from their peculiar structure, we are no longer able to follow them. We have hitherto had nerves spreading over certain membranes, and devoted to peculiar sensation; or ramifying over the locomotive portion of the frame generally, and connected with common sensation; or over muscles, and connected with motion, voluntary or involuntary: but here is a nerve identifying itself indeed with every other, but peculiarly *the nerve of the bloodvessels*, covering them as with a tunic, and connected in some mysterious way with their action. This is a singular circumstance, and to which I shall recall your attention by-and-by.

Connected with all the main Arteries.—It is found at its commencement, thus encircling the internal carotid, and can be traced, for a certain distance, on all its branches. It covers in the same manner the beginning of the external carotid, and can be followed over the main arteries of the neck, and the face, and the glands.

Course down the Neck.—The ganglion grows smaller, and ends in a nervous chord. *The great organic* I have ventured to call it, and the reason for which will presently be more apparent. It now unites itself with the cerebro-visceral, and passes down the neck in the same sheath with it, and lying between the carotid and the cerebro-visceral, and therefore, for any experiment, clearly distinguishable by this peculiarity of situation, by its being the smaller nerve of the two, and of a redder hue, and a more pulpy substance. As it pursues its course there are frequent anastomoses between these nerves, and also very small filaments from the lower cervical nerves. When we reach the base of the neck, it communicates with the recurrent nerve.

It enters the Thorax.—As it approaches the thorax, and when entering into it, its net-work investing connexion with the arteries becomes again more evident; it may be seen surrounding the vertebral artery, and more particularly the axillary artery, on the trunk of which it cannot be mistaken, and on the branches of which it may be long followed. It begins also to be more evident on the carotids, and forms on them numerous plexuses, interspersed with ganglions, and which seem to be the sources of new radiations; and these run along the great vessels to the heart.

The Posterior Cervical Ganglion.—As it enters the thorax, or under the first dorsal vertebra, this nerve enlarges again, but not to the same degree as at the superior ganglion. Here we briefly trace some important branches—one of considerable size to the seventh (sixth) cervical nerve, not, as our best veterinary anatomist asserts, on account of "the mediate connexion which it thus has with the five anterior cervical nerves," because it has had direct communication with them in its passage down the neck; but because a plexus of spinal nerves is here beginning to be formed for the supply of the fore extremities, and this nerve is called upon to contribute its influence whatever it may be There are branches to the last cervical—to the first and second dorsals—a plexus about the root of the trachea—fibrils to the œsophagus—more to the recurrent, and, chief of all, the cardiac plexus, given to, and plainly ramifying over, the great vessels of the heart. I have not time to enumerate the ramifications and plexuses which are here formed; they are so numerous and complicated, that no vessel, of the heart or lungs, or even any portion of a vessel, should be without this nervous tunic.

Division within the Thorax.—All these are ramifications from this nerve; the trunk of which has entered the thorax, and runs along the spine under the articulations of the ribs; but it soon divides on each side. The proper great organic pursues its course under the articulations of the vertebræ; and, between the heads of each rib, it swells out into a small ganglion, from each of which filaments proceed to the intercostal nerve; going principally to the motor branch of it, and receiving filaments from it: and thus it reaches the diaphragm, under the crus of which it passes into the abdominal cavity.

The Splanchnic Nerve.—Varying in different subjects, fibres begin to be given off from the trunk of the great organic nerve about the 5th, 6th, and 7th rib, and these unite and form a considerable branch on each side, which descends some distance from the spine, and pursues its course over the ribs, and under the pleura, to the diaphragm, and bearing the name of the *greater splanchnic nerve.* It also enters the abdomen under the crus of the diaphragm. Another smaller branch is given off from the great organic, while it is running along the crus of the diaphragm, and which, after turning backwards, and describing a kind of arch, also enters the abdomen.

The great Organic in the Abdomen.—The main trunk of this nerve continues in the abdomen close to the transverse processes of the vertebræ, and, gradually creeping up the sides of the vertebræ as we proceed backward along the loins, it pursues its course to the pelvis. Opposite to each lumbar nerve a ganglion is formed, and from it two little branches are given off: one anastomoses with the lumbar nerve on the same side, and the other either creeps under the aorta and unites itself to the corresponding branch on the opposite side, or climbs over the aorta, mingling with the net-work of nervous matter from the organic nerve, and which has been continued along the whole course of the vessel, and then, emerging from this plexus, it joins its fellow on the opposite side of the spine. When we reach the pelvis, the great organic still continues its path along the sacrum, forming the same kind of ganglia, and communicating with the sacral nerve on the same side, and with the branch of the organic on the other side, and that through the medium of dense interposed nervous plexuses. Having passed the sacrum, the nerve rapidly diminishes, it runs under the peritoneum for a little way, and then, reduced to extreme minuteness and thinness, it disappears; and, as some tell us, in a ganglion, formed by the union of the two nerves, which describe a kind of arch to meet each other.

The Splanchnics in the Abdomen.—The splanchnic nerves, having entered the abdomen, bend suddenly downward, and form the principal portion of the semilunar ganglion. A part of the lesser splanchnic is devoted to the same purpose, while another branch of it goes on and enters into the renal plexus. I have already stated, that the cerebro-visceral and the phrenic nerves contribute important branches to the same ganglion.

The Semilunar Ganglion.—Under the posterior aorta, at the root of the cœliac artery, and occupying the space between the cœliac and the anterior mesenteric arteries, the splanchnics, the left branch of the cerebro-visceral, and a branch from the phrenic, meet, and form what has been called the *semilunar ganglion,*

> If shape it might be called, that shape had none
> Distinguishable,—
> Or substance might be called, that shadow seemed,
> For each seemed either.

As these nerves approach each other, they lose their individual form and character; they diffuse themselves into numerous filaments, uniting again to form distinct ganglia; and these matted together and surrounding each other, now blending together, and now separating from each other, then confusedly crossing each other in every direction, so that we have scarcely passed the border of this congeries of nerves, when we cease to be able to follow either of the contributory branches, and have only the mingled substance of all. This ganglion or collection of ganglia, has no determinate shape in any of the domesticated animals; except that you may trace a somewhat convex border posteriorly, and one more concave anteriorly in the direction whence the contributing branches came, and thus forming a rude kind of crescent. From the convex border of this lunar, I would rather call it solar plexus; there are divergent filaments of different consistence and size, and some of them minute, filmy, "shadowy;" these are rays, radiations of nervous influence yet strangely mingling and interweaving with each other, and combining the properties and the powers of the different principles of organic life. Therefore we no longer speak of particular nerves, or branches of nerves, but of plexuses; and we give them a name from the viscus or part to which they are directed. I will not detain you by describing all of them, but I would urge you to follow them in some slow and careful dissection, and observe how every viscus and every portion of the organic system which the abdomen contains, lives and acts by influence derived from this central sun.

The different Plexuses from the Semilunar Ganglion.—The names and the connexions of some of these plexuses, I must briefly run over. *The anterior mesenteric* I find upon the root of the anterior mesenteric artery. As the ganglion was composed principally from the great organic, so it retains the character of that nerve in the mode of its distribution. Whatever may be the ultimate destination or function of some of the fibrils, we find the plexus on an artery, not merely running along it like a motor or a sensitive nerve, but forming a tunic around it, penetrating it at every assignable point, and becoming, as it were, a portion and a part of it. We find this plexus first on the root of the anterior mesenteric artery; and composed, too, not merely of fibres from the semilunar ganglion, but from the lumbar nerves. The plexus pursues the course of the artery—accompanies all its branches—radiates on every side—spreads over the mesentery—encircles the various glands—reaches the small intestines—penetrates with the blood-vessels through their parietes, and ramifies in their internal surface. Other branches of this plexus I can trace to the cæcum and the cæcal portion of the colon.

The Posterior Mesenteric Plexuses.—I have never lost sight of the plexus which thickly covers the large blood-vessels in every portion of their course. The posterior aorta is now more richly supplied, and I trace ramifications from it, over that part of the colon which the anterior plexus had not reached; and to the mesocolon with all its glands; and to the rectum: and here is a reinforcement of nervous power from the branches of the great organic, which I described as still continued along the abdomen, after the splanchnics had formed the semilunar ganglion: and also connected with this plexus are fibres from the lumbar and sacral nerves.

The Renal and Spermatic Plexuses.—Hence I might trace the plexus to the kidneys, reinforced by filaments from the lesser splanchnic; and I might show it to you winding round the origins of the emulgent arteries; and penetrating with them into the very substance of the kidney, and continued even over the ureters; and, continuous with the renal, the *spermatic* plexus, diffused over all the organs of reproduction.

The Hepatic Plexus,—or, to mention only one plexus more, the *hepatic*, seemingly one of great importance. I first find it around the root of the hepatic artery; and trace it about that vessel in its course to the liver, transmitting, however, as it goes along, filaments to the phrenic arteries or those of the diaphragm, and also to the stomach and the pancreas; and then dividing into two collections of complicated branches, the *right* and the *left* hepatic plexuses. I follow the first over the right hepatic artery, and the vena portæ; and the biliary ducts, and the gall-bladder; and some fibres of it branching off and going to the lower orifice of the stomach, and uniting with the cerebro-visceral, and proceeding with it down the duodenum. The *left* plexus I can also follow to the liver, and through the ramifications of all the vessels of that gland; and I trace it on to the cardiac orifice of the stomach, and observe its anastomosis or identification with the cerebro-visceral here; but all this, Gentlemen, and by some abler anatomist, for I am now too old for that portion of my labour, will at some future time be demonstrated to the veterinary class: but in the mean time I would urge you, at your earliest leisure, to trace this out for yourselves. You may not be able to unravel all the intricacies of the plexuses, but the grand thing will be sufficiently plain, that from this central combination of the influence and power of all the organic nerves, every abdominal and pelvic organ and vessel is supplied.

The Inquiry into the Function of the great Organic somewhat unsatisfactory.—And now, Gentlemen, the most fearful part of my subject, the function of this last of the organic nerves. I cannot forget what Magendie has so truly said, that "suppositions, hypotheses, opinions, are all that the works of physiologists have hitherto presented upon a question the most interesting of all physiology." And that "it would be wiser and more conducive to the advancement of science to confess that, at the present, the use of the sympathetic nerve is unknown." It is unknown—it has not been experimentally proved. I am inclined to believe that it never can be. There is such a chain of intercommunication and connexion between the branches of this nerve and of every other,—such a continuous plexus, if I may so term it, that we know not where or how we shall totally destroy its influence on any part. Experiment may go some way, but after all we shall be principally left to deductions from what we know of other nerves, and what they effect, and what they have left perfectly unaccomplished. This, I feel, is unsatisfactory, but at least it will teach us caution in drawing our conclusions, and diffidence in the statement and defence of our opinions. Some opinion however we must form, or we shall be perfectly at sea on many an important point of physiology and practice. That opinion will be confirmed, or corrected, or overthrown by the result of future experiments. It may prove, as Magendie sarcastically observes, "*a jeu d'esprit;*" but we could tell him, that in the conducting of experiments on subjects like these, there are so many circumstances to be taken into consideration, and so many unforeseen and unobserved causes of error, that the conclusion from them, and long course of them too, if we could divest them of their barbarity, is a more veritable *jeu d'esprit* than any opinions that are grounded on a slow and careful advance from that we do know, to that which we wish to ascertain.

The Function of the Great Organic continued.—I have endeavoured, how satisfactorily you must judge, to determine the function of the nerves derived from the lateral column of the spinal chord; and the result has been, that they are *organic* nerves: and, as being uniformly lost on some muscular expansion, they are *motor organic* nerves,—guiding the mechanism and effecting the movements of the great machine, either totally independent of the will, as in the discharge of the natural function of the different organs; or by constraining the voluntary muscles to assist or to repress the action of the involuntary, when our "comfort or accommodation,"—when the ravages of disease, or the preservation of life, require it. Thus the heart beats and the lungs heave, and the peristaltic motion of the stomach and intestines is carried on. But is this the whole of organic life? No; it is only a part, and a very inferior one,—it is the mechanical part. The food would in vain traverse the stomach and be propelled through the intestinal canal, if there were not a power by which it was dissolved and changed, and its valuable parts separated, and prepared to become blood. The arterialisation of that blood is a mere mechanical and chemical affair. It is exposed to the action of the atmosphere, and it undergoes a change for which the simple principles of chemistry will enable us to account: but, thus arterialised, it would permeate the arterial and capillary vessels to no purpose, if there is not a power by which, in the minuter capillaries, it is assimilated into the substance of the frame, or many a strangely varied secretion is performed. This is the power for which we are now seeking. No beautiful adjustment of various parts,—no exquisite consent of motion between them, can accomplish this. No chemistry will here avail; we can trace its agency to a certain extent, but as we proceed we find its most invariable and energetic laws suspended—subverted. There is a principle still wanting. We must not look for it in the brain: if the mere mechanical part was in a manner independent of the will, at least in its most essential part it was so, this most important portion of organic life must be perfectly so: and so it is. Not only are we unconscious of the solution and chylification of the food, and the performance of the various secretions, but let every mental "agent" be "bent up" to the terrible feat of arresting or materially interfering with the organic process, we should be perfectly powerless. Where, then, shall we go?

It is the Nutritive or Secretory Nerve.—There is a nerve, the function of which we have not yet determined—the sympathetic or great organic nerve. On what tissue are its ramifications chiefly expended? It is scarcely found ere it is forming complicated plexuses on every neighbouring bloodvessel; it is so even within the cavernous sinus; it is enclosing the carotids, external and internal, with a kind of nervous tunic. I trace it round the temporal to the face and head, and also on every subdivision of the carotid, until, from the minuteness of the vessel and the pulpiness of the nerve, it eludes my sight. I trace it into the thorax, and its ramifications are given to large bloodvessels there; I can follow it in the branching of every vessel, and in the very substance of the viscus, filaments or plexuses of the organic nerve accompany the tube. I examine its course when it enters the abdominal cavity, and there too it is on the roots of the different arteries that the plexus is first observed, and it accompanies the ramifications of each artery to its ultimate destination. Where it ceases I know not. That it accompanies and envelops the capillaries I dare not affirm; but as I gradually lose it through want of visceral power to pursue it, I think it probable that it does accompany and envelop them; and for want of any better, or any other visible agent—for want of any other explanation of its influence over the vessels to which it seems so exclusively directed, I attribute to its agency the most important functions of organic life, nutrition, secretion, and absorption.

Its Anastomoses.—But I perceive another character about this nerve—its anastomosis with every other nerve in the frame, beginning within the cranium, and terminating on the coccygean vertebræ. A singular kind of anastomosis it is, imparting a portion of itself, and receiving fibres from both the roots of the cerebral and spinal nerves, but most of all from the motor. I cannot conceive of the existence of any secretion, which does not imply sensibility in the secreting vessels, and some power exerted in order that an effect may be produced. The gland, or the vessels of the gland, must be conscious of the presence of the substance to be operated upon, and of the accomplishment of that effect; and the vessels themselves cannot be palsied and motionless while the effect is producing. Therefore I have the few sensitive fibrils, but derived from every nerve, for everywhere these secretions are going on. The power of these fibrils, by their connexion with the organic ones, may be so modified, that I shall have the organic sensibility I want, without that animal feeling which would bring the operation too much under the influence of the mind. I have more motor fibrils, for when I look over the frame, I have enough for these secerning vessels to accomplish. And then, for aught I know, when fibrils from the organic nerve are communicated to every spinal one, it may be to add, as it were, a third set of fibres to the two of which it was already composed; and in addition to sensation and the power of motion, to bestow the faculty of secretion, or to assist the discharge of it, in places out of and far away from the thorax or the abdomen.

The Influence of the Nerves on the Secretions.—Have I any experiments to refer to, in corroboration of this?

There are said to be experiments against this hypothesis. The great organic nerve has been divided in the neck, and no perceptible effect followed. I am not aware of the history of these experiments, or the time that elapsed before the animal died, or the result of the experiment supposed to be ascertained; but I must refer again to the innumerable anastomoses which seem to render it difficult or impossible to cut off the influence of this nerve from any other nerve or organ; and I must also observe that the effects of the suspension of the functions of this nerve would not immediately develop themselves. If the par vagum is divided, the animal comparatively soon dies. Respiration is a vital action, and must not be long disturbed, nor at any time suspended: but any or almost all of the secretions may be for a long time suspended, and there will be no apparent distress or irregularity to mark the evil. The true answer however is, that these anastomeses render it difficult or impossible to cut off the influence of the nerve from any organ.

If however I have no experiments, I have many facts elucidating the power of the nerves over the secretions. My illustrations are very simple ones. The secretion of tears from mental emotion would be my first, if I were speaking of the human being; but the deer will weep, and I have seen the tears trickle down the cheek of the horse and the dog. The saliva running from the mouth of the hungry dog watching impatiently for his food;—the obstinate retention of the milk by the cow, until after her calf is supplied; for when the dairy-maid has wrung the last drop she can extract, the mother has retained more than enough for her offspring:—these things show that the cerebral or spinal nerves, not only are concerned in the act of secretion, but, in some rare cases, and they are rare, they are brought under the direct influence of the mind. Need I speak of the effect of the mind on the motions of all the visceral organs, and more particularly on the nature and quality of the secretions; or of the long train of sympathies which exist between various parts of the organic and animal system? Why should worms in the intestinal canal of the dog produce fits; and staggers in the horse, amaurosis? Why should one morsel of hay and a drop of water invigorate the whole frame of the tired post-horse, or a poison taken into the stomach produce instant death? It is the result of this connected chain of nervous influence. But what has this to do with the great organic nerve? Why, it is the only nerve that has been traced to many of the seats of these sympathies; it is the only nerve that has been traced to many of the glands, and the only one which, attaching itself to the blood-vessels, seems to be pursuing its course, and can be traced so far as our sight will permit, to the main secreting organs, the capillary vessels.

I am detaining you beyond the accustomed time, but I must say one word of the function of this nerve within the abdomen. In the semilunar ganglion it has united itself to a portion of the cerebro-visceral and the phrenic. Let me recall to your recollection one viscus, the stomach, in which the effect of two of them was plainly distinct. The cerebro-visceral was divided—chyle was still produced. The food was partially digested—but for want of the peristaltic motion of the stomach, it could not be brought successively within the influence of the gastric juice. Here are plainly displayed the functions of the two nerves, the great organic penetrating the coats of the stomach, and producing the secretion of the solvent fluid—the cerebro-visceral producing the peristaltic motion: and so it is throughout the intestinal canal: we have the phrenic nerve to assist in the compression and final expulsion of the food by contracting the diaphragm, and diminishing the abdominal cavity, while the abdominal and thoracic muscles are contributing their aid by means of these frequent anastomoses.

And now, Gentlemen, I have done with the nervous system. Of the nature of the nervous agency or fluid, or of the mysterious presiding power, whether it regards the animal or organic life, or both, I will not say a word. You will refer, at some period of serious leisure, to the speculations of physiologists on these abstruse but interesting points. You will find in Dr. Bostock's excellent "Compendium of Physiology," a clear statement and luminous comparison of all that has been said and written. With regard to this great organic nerve, you will have the most laboured and the best account of its functions in a note by Dr. Copland appended to his Translation of Richerand, although the mere experimentalist perhaps will tell you that it is nothing but, "supposition, hypothesis, and opinion—a mere *jeu d'esprit.*" But I must conclude.

I feel, and you must have experienced, how difficult it is in a few lectures, and even assisted by demonstrations, to give a clear as well as comprehensive view of such a subject. If, however, I have succeeded in convincing you, that with regard to the animal system at least, it is not so mysterious as you perhaps had imagined; and if I have afforded the slightest clue to the understanding of a portion of the organic system, I am content. As for my peculiar opinions, I have stated none of them positively. Some of them are adopted, not as being satisfactory, but for want of better; and I trust you and I will always be open to conviction, and zealous in our search after truth.

AN ACT

FOR REGULATING

SCHOOLS OF ANATOMY.

August 1st, 1832.

PREAMBLE.—*Secretary of State to grant Licenses to practise Anatomy.*

WHEREAS a knowledge of the causes and nature of sundry diseases which affect the body, and of the best methods of treating and curing such diseases, and of healing and repairing divers wounds and injuries to which the human frame is liable, cannot be acquired without the aid of anatomical examination; and whereas the legal supply of human bodies for such anatomical examination is insufficient fully to provide the means of such knowledge: and whereas, in order further to supply human bodies for such purposes, divers great and grievous crimes have been committed, and lately murder, for the single object of selling for such purposes the bodies of the persons so murdered: and whereas therefore it is highly expedient to give protection, under certain regulations, to the study and practice of anatomy, and to prevent, as far as may be, such great and grievous crimes and murder as aforesaid; be it therefore enacted by the King's most excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, That it shall be lawful for his Majesty's principal Secretary of State for the time being for the Home Department in that part of the United Kingdom called Great Britain, and for the chief Secretary for Ireland in that part of the United Kingdom called Ireland, immediately on the passing of this Act, or so soon thereafter as may be required, to grant a license to practise anatomy to any fellow or member of any college of physicians or surgeons, or to any graduate or licentiate in medicine, or to any person lawfully qualified to practise medicine in any part of the United Kingdom, or to any professor or teacher of anatomy, medicine, or surgery, or to any student attending any school of anatomy, on application from such party for such purpose, countersigned by two of his Majesty's justices of the peace acting for the county, city, borough, or place, wherein such party resides, certifying that, to their knowledge or belief, such party so applying is about to carry on the practice of anatomy.

Secretary of State to appoint Inspectors of Schools of Anatomy.

II. And be it enacted, That it shall be lawful for his Majesty's said principal Secretary of State or chief secretary, as the case may be, immediately on the passing of this Act, or as soon thereafter as may be necessary, to appoint respectively not fewer than three persons to be inspectors of places where anatomy is carried on, and at any time after such first appointment to appoint, if they shall see fit, one or more other person or persons to be an inspector or inspectors as aforesaid; and every such inspector shall continue in office for one year, or until he be removed by the said Secretary of State or chief secretary, as the case may be, or until some other person shall be appointed in his place; and as often as any inspector appointed as aforesaid shall die, or shall be removed from his said office, or shall refuse or become unable to act, it shall be lawful for the said Secretary of State or chief Secretary, as the case may be, to appoint another person to be inspector in his room.

And to direct what District each Inspector shall superintend.

III. And be it enacted, That it shall be lawful for the said Secretary of State or chief secretary, as the case may be, to direct what district of town or country, or of both, and what places where anatomy is carried on, situate within such district, every such inspector shall be appointed to superintend, and in what manner every such inspector shall transact the duties of his office.

Inspectors to make Returns of Subjects removed for Anatomical Examination.

IV. And be it enacted, That every inspector to be appointed by virtue of this Act shall make a quarterly return to the said Secretary of State or chief secretary, as the case may be, of every deceased person's body that during the preceding quarter has been removed for anatomical examination to every separate place in his district where anatomy is carried on, distinguishing the sex, and, as far as is known at the time, the name and age of each person whose body was so removed as aforesaid.

And to inspect Places where Anatomy is practised.

V. And be it enacted, That it shall be lawful for every such inspector to visit and inspect, at any time, any place within his district, notice of which place has been given, as is hereinafter directed, that it is intended there to practise anatomy.

Salaries to Inspectors.

VI. And be it enacted, That it shall be lawful for his Majesty to grant to every such inspector such an annual salary, not exceeding one hundred pounds, for his trouble, and to allow such a sum of money for the expenses of his office, as may appear reasonable; such salaries and allowances to be charged on the consolidated fund of the United Kingdom, and to be payable quarterly; and that an annual return of all such salaries and allowances shall be made to Parliament.

Persons having lawful Custody of Bodies may permit them to undergo Anatomical Examination in certain Cases.

VII. And be it enacted, That it shall be lawful for any executor or other party having lawful possession of the body of any deceased person, and not being an undertaker or other party intrusted with the body for the purpose only of interment, to permit the body of such deceased person to undergo anatomical examination, unless, to the knowledge of such executor or other party, such person shall have expressed his desire, either in writing at any time during his life, or verbally in the presence of two or more witnesses during the illness whereof he died, that his body after death might not undergo such examination, or unless the surviving husband or wife, or any known relative of the deceased person, shall require the body to be interred without such examination.

Provision in case of Persons directing Anatomical Examinations after their Death.

VIII. And be it enacted, That if any person, either in writing at any time during his life, or verbally in the presence of two or more witnesses during the illness whereof he died, shall direct that his body after death be examined anatomically, or shall nominate any party by this Act authorised to examine bodies anatomically to make such examination, and if, before the burial of the body of such person, such direction or nomination shall be made known to the party having lawful possession of the dead body, then such last-mentioned party shall direct such examination to be made, and, in case of any such nomination as aforesaid, shall request and permit any party so authorised and nominated as aforesaid to make such examination, unless the deceased person's surviving husband or wife, or nearest known relative, or any one or more of such person's nearest known relatives, being of kin in the same degree, shall require the body to be interred without such examination.

The Body not to be removed from the Place where such Person may have died without a Certificate.

IX. Provided always, and be it enacted, That in no case shall the body of any person be removed for anatomical examination from any place where such person may have died, until after forty-eight hours from the time of such person's decease, nor until after twenty-four hours notice, to be reckoned from the time of such decease, to the inspector of the district, of the intended removal of the body, or, if no such inspector have been appointed, to some physician, surgeon, or apothecary, residing at or near the place of death, nor unless a certificate stating in what manner such person came by his death shall previously to the removal of the body have been signed by the physician, surgeon, or apothecary, who attended such person during the illness whereof he died, or if no such medical man attended such person during such illness, then by some physician, surgeon, or apothecary, who shall be called in after the death of such person to view his body, and who shall state the manner or cause of death according to the best of his knowledge and belief, but who shall not be concerned in examining the body after removal; and that in case of such removal, such certificate shall be delivered, together with the body, to the party receiving the same for anatomical examination.

Professors, Surgeons, and others, may receive Bodies for Anatomical Examination.

X. And be it enacted, That it shall be lawful for any member or fellow of any college of physicians or surgeons, or any graduate or licentiate in medicine, or any person lawfully qualified to practise medicine in any part of the United Kingdom, or any professor, teacher, or student of anatomy, medicine, or surgery, having a license from his Majesty's principal Secretary of State or chief secretary as aforesaid, to receive or possess for anatomical examination, or to examine anatomically, the body of any person deceased, if permitted or directed so to do by a party who had at the time of giving such permission or direction lawful possession of the body, and who had power, in pursuance of the provisions of this Act, to permit or cause the body to be so examined, and provided such certificate as aforesaid were delivered by such party together with the body.

Such Persons to receive with the Body a Certificate as aforesaid, which shall be transmitted to the Inspector.

XI. And be it enacted, That every party so receiving a body for anatomical examination after removal shall demand and receive, together with the body, a certificate as aforesaid, and shall, within twenty-four hours next after such removal, transmit to the inspector of the district such certificate, and also a return stating at what day and hour, and from whom the body was received, the date and place of death, the sex, and (as far as is known at the time) the Christian and surname, age, and last place of abode of such person, or, if no such inspector have been appointed, to some physician, surgeon, or apothecary, residing at or near the place to which the body is removed, and shall enter, or cause to be entered, the aforesaid particulars relating thereto, and a copy of the certificate he received therewith, in a book to be kept by him for that purpose, and shall produce such book whenever required so to do by any inspector so appointed as aforesaid.

Notice to be given to Secretary of State of Places where Anatomy is about to be practised.

XII. And be it enacted, That it shall not be lawful for any party to carry on or teach anatomy at any place, or at any place to receive or possess for anatomical examination, or examine anatomically, any deceased person's body after removal of the same, unless such party, or the owner or occupier of such place, or some party by this Act authorised to examine bodies anatomically, shall, at least one week before the first receipt or possession of a body for such purpose at such place, have given notice to the said Secretary of State or chief secretary, as the case may be, of the place where it is intended to practise anatomy.

How Bodies are to be removed for Examination.—Provision for Interment.

XIII. Provided always, and be it enacted, That every such body so removed as aforesaid for the purpose of examination shall, before such removal, be placed in a decent coffin or shell, and be removed therein; and that the party removing the same, or causing the same to be removed as aforesaid, shall make provision that such body, after undergoing anatomical examination, be decently interred in consecrated ground, or in some public burial-ground in use for persons of that religious persuasion to which the person whose body was so removed belonged; and that a certificate of the interment of such body shall be transmitted to the inspector of the district within six weeks after the day on which such body was received as aforesaid.

Persons described in this Act not to be liable to Punishment for having in their possession Human Bodies.

XIV. And be it enacted, That no member or fellow of any college of physicians or surgeons, nor any graduate or licentiate in medicine, nor any person lawfully qualified to practise medicine in any part of the United Kingdom, nor any professor, teacher, or student of anatomy, medicine, or surgery, having a license from his Majesty's principal Secretary of State or chief secretary as aforesaid, shall be liable to any prosecution, penalty, forfeiture, or punishment, for receiving or having in his possession for anatomical examination, or for examining anatomically any dead human body, according to the provisions of this Act.

Act not to prohibit Post-Mortem Examination.

XV. And be it enacted, That nothing in this Act contained shall be construed to extend to, or to prohibit any post-mortem examination of any human body required or directed to be made by any competent legal authority.

So much of 9 Geo. 4. c. 31, as directs that the Bodies of Murderers may be dissected, repealed. Such Bodies to be hung in chains or buried, as the Court shall direct.

XVI. And whereas an Act was passed in the ninth year of the reign of his late Majesty, for consolidating and amending the statutes in England relative to offences against the person, by which latter Act it is enacted, that the body of every person convicted of murder shall, after execution, either be dissected or hung in chains, as to the court which tried the offender shall seem meet; and that the sentence to be pronounced by the court shall express that the body of the offender shall be dissected or hung in chains, whichsoever of the two the court shall order; be it enacted, That so much of the said last-recited Act as authorises the court, if it shall see fit, to direct that the body of a person convicted of murder shall, after execution, be dissected, be and the same is hereby repealed; and that in every case of conviction of any prisoner for murder, the court before which such prisoner shall have been tried shall direct such prisoner either to be hung in chains, or to be buried within the precincts of the prison in which such prisoner shall have been confined after conviction, as to such court shall seem meet; and that the sentence to be pronounced by the court shall express that the body of such prisoner shall be hung in chains, or buried within the precincts of the prison, whichever of the two the court shall order.

Limitation of Actions.

XVII. And be it enacted, That if any action or suit shall be commenced or brought against any person for anything done in pursuance of this Act, the same shall be commenced within six calendar months next after the cause of action accrued; and the defendant in every such action or suit may, at his election, plead the matter specially, or the general issue not guilty, and give this act and the special matter in evidence at any trial to be had thereupon.

Offences against this Act.

XVIII. And be it enacted, that any person offending against the provisions of this Act in England or Ireland shall be deemed and taken to be guilty of a misdemeanor, and being duly convicted thereof, shall be punished by imprisonment for a term not exceeding three months, or by a fine not exceeding fifty pounds, at the discretion of the court before which he shall be tried; and any person offending against the provisions of this act in Scotland shall, upon being duly convicted of such offence, be punished by imprisonment for a term not exceeding three months, or by a fine not exceeding fifty pounds, at the discretion of the court before which he shall be tried.

Interpretation of certain Words in this Act.

XIX. And in order to remove doubts as to the meaning of certain words in this Act, be it enacted, that the words "person and party" shall be respectively deemed to include any number of persons, or any society, whether by charter or otherwise; and that the meaning of the aforesaid words shall not be restricted although the same may be subsequently referred to in the singular number and masculine gender only.

Commencement of Act.

XX. And be it enacted, that this Act shall commence and take effect from and after the first day of August in the present year.

Act may be amended.

XXI. And be it enacted, that this act may be altered or amended during the present session of Parliament.

ANATOMISTS AND INSPECTORS.

CIRCULARS DRAWN UP BY DR. SOMERVILLE,

Inspector of Anatomy, London.

Sir,—I am desirous, on being appointed inspector pursuant to the Act for regulating the Schools of Anatomy, to solicit your cooperation in carrying into execution the enactments of the legislature.

In order to facilitate this object, and to secure uniformity and regularity of proceeding, I beg leave to submit to you a general form of return as required by the statute, together with an abstract of the duties which appear to me to be imposed by it upon teachers of anatomy. It is unnecessary for me to point out how important it is for the satisfaction of the public mind, that the natural feelings of human nature should be consulted by a strict attention to the 13th section of the Act, which provides for the decent interment of bodies after they have undergone anatomical examination.

I have only to add, that I shall be anxious to receive such advice and information as your knowledge and experience may suggest, and to assure you that any such communications, as well as the returns, shall be considered by me strictly secret and confidential, and shall only be reported to the public authorities as directed and required by the Act of Parliament. I am, Sir,

Your obedient servant,

J. C. Somerville.

Office of Inspector of Anatomy, 5, Saville
Row, 30th Aug. 1832.

FORM OF APPLICATION FOR A LICENSE TO PRACTISE ANATOMY.

The party applying for a license to practise anatomy must make a written application to the Secretary of State, signed by him with his christian and surname at length, in which application he must state he applies for a license to practise anatomy, in pursuance of the Act passed for regulating schools of anatomy. He must state also the place of his residence, and the place where he is about to carry on the practice of anatomy, and must further state that he is, according as the case may be [a fellow or member of a College of Physicians or Surgeons], or [a graduate or licentiate in medicine], or [a person lawfully qualified to practise medicine in England, Scotland, or Ireland], or [a professor or teacher of anatomy, medicine, or surgery], or [a student attending a school of anatomy.]

This application is to be countersigned by two justices of the peace acting for the county, city, or borough wherein the party applying resides, and so describing themselves, certifying the place of residence of the party, and also the place where they believe him to be about to carry on the practice of anatomy.

DUTIES OF TEACHERS OF ANATOMY.

To obtain a license according to the provisions of the Act 2d and 3d Will. IV. c. 75.

Upon receiving a body for anatomical examination, to demand and receive with the body a certificate from the physician, surgeon, or apothecary, who attended the person during the illness of which he died, or from some surgeon, physician, or apothecary, who shall have been called in after the death of such person to view the body, stating the manner or cause of the death, and to transmit within twenty-four hours next after the removal of the body, such certificate to the inspector of the district, and also a return, stating at what day and hour, and for whom the body was received, the date and place of death, the sex, and, as far as is known at the time, the christian and surname, age, and last place of abode of such person; or if no such inspector shall have been appointed, to some physician, surgeon, or apothecary, residing at or near the place to which the body is removed; and to enter, or cause to be entered, the aforesaid particulars, and a copy of the certificate, in a book to be kept by him for that purpose, and to produce such book whenever required by the inspector.

To give notice to the Secretary of State of every place where it is intended to practise anatomy at least one week before the first receipt or possession of a dead body for such purpose at such place.

To take care that every body removed for examination shall be removed in a decent and fitting manner, and to make provision that such body, after undergoing anatomical examination, be decently interred according to the thirteenth clause of the said Act; and that a certificate of the interment of such body be transmitted to the inspector of the district within six weeks after the day of the reception of such body.

Confidential Return from the School of Anatomy at ——————————

as required by the 2d and 3d Will. IV. c. 75.

Christian Name and Surname of Deceased.	Age and Sex.	Last Place of abode of deceased.	Date and Place of Death.	Time of Removal.	Day and Hour at which the Body was received, and from whom.	Observations.

(Signed) ———————————— *Teacher.*

DUTIES OF THE INSPECTOR.

He shall keep an account of all the schools of anatomy licensed by the Secretary of State within the district for which he shall be appointed, with the names and residences of the teachers.

He shall make a quarterly return to the Secretary of State of the body of every deceased person, that during the preceding quarter has been removed for anatomical examination to every separate place in his district where anatomy is carried on; distinguishing the sex, and, as far as is known at the time, the name and age of each person whose body has been so removed as aforesaid.

He shall visit and inspect from time to time every place within his district, of which place notice has been given that it is intended there to practise anatomy; he shall take care that the provisions of the Act of Parliament are enforced, and shall report to the Secretary of State any irregularities or offences against the Act which he shall observe.

He shall enter, in a register kept for the purpose, the returns from each school separately, and shall keep copies of all his returns and reports to the Secretary of State, and shall enter all the correspondence relating to the duties of his office in a letter book.

CASE OF

CARCINOMA OF THE UTERUS.

By A. A. Harvey, *M. D., Bath.*

Being called in to attend Mrs. Cook of this city, ætat. 46, I found her labouring under this almost incurable disease in its last stage. The discharge from the uterus continual, excessive, and very fetid. She was confined to her bed from debility, and very much reduced in flesh; and her case hopeless, which can be affirmed by Dr. Shuttlebury and Mr. Mansford, surgeon, of this place. Having been successful in one case before, I undertook to attend, and her cure was perfected in the space of ten weeks, by the following remedies. She has continued free from the disease ever since (*five years*), and is in good health.

℞ *Decocti cort. Peruv.* ℥vii;
Tincturæ ejusdem comp.;
Syrupi papav. albi; *aa* ℥ss;
Liquor arsenicalis (Fowler's) guttas xx
M fiat mistura; *cap. cochl. amp.* ii *ter in die.*

℞ *Opii purif.* gr. ij; *ft. pilula omni nocte hora somni sumenda*;
Ol. ricini ℥ss *pro re nata.*

℞ *Infusi laur. cerasi* lbss;
Decocti querci, font lbiss;
Tinct. myrrh ℥i;
Tinct. opii ℥ss;
Argenti nitrati ʒi. *M fiat injectio bis in die utenda.*

Warm milk and laudanum thrown up the rectum *daily.*

The above remedies were occasionally altered, as symptoms required.

I should not have mentioned this case had it not been for one of the remedies lately stated to have been used by a Mr. Jewel in vaginal discharges, but whether it was the caustic laurel or arsenic which effected the cure in Mrs. Cook's case I know not. I should rather think the laurel, as I have used a tincture of the latter with the happiest success in tubercular consumption with considerable increased vascular action, and it has brought down the pulse from 100 or 120 in the minute to 80 and under, in a few days, with the assistance of one or two bleedings, and a few saline purgatives, after which I have given tonics, and invariably cured the disease.

Bath, August 27, 1832.

TREATMENT OF CHOLERA

ON BOARD THE

CIRCASSIAN, CONVICT SHIP.*

SIR,—I beg leave to transmit the cases of Martin Kelly and Ann M'Namara, persons embarked on board this ship, which although not so severe as some commonly met with, still, I am of opinion, had they not been early under treatment, might ultimately have proved so.

My treatment of diarrhœa is simply this. I bleed them to not above sixteen ounces, on the very first application to me, which has generally been in the early stage of the disease, owing to the very strict orders which I have given to that effect. I then immediately commence giving three grains of calomel, and half a grain of opium every four hours, until the gums begin to be sensibly affected, and the dejections, which at first are of a very light colour, and in most instances watery, have assumed a healthy appearance.

I then give them a little rhubarb and magnesia in peppermint water; and of the twenty-nine cases which I have had (exclusive of the four of cholera) four have this day been discharged to duty, and the others are all in a most favourable state; indeed a great part of them are only continued in the Return to owing the soreness of their mouths, and the danger of relapse from imprudences in their diet.

The ship is tolerably well ventilated, and the prisons, hospitals, and barracks, are daily fumigated and sprinkled with chloride of lime, and the people kept in the greatest possible state of cleanliness.

I am, Sir,
Your much obliged and very
humble servant,
WM. PORTEOUS.

Circassian Convict-Ship,
Standgate Creek, Aug. 29, 1832.
To W. Maclean, Esq.

Report of two Cases of Cholera on board of the Circassian Convict Ship off Woolwich.

CASE I.

Ann M'Namara (soldier's wife), aged 18, was attacked at eleven p. m., on the 18th of August, with vomiting, griping, and purging, followed towards the morning by cramps in the abdominal muscles, and excessive thirst.

She says that she vomited four times, but the purging was almost incessant until eight o'clock this morning (19th), and she does not know the nature of the dejections farther than that they were watery, but the matter ejected from the stomach tasted bitter. At eight o'clock she complained to me, and was then suffering severely from cramp in the abdominal muscles; countenance anxious but not shrunk; pulse 76, of good strength. The vomiting had ceased, but she had been purged about a quarter of an hour previously.

I gave her immediately twenty-five drops of laudanum in half a wineglassful of brandy, and bottles of hot water were applied to the abdomen, after which she had no more purging or vomiting. The cramp left her in a short time, when she fell asleep, and continued so for about an hour.

At one p. m., she had a copious loose stool, resembling very thin rice-water, without any griping. She had great thirst and vertigo, and had occasional slight twitches of cramp in the abdominal muscles during the day, for which she took six or seven drops of laudanum in a very small quantity of brandy, three or four times, and the hot water was continued.

At ten a. m., she began taking four grains of calomel and half a grain of opium every four hours, and warm gruel was given for drink, which treatment was continued during the night.

Next morning she was much better; had slept a little; the pain had entirely left her; had had three or four loose dejections of the same nature as the one above mentioned; and about two o'clock she voided urine for the first time for twenty-four hours. Pulse 80.

The calomel and opium were continued throughout the day, and she had two or three loose dejections, each succeeding one of more natural appearance; and on the morning of the 21st they were perfectly healthy, and she rapidly recovered without any other particular symptom.

CASE II.

Martin Kelly (soldier), aged 28, complained to me on the morning of the 23d of August, of having been purged two or three times during the night without any griping. His dejections were described as being light coloured. He took three grains of calomel and half a grain of opium every four hours during the day and throughout the night.

At noon on the 24th, while sitting on the poop, he was suddenly seized with vertigo and a sense of faintness, of which I was almost immediately informed; and when seen, he complained of excessive weakness and great nausea, but he had not vomited nor had he been purged since the morning, and then only once, and once during the night. His countenance was pale and almost ghastly; pulse weak, irregular, and about 80, and his skin was covered with a profuse cold perspiration.

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* Communicated by the Central Board of Health.

I had him immediately conveyed to the hospital, and put to bed; bled him to ℥xvi; gave him twenty-five drops of laudanum in two ounces of brandy, and applied warmth to his extremities.

He expressed himself immediately relieved by the bleeding; the pulse became regular and full; the cold perspiration was succeeded by a fine warm moisture, and he had no farther unpleasant symptom.

The calomel and opium were continued every four hours, until the morning of the 26th, when his gums were slightly affected, and the dejections, which at first were loose and very light coloured, became quite natural. He then took a dose of rhubarb and magnesia, which operated, and without having any other particular symptom, he was this day discharged to duty.

W. Porteous,
Surgeon Superintendent.

Standgate Creek, Aug. 29, 1832.

COLD WATER

IN THE COLLAPSE STAGE OF

MALIGNANT CHOLERA.

To the Members of the Central Board of Health, *London.*

Gentlemen,—The perusal of Dr. Hardwicke Shute's communications induces me to transmit an account of a most severe case successfully treated some weeks back almost on the same principles.

On the 6th August, at seven p.m., I visited a young man, ætat. 20, who was in the confirmed collapse stage of cholera, attended with the following symptoms:—General coldness and purplish colour of the skin, from every pore of which issued copious cold perspiration, or rather cold water; breath and tongue cold, the latter feeling like a frog; no pulse at the wrist; cramps in almost every muscle, and the vomiting and purging of transparent fluid excessive; urine suppressed; the countenance, if at first in any degree shrunken, became in less than an hour completely collapsed, and the voice decidedly a choleric whisper. On the 7th the pulse had not returned, yet the diarrhœa was considerably checked, but an immoderate flow of urine for a short time was observed. The skin was dry, though still purplish and cold; cramps not so urgent; tongue no longer cold; slightly crusted; vomiting quite as frequent and copious. On the 8th there was some improvement, some warmth of skin, but no urine. On the 9th, a slight tinge of bile in the evacuations; no urine; pulse returned, but beating slowly. On the 10th, motions decidedly bilious, and the last feculent. Skin warm and pulse better; no urine till the 11th, from which time he gradually improved; was out on the 16th, and resumed his labour on the 24th, having had scarcely any consecutive fever.

The treatment was as follows:—A mustard plaster was applied to the spine, from the occiput to the sacrum, and followed by a blister spread on adhesive plaster. An enema of starch and laudanum was immediately administered, repeated in three hours, and then every twelve hours for four days; but after every loose motion, three-fourths of a pint of starch alone were thrown up the rectum. No medicine of any kind, but cold water as much and as frequently as the patient desired.

A medical friend, who saw this case with me on the 11th, afterwards treated a pulseless old man, ætat. 70, on the same plan, except giving 40 drops of laudanum in chalk mixture, and speedy recovery ensued; reaction being so decided as to allow of bleeding.

I gave cold water *ad libitum* to the first patient I attended with cholera, and withdrew all stimulants, and succeeded; but many other remedies were also tried, and it was not till I lost two cases, and witnessed many deaths under other plans, that in the cold stage I adopted the simple method above detailed. Experience alone can decide, whether the counter-irritation be or be not essential in addition to the cold water. I am, Gentlemen,

Your obedient servant,
Joseph B. Whiting.

King's Lynn, Sept. 2nd, 1832.

In premonitory cases, the astringent and opiate plan recommended to the medical practitioners of this town by my brother Dr. Whiting, has been marked by the most decided success in arresting the progress of the disease, of which there have certainly been *three*, if not five importations.

TREATMENT OF

MALIGNANT CHOLERA.

BY

CALOMEL, OPIUM, BRANDY, AND HYDROCYANIC ACID.

By W. P. Foster, *Esq., M.R.C.S., Hackney.*

Having, at the request of the governors of the poor of the parish of Bermondsey, attended at the establishment at Merton in Surrey for the reception of the aged poor and the parish children, amongst whom the cholera has prevailed to a great extent, and having met with the most gratifying results from the treatment I adopted, I think it right to make it generally known.

With the exception of nine cases, in which there was no discoloration of the skin, and of four, in which the fluid vomited was of the same character as the dejections, the symptoms were analogous in all the cases which came under my notice. I need, therefore, only state the particulars of one of each description, with the treatment adopted.

CASE I.

John Hayes, ætat, 11, was attacked July 14th at half past nine a.m., with violent vomiting and purging. I saw him within a few minutes after the attack; he complained of violent pain in the bowels and head; vomiting and purging almost incessant; great prostration of strength; eye glassy and much sunk; countenance depicted extreme anxiety; the whole surface cold; excessive thirst; tongue loaded with a brown fur in the middle, but white towards the edge, perfectly cold; the pulse in the radial artery imperceptible, and action of the heart nearly so; skin surrounding eyes and lips quite blue, which shortly extended over the whole body; had passed no urine since preceding evening. I had him undressed and wrapped in a warm blanket, and covered with twelve or fourteen others; administered ℥iij brandy undiluted; followed immediately by hyd. submur. gr. x; pulv. opii gr. i; vomiting ceased for about five minutes; but returning, gave ℥i brandy, and repeated same every five minutes.

Half past ten a.m. (about an hour after I first saw him); pulsation at wrist was quite perceptible, perspiration copious, and he passed some urine; vomiting and purging still continued, but not so frequent; alvine secretions as clear as water, with small particles floating, resembling grains of boiled rice, and about same size. Eleven a.m. Blueness gradually subsiding; has only vomited once last half hour; brandy repeated.—Twelve o'clock. Pains in abdomen much relieved; dejections not so watery and less frequent; pulse 120; quite distinct, but feeble; tongue much cleaner; vomiting ceased.—Two p.m. Vomiting returned; repeat brandy.—Four p.m. Has not vomited since the last brandy; motions appear slightly turgid with bile; perspiration continues copious; has passed great quantity of urine.—Seven p.m. Motions very offensive; much more natural, and bile very apparent; feels inclined to sleep.—Ten p.m. Sleeping roundly; pulse 96, and fuller; vomiting not returned; skin moist, and of natural colour.

July 5. Two a.m. Slept since I last saw him; p. 96; skin moist.—Six a.m. Passed two motions, last almost natural, large proportion of bile; no pain; complains only of weakness; tongue clean; pulse 96. Ordered strong beef-tea with small proportion of bread every two hours; glass of port wine twice a day.

6. Is quite convalescent; mutton chop for dinner; continue port wine a few days.

CASE II.

Elizabeth Jones, ætat. 14, attacked 6th July at half past ten a.m. Pain in head and bowels; constant vomiting and purging; eyes glassy and much sunk; countenance anxious; whole surface cold; no pain at wrist; action of heart very feeble; does not remember when last passed urine, but not for some hours previous to the attack; tongue furred, brown in middle, white towards edges; quite cold; prostration of strength so great as scarcely to be able to turn in bed. Gave ℥iij brandy, immediately followed by hyd. submur. gr. iij; pulv. opii gr. i, in form of powder, repeating brandy in ℥i doses every five minutes, till copious perspiration appeared and pulse became distinct.—Twelve o'clock. Vomiting continues; repeat brandy; dejections of consistence of thin gruel.—One p.m. Motions more watery, and continue as frequent as at commencement; vomiting continues; pulse 130; very feeble; tongue cleaner; continue brandy.—Three p.m. Vomiting continues; no pain; motions quite pellucid, with similar appearance as noted in former case, and fluid vomited precisely the same. Sumt. acid hydrocyanic gtt. i, in aquâ; repeat brandy four p.m. Has not vomited since the prussic acid; dejections not so frequent or so watery; pulse 98, and feeble; countenance much less anxious, tongue much cleaner.—Six p.m. Has just passed a motion evidently tinged with bile; no return of vomiting; pain quite subsided; feels drowsy; tongue clean; pulse 94 and fuller.—Ten p.m. Is much refreshed by sleep; no pain; complains only of weakness. Ordered beef-tea and bread, as in former case.

July 7. Quite convalescent; this patient never became blue.

I think these cases sufficiently show the treatment pursued. I had altogether forty-seven cases of decided Asiatic cholera; the symptoms in all were the same, excepting the vomiting in some being more violent than in others, and the difference noted in the early part of this letter; out of these I lost but one, under the following circumstances.

CASE III.

Joseph Goodacre, ætat. 24, who had suffered from paralysis of lower extremities since birth, and of a weakly constitution, was seized about half-past two a. m., July 7th. A short time previously, I had seen him in bed apparently in usual health. I was not sent for till half-past six, owing to his concealing the symptoms of the case from the watchman who went round every half hour. The whole of his face and body was then of a dark slate colour, or rather approaching to purple; surface cold, no pulse at wrist, and action of heart quite imperceptible; tongue intensely cold; and almost black; dejections resembling water with same appearance as in former cases, and the fluid vomited so similar, that it would have been difficult, if not impossible, to distinguish one from the other. Gave half a pint of brandy undiluted, followed by hyd. submur. gr. x, pulv. opii gr. i, repeating brandy in ℥ii doses every three minutes, but without any effect. Mustard cataplasms were applied over the whole of the chest, abdomen, and legs, and hot bottles to feet; cramps in bowels and extremities were excruciating. Vomiting was allayed by ii gtt. prussic acid in half a wineglass of cold water. He died without any alteration of symptoms at half-past one p. m., being ill about eleven hours. The action of the heart was never perceptible from the time I first saw him. He stated having passed a usual quantity of urine about half an hour previous to the attack.

Previously to adopting the mode of treatment described, I had *seen* two cases of cholera, which were treated with saline medicines and small doses of calomel and opium with camphor, accompanied by hot brandy-and-water in small quantities, and injections of gruel, with half an ounce of tinct. of opium in each, but both proved fatal. As early as possible I had each patient wrapped in a blanket, and covered with 12 or 14 others at once; and bathing the palms of the hands with vinegar, I found most successful in allaying the extreme thirst so generally complained of. The brandy appears to me extremely useful in restoring the circulation, and inducing copious perspiration; and I feel satisfied, that if medical aid be sought at the commencement of the disease, and the treatment followed which I have found attended with such success, the fatality of the present alarming epidemic will be materially diminished. It is worth noting, that though most of the inmates (about 170 in number) occupy double beds; there was no one instance in which both individuals were attacked. This does not bespeak contagion, at least in the early stages of the disease.

Church-street, Hackney,
29th August, 1832.

TREATMENT OF

MALIGNANT CHOLERA.

BY CALOMEL.

By J. Stanley, M.D., *Whitehaven.*

Sir,—Previous to actually seeing cases of spasmodic cholera, medical men are, in my opinion, in almost total ignorance of the real appearance and character of this dreadful disease,—for any description, however accurate, falls far short of the reality; they are also in the same predicament respecting the treatment, so many and various are the modes pointed out by different practitioners. Calomel, or some preparation of mercury, combined with opium, constitutes some part of the treatment in the majority of instances, but not totally so, as in those of Dr. Ayre and one or two others. Prior to the disease visiting this town, I had made up my mind to give the mercurial plan a trial on the first favourable opportunity. Aware as you must be of the difficulty of enforcing strict attention to the exhibition of medicines among the lower classes, especially when they are to be given in small and frequently-repeated doses, I was obliged to wait till my period of attendance on the cholera hospital afforded me that opportunity.

I selected three cases, the one a collier, aged 36, admitted in the stage of collapse. Pulse gone; skin cold and blue; the voice extremely feeble; strong characteristic expression of countenance, with vomiting and purging. The second a sailor, aged 17; collapse commencing; diminished pulse; coldness of surface; violent and frequent vomiting and purging; cramps of the muscles; with premonitory symptoms for three or four days. The third, a collier, aged 28; collapse commencing; violent spasms; vomiting and purging; feeble pulse; with cold and livid extremities.

In describing the treatment, which was

similar in all, I shall take the first case. Previous to my seeing the patients, the nurses had applied mustard cataplasms over the stomach and bowels, with friction to the limbs, and a draught of hot tea, with a spoonful of brandy, which was quickly ejected. I immediately ordered 15 grs. of calomel, with half a gr. of opium, to be formed into a pill, given directly and repeated, every quarter of an hour till four doses were taken, after which a pill, containing 1½ gr. of calomel was directed to be given every quarter of an hour, with a tablespoonful of a mixture containing about two drops of laudanum, and the same of the sp. am. arom. after each pill. This plan was continued for 24 hours without intermission, at the end of which period, by gradual steps the purging ceased, the pulse returned, the livid colour disappeared, a general warmth and universal perspiration were produced, the voice grew stronger, and the improvement was so decided, that I ordered one pill only to be taken every hour, after twelve hours this was discontinued, the patient having no material ailment except debility; it was now necessary to give an aperient, the bowels being in a constipated state. He was discharged in four days well; no salivation ensued; the only troublesome symptom during the cure was excessive thirst; this made him incessantly call for drink, of which, if the nurses inadvertently gave too much, vomiting was excited; I found very small effervescing draughts the most efficient in removing it.

In this case then 219 grains of mercury were taken without any appearance of ptyalism being produced, or any unpleasant symptom,

The other two cases were treated exactly similar and with the same results, but being somewhat milder in their symptoms, a less quantity of mercury was necessary. The first case was considered by those who saw it as a hopeless one.

I am perfectly aware that it may be said the favourable results of three cases are as nothing in deciding a point of such magnitude; I admit it, but I conceive the above cases tend strongly to support the plan of Dr. Ayre and others, and strengthen the opinion of those who consider the mercurial method of treatment in this dreadful scourge and pestilence the best and most eligible.

August 29, 1832.

COLD WATER IN CHOLERA.

To the Editor of THE LANCET.

SIR,—I have been much pleased by the perusal of Dr. Hardwicke Shute's paper, detailing the important results of his treatment of the collapsed stage of malignant cholera, by the unrestrained use of cold water.

In the treatment of the patients in the cholera hospital of Warsaw, in the summer and autumn of last year, I had frequent opportunities of witnessing the distressing sufferings occasioned by a restricted use of liquids of any kind—a small cup (about two tablespoonfuls) of *salop* being all that was allowed, and that only at intervals of half an hour or an hour. The consequence was, that the thirst became almost insupportable, and the hospital continually resounded with the cry of "water"—sounds more heart-rending than even the sight of the most violent symptoms of the disease. The alleged reason of this restriction, on the part of the medical officer, being, the supposed injurious effects of distending the stomach, on the operation of the remedial agents administered, which were chiefly large doses of calomel. I then had my doubts of the propriety of depriving the unfortunate patient of what nature so urgently demanded. This opinion was subsequently confirmed, when I found that the treatment adopted at Berlin with the greatest success in the stage of collapse was cold affusion, and allowing, at the same time, the free use of cold water as a beverage. * * *

I am, Sir, yours, &c.,
CHARLES ROBERT BREE.
Ipswich, Sept. 1, 1832.

Observations on the Blood. By W. STEVENS, M.D.

Sur le Sang Humain. Par S. DENIS.

Sur le Sang. Par M. L. R. LECANU.—(Concluded.)

Experimental Inquiries in Chemical Physiology. (*Part I.*) *On the Blood; with Remarks on the Nature, &c. of Cholera.* By HORATIO PRATER, M.D.. London: Highley. 1832. 8vo. pp. 304.

SINCE the publication of our first notice of Dr. Stevens' work (page 659), another volume has appeared on the same subject, which we find to be one well worthy to receive our early attention. We accordingly add the title to our former list, and include a notice of it in the present review.

It is a book which, though by no means exempt even from material faults, displays nevertheless much patience and experimental dexterity on the part of the author, and at least proves Dr. Prater to be sufficiently competent to pronounce opinions on some of the most interesting and important of the questions involved in the speculations of Dr. Stevens;—opinions which, we shall subsequently find, agree materially with those which we have already promulgated. Of this coincidence proof shall be afforded anon.

A clever contemporary, the *Dublin Medical Journal*, has also taken up the discussion, and in its number published three days after the appearance of our first review, affords its readers an exposition of Dr. Stevens' theories, in which almost precisely the same objections as our own are opposed to the Doctor's opinions.

Before proceeding further, a brief recapitulation of the chief features of Dr. Stevens' theories will be desirable, placing in juxta-position therewith the objections which we stated for their disproof; and with a view to economise our space we adopt the subjoined form:—

Theories and Opinions of DR. STEVENS.	*Comments of* THE LANCET *thereon.*
1. Venous blood contains carbonic acid.	1. Admitted.
2. Carbonic acid contained in membranous vessels, and exposed to air, passes through the membrane, and is replaced by oxygen. Such are the aërial changes in respiration.	2. Considered highly probable, and the originality of the opinion awarded to Dr. Stevens.
3. *All* acids, including the carbonic, *blacken* red blood.	3. Doubted; because we have found that a current of carbonic acid does not darken dilute solutions of florid colouring matter.
4. All neutral salts, with alkaline or earthy bases, redden black blood.	4. Admitted.
5. Arterial blood, therefore, owes its scarlet colour *not to oxygen*, but to the effect of *the salts* on its colouring matter, and venous blood is black, because the agency of the salts is prevented by carbonic *acid*.	5. Denied; because we found that removing the carbonic acid by the air-pump from healthy, dark venous blood, *did not enable the salts therein contained to restore its red colour*.
6. Alleged in proof of the preceding facts, that red crassamentum immersed in distilled water becomes black, because the water deprives it of its salts, and again becomes red in a saline solution, even in an atmosphere of carbonic acid.	6. Dr. Stevens's experiments here proved to be erroneous, by our showing that even maceration in water *does not* remove the salts from the crassamentum. This we proved by the analysis of crassamentum thus treated, finding it to contain its full complement of saline matters.

Such is a fair epitome of Dr. Stevens' views, and our objections thereto. The very key-stone of Dr. Stevens' arch—that which holds the entire of his edifice together—that which, once removed, must cause the complete destruction, by its own weight, of the whole erection, consists in his opinion that healthy *venous* blood is *black*, because it contains an *acid* which prevents *the salts* from making it *red*. How do we show the fallacy of this assertion? We remove the acid by the air-pump, and find that the blood is *blacker than before*. We prove, too, that the acid is effectually separated by receiving it into lime-water. Our Dublin contemporary, we observe, goes a step further, and on re-admitting atmospheric air to this black saline blood, discovers that it forthwith again becomes florid. The experimental evidence is here complete, and overturns the whole of Dr. Stevens' theory, from the dome to the foundation. We may add, that since our review was written, we observe that Sir Charles Scudamore, in treating of the pneumatic physiology and chemistry of the blood, has described an experiment of similar import to our own, to which Dr. Prater thus alludes:—

"But though the above facts seem to show that oxygen can decidedly hasten coagulation, there is no reason at all for believing that in the gaseous form it can keep blood fluid, in how large proportion soever it may be mixed with it.

"There is another very obvious difference between the effect of oxygen (or at least air) on the blood and the strong saline solutions. The difference alluded to is the length of time taken by the former to change its colour; but no sooner are the latter added than the effect takes place. From what has been said, it is obvious that we are not inclined at present to agree with a recent opinion, which would deprive oxygen altogether of the power of reddening the blood, and transfer such power to the saline matter necessarily present in the healthy condition of the fluid in question.

"Without entering formally into all the objections that may be brought against the theory alluded to, it will perhaps be sufficient to state the following:

"In Sir C. Scudamore's sixth experiment on the blood (p. 23), by remaining under the void of the air-pump only five minutes, blood was rendered almost black in colour. In his fifth experiment it became considerably darker in colour after the short interval of six minutes and a half. Considering these experiments as they were repeated with similar results (p. 24) sufficient to establish a general principle, how, it may be asked, can the exclusion of air change the quantity of saline matter in the blood? It is equally difficult to see how carbonic acid should produce such a change of colour in this case, since the acid, if present, would surely have a tendency to fly off.

"And indeed, whether or not, it is certain that neutral or alkaline salts never could be employed as substituted for the vital gas. This remark, however, does not affect the opinion that they may be quite indispensable as auxiliaries."—*Prater*, p. 243.

We thus find several commentators, in writings published almost at the same moment of time, drawing the same conclusions against Dr. Stevens, not from the indications of "common sense" which Dr. Stevens deems so conclusive, but from direct, unquestionable, pertinent experiments. The fact of four different experimentalists, ignorant of each other's proceedings, individually observing the same things, and deriving therefrom the same conclusions, must be regarded as almost irrefragable evidence in favour of those conclusions.

We now proceed with our task, commencing at the point at which our strictures were intermitted, namely, the topic of "*the globular appearance of the blood.*" The following extract shows that the author deems that appearance to be created merely by the presence of *undissolved, or free air* (*oxygen* in the *arterial*, and *carbonic acid* in the *venous* blood):—

"I exposed a small particle of pure colouring matter, which was found on the upper surface of an albuminous, or buffy crust, but this of itself was not globular; and we have reason to believe, that neither the fibrin nor any of the other solid ingredients have a globular form, except that which is produced by the air which they contain. Pure rain water has no solid ingredients; but still when this is exposed in the solar microscope, it exhibits a globular appearance, similar to that which exists in the serum. This appearance in the water is evidently produced by innumerable particles of free air, which by some means or other are retained in the fluid, and exist separate and distinct from each other. Now, as these globules have almost exactly the same appearances with those in the serum, and as the globules in the water are evidently produced by the innumerable particles of free air in that fluid, *we may infer, at least for the present*, that the globules in the blood are not produced by any of the solid ingredients, but that the same cause which produces the globular appearance in the water, is also the cause of the globules in the circulating current."

This doctrine is advanced by the author altogether on the following foundation. He commences by adducing experiments with the *solar* microscope, in which he states, 1st, that he has observed that the globular appearance is "nearly as distinct" (page 63) in the *serum* as with the colouring matter. 2ndly (page 63), That after serum has been exposed for a short time to the *high heat* in the lens, the air is driven off and the globules disappear. 3dly, That common water impregnated with air presents a globular aspect.

The whole of these experiments are inconclusive, as well the proofs of the existence of *free* gases in the blood, as of these being the cause of the globular aspect, inasmuch as *the high heat of the solar microscope* would extricate and *set free* any *dissolved* air in the first place; and, secondly, would lead to the generation of *steam* globules, which would put on the same appearance. Thirdly, the experiments of Donné, Raspail, and other distinguished observers, prove that the blood possesses this globular appearance at a temperature too low for the generation of steam. Fourthly, the diameter of the globules in warm-blooded animals is not the same, though the densities of the blood be identical.

But supposing all these objections to be groundless, the following experiment is sufficient to prove Dr. Stevens to be in error. We have found by adapting an *ordinary* microscope to a simple apparatus for exhausting air, that *blood deprived of its air still retains its globular structure.*

But Dr. Stevens, not content with invading the old opinions respecting the globules on the strength of such faulty and imperfect experiments with the *solar* microscope, goes yet a step further, and infers from them, in favour of his chain of hypotheses, that *oxygen* gas exists in a *free* or undissolved state in arterial blood! This summary mode of inference is unworthy of a scientific man. The utmost that the experiments *could* prove (even overlooking their sources of error) is, that *some* gas exists in arterial blood, &c. But Dr. Stevens does not stop here. He identifies the gas without delay, forthwith pronounces it to be *oxygen*, and thus conveniently supplies himself with an argument, which, if once admitted, would certainly work most favourably in corroboration of his doctrines. But of the existence of oxygen gas, whether in a free or dissolved state, in arterial blood, Dr. Stevens gives no proof whatever. It should be remembered, too, that the proof would not be intrinsically difficult if it could be afforded at all. Why did not Dr. Stevens sacrifice a large warm-blooded animal, receive its arterial blood, *collect* the oxygen by careful pneumatic manipulation, and then rigidly examine into the identifying and peculiar properties of the gas obtained? With less accurate proof than this, science cannot be satisfied, and without such testimony must this portion, too, of the new fabric sink to the earth.

The two succeeding sections of the volume, namely, that on the "latent power of attraction," and that on the "*modus operandi* of agents on the living body," do not require any special comment. The author labours hard in the second to disprove the opinion that the mode of action of poisons, medicines, &c. is by an impression first made on the sentient extremities of the nerves. The subject, however, is so much more impartially discussed in Dr. Christison's admirable book, that we must recommend our readers not to be satisfied with Dr. Stevens' arguments without consulting Dr. Christison's also. In the chapter on "Attraction," Dr. Stevens endeavours, and we think successfully, to vindicate the originality of some of his peculiar opinions.

Before leaving this department of the volume, we deem it necessary to notice a statement of the author which perplexes us not a little, and which we refer to here, only for its singularity, inasmuch as it is altogether unconnected with the knot of theories which we are endeavouring to unravel.

At page 57, Dr. Stevens, in describing an experiment on a rabbit, says—

> "A rabbit, apparently about six months old, was killed, by completely destroying the structure of the brain with a sharp bodkin. The chest was opened about fifteen minutes after death. The lungs collapsed as usual on exposure to the air. Both sides of the heart appeared to be gorged with black blood; the coronary vessels were beautifully injected; but on its first exposure to the air, there was not the slightest motion in any of the fibres; and the heart appeared to be completely dead. The pericardium was removed, and *in about two minutes after, one of the superficial branches of the coronary arteries, on the right ventricle, commenced to contract, and acted with force until it had propelled the whole of its contents into the substance of the ventricle; the artery then ceased to act, but only when it was empty of blood.*"

Bearing in mind the myriads of experiments which have been instituted for the express purpose of ascertaining whether or not arteries possess a muscular tunic; recollecting how the arteries of animals, living and dead, have been scraped, and pinched, and galvanized, and acidulated, for the purpose of irritating them to movement; reflecting on the closeness with which they have been watched, under circumstances precisely parallel to those described in the preceding experiment; and, lastly, thinking of the fact, that not one of the eager controversialists engaged in this discussion ever had the good luck to obtain even a glimpse of such a contraction as fell beneath the observation of Dr. Stevens,—remembering all these things, we can only say, that this single discovery, *if corroborated*, will do more to perpetuate the author's fame than nine-tenths of the hypotheses now under investigation.

We now approach the division of the work in which Dr. Stevens' theories are brought to bear on the nature and treatment of disease. The section is entitled "General Observations on Fever," and extends through 190 pages of the book. In our notice thereof we shall carefully eschew all matters irrelevant to the peculiar nature of the work.

The first prominent position of the author is, that there are two classes of fevers, one produced by poisons, another not. At page 175 he then observes,

> "We shall afterwards see that in those fevers that are produced by the aërial poisons, the blood is vitiated, even before the attack, and this, in the essential fevers, is decidedly the first link in the morbid chain. When we inject a putrid fluid into a vein, we derange the blood, and this vitiated blood produces all the symptoms of putrid fever. Or when we inject a weak solution of tartarised antimony into a vein, this acts as a poison in the blood, and produces, for a time, all the phenomena that occur in the first stage of those fevers that are produced by the aërial poisons. And from what I have seen, my belief is, that in all the fevers produced either by marsh miasma, or contagion, the diseased action of the solids is as much the effect of the diseased state of the blood, as it is in those cases where we cause fever by injecting a putrid or a poisoned fluid directly into the circulating current."

Again, with reference to the primarily diseased state of the blood in fevers, he says,

> "Dr. Rush states, on the authority of Baron Humboldt, that when the yellow fever at Vera Cruz proves fatal in a few hours after the attack, the physicians of that city had endeavoured, in vain, to ascertain by dissection the cause of the death; for those parts which were supposed to be the seat of the disease 'frequently exhibited no marks of inflammation, nor of the least deviation from a healthy state.' Now, if the physicians of Vera Cruz could not discover, in such cases, the cause of death, it was merely because they looked for it only in the solids, where it did not exist. Had they examined the diseased condition of the black and saltless blood, with which the heart is invariably gorged, and had they known how impossible it is for such inert blood to stimulate the heart, or support life, they might easily have accounted for the cause of the sudden death without any reference to the condition of the solids."

At page 186, Dr. Stevens still more pointedly insists on the blood becoming changed *before* the supervention of manifest disease. Our readers will see that his assertions are as decided and peremptory as language will permit.

> "Whatever the remote cause of fever may be, it is very evident that this cause must invariably exist, not only at the moment of attack, but even previous to that period. Now, in the fevers from poison, the blood is invariably diseased previous to the commencement of the cold stage. During this period there are premonitory symptoms, but these are evidently the effect of the diseased state of the vital fluid; and that these precursors of fever are not the effect of any local inflammatory disease is evident from the fact, that frequently during this period there is no pain in any of the organs, but a want of action, particularly in the extreme vessels, and consequently, a decrease of heat in the whole system."

And at page 187.

> "This is particularly the case in the worst form of the African typhus, in cholera, the plague, and probably in all other varieties of malignant fever, where the blood is under the influence of an active poison, and where its vitality is diminished and its structure is injured even before the attack."

Further on the author adduces a considerable body of evidence, and many quotations, to show that the deranged state of blood has been found to exist during the prevalence of epidemic fevers, in persons who did not labour under the disease; and, again, in persons actually infected. In all these assertions, however, the author either rests his claim to confidence on mere presumption, or on the evidence of physical, not chemical, observations. It is manifest that we cannot, in matters of philosophic inquiry, be satisfied with this kind of testimony, especially where the exact sciences could have been brought to bear with facility on the subject. Precise analyses of the blood might and ought to have been performed on the specimens, too, respecting which the author speaks of physical alteration. Without this his argument is unstable, and his facts are barren of application. Moreover, we shall presently see that in another pestilential disease, to which his "observations on fever" are applied with as much confidence as if fever itself was the subject of discussion, the primarily diseased state of blood has been completely and satisfactorily disproved by chemical evidence.

But before approaching this subject, we are bound to notice an episode of the author on scurvy, from which our readers will perhaps perceive, that "the saline theory and practice" may, under the blighting influence of indomitable speculation, be converted as readily to the destruction of mankind as, according to its warmest supporters, it is fully calculated for universal good. Our readers will remember that Dr. Stevens asserts that *all* acids blacken and derange the blood. It might be presumed that the paramount utility of *lemon-juice* in sea scurvy was rather contradictory to this opinion. But how does Dr. Stevens meet this fact? why, will our readers believe it? He flatly pronounces lemon juice to be a poison in this very disease, and lauds the efficacy of his saline treatment therein! And all this from one who admits that he never saw a case of scurvy in his life! Our readers shall peruse his own words:—

"Now that the citric acid is fortunately no longer in general use, as a preventive against scurvy, this disease has become, comparatively speaking, extremely rare; and though salted provisions be used as much as formerly on board of the vessels, still I have never seen even one case of scurvy amongst the sailors during the twenty years that I resided in the West Indies; consequently, I have had no opportunity of examining the vitiated state of the blood in this disease."

Again:—"And with respect to scurvy, it would not be difficult to prove, from the writings of Trotter and others, that this disease, which at one period was so distressing in the British navy, was frequent and fatal almost exactly in proportion to the quantity of citric acid, which was used as a preventive, and, unfortunately also, *as a cure* for the disease."

Our duty to the public would not be performed if we failed to place in juxta-position with the preceding most unwarrantable and dangerous statement, the following extract from the Report on the Millbank Penitentiary, by which our readers can judge of the degree of confidence to be reposed in Dr. Stevens' assertions respecting sea scurvy:—

"The next time I saw this disease in a very spreading degree, so as to affect the whole fleet at a period when the existence of the country depended on the fleet keeping the sea, was in the year 1801, when my Lord St. Vincent took the command of the channel fleet. A short time after we sailed and in not more than a fortnight the scurvy made its appearance and spread very rapidly through the fleet. Fresh provisions were not then supplied to it as now, nor vegetables. Being aware that lemon-juice was then in store and could be drawn for the fleet, I expressed to the commander-in-chief my great anxiety that a fresh supply should be had as fast as possible. The fleet was then blockading Brest, a cutter was dispatched to communicate the state of the health of the fleet, a supply of lemon-juice came out, and we gave it freely to those labouring under the disease, and daily mixed with water and sugar, to the whole of the crews of the ships, and continued its use during the time we were at sea which was nearly seventeen weeks, during which time the fleet had not as a fleet a single fresh meal, nor anything in the shape of an anti-scorbutic, but lemon-juice. The disease, under the use of this, totally disappeared; we returned with twenty-four sail of the line into Torbay, out of which number there must have been ten or twelve three-deckers, and I think, estimating fairly, there could not upon an average have been less than seven hundred men in each. Out of twenty-four sail of the line, there was not a single case of scurvy, and what was extraordinary, to such a state of health was that fleet brought by the use of lemon-juice, that the *Glory* had only four men on her sick-list; so that out of fifteen or sixteen thousand men, there were only sixteen subjects for the hospital, and some of the ships had not lost a man at that time."—*Dr. Baird.*

The remaining part of this section is chiefly occupied by the details of various facts which have long been familiar to the public, tending to show the salutary influence exercised by salt on the animal economy. Of this no one entertains a doubt; neither, we believe, will many be found to question the good effect which salt may produce in some states of disease; but we question whether any rational being exists who can peruse Dr. Stevens' book with attention, and note his freedom of assertion, his mode of overlooking facts, and his intolerable errors of reasoning, without acknowledging that he has done little towards encouraging cautious practitioners to persevere in the use of saline remedies. 'Tis said that an injudicious advocate oftener injures than aids a good cause, and we are strongly disposed to think that in the present instance, Dr. Stevens has done more to prevent the employment of salt as a remedy, than the bitterest enemy of that practice could accomplish. We should perhaps here observe, that although a theory like that of Dr. Stevens may be altogether destitute of foundation, yet that *the practice which gave rise to that theory* may be good. But, on the other hand, the futility of the hypothesis will, in the minds of nine-tenths of the profession, outweigh the *practice* itself, and truth thus be sure to suffer.

But in the case before us has *the practice* triumphed in the yellow fever, of which it seems the author had ample experience? We are unable to answer. All that we know is, that his statements on this point are as flatly disputed as those respecting his treatment of cholera in the Coldbath-fields prison. The book before us gives us, on this subject, no tangible evidence beyond the papers by Dr. Stevens, which have already appeared in this journal, and which are reprinted at full length in the volume before us. Indeed, at least 100 pages of the book, or about one-fifth of its whole contents, are occupied by reprints of various papers which have already been universally circulated among the profession.

The only section remaining for us to examine, is the Appendix on Cholera. A considerable part of this consists of a letter addressed by Dr. Stevens, to a contemporary journal, written and published before the cholera reached this country, and before a single analysis of cholera blood had been performed; yet, in this letter, the author's peculiar theories are insisted on as strenuously and pointedly as they were in the case of the yellow fever. The author, as usual, first leaps at his conclusion, and then presents us with the analytic labours of Turner, Thomson, and O'Shaughnessy, in a cavalier note of seven lines at the foot of the page, as corroborative of his own views. This, in matters of literature, is too bad.

The details of the mode of putting into practice the saline method in cholera, and the controversy regarding the honesty of the Coldbath-fields reports, occupy the remainder of the volume. We deem it unnecessary to enter into any lengthened notice of the first, because it is now proved, experimentally and analytically, that the blood does not undergo the change which Dr. Stevens presumes, until profuse evacuations have taken place. The saline practice is, therefore, independently of all theory, inapplicable to the premonitory symptoms. As for its utility in confirmed collapse, we leave to those who will be bold enough to trust to it, the grateful task of narrating their success.

When Mr. Thackrah's second edition of his work on the blood reaches us, we will return to Dr. Prater's volume.

The Anatomy and Physiology of the Organ of Hearing; with Remarks on Congenital Deafness, the Diseases of the Ear, some Imperfections of the Organ of Speech, and the proper Treatment of these several Affections. By David Tod, Member of the Royal College of Surgeons. London: Longman and Co. 1832. 8vo. pp. 147.

The work now before us contains many original views, both on the anatomy and physiology of the human ear. We shall condense and extract those parts which seem most remarkable for novelty, that such of our readers as are devoted to minute anatomical research may have an opportunity of comparing Mr. Tod's statements with the results of their own inquiry or dissection. If we refrain from expressing any opinion of our own, it is because Mr. Tod has described various structures and membranes unknown to Scarpa, Sœmmering, or the best writers on the anatomy of the ear. It is, however, but justice to say, that the author is prepared to demonstrate the existence of the new parts described by him, by numerous preparations in his possession.

After a description of the external ear and its muscles, to which latter the author has added two, Mr. Tod proceeds thus to describe the membrana tympani.

"The *membrana tympani*, which separates the external from the internal portion of the ear, is nearly of a circular form in the adult, and of an irregular oval in the child, with its short diameter placed somewhat transversely and obliquely.

“ It is composed of two laminæ or membranes, an external and an internal; of a muscle, and a number of blood-vessels, nerves, and absorbents.

“ The *external lamina* is merely a continuation of the cuticle which lines the meatus externus. It is extremely thin and delicate, and can be readily demonstrated by maceration or boiling.

“ The *internal lamina* is a continuation of the membrane which lines the cavity of the tympanum, called the *membrana propria tympani.* It is firmly fixed to the internal margin of the groove of the os annulare, and to the whole length of the manubrium mallei.

“ The *muscle* which has been named *musculus tympani*[*] arises from the groove of the os annulare, and from thence runs obliquely inwards like radii from a circumference towards a centre, between the external and the internal lamina, and is

“ Inserted into the apex and whole length of the external surface of the manubrium, and into the surface of the processus brevis of the malleus.

“ Action. To draw the parts into which it is inserted according to the direction of its fibres: those which are fixed to the manubrium draw it outwards in various directions, and those which are attached to the processus brevis draw it upwards and inwards.

“ The *musculus tympani* is a beautiful semitransparent muscle, composed of a very thin and delicate fasciculus of fibres, connected to its coverings by cellular substance.”

In the cavity of the tympanum, the author seems to have found a wide field for discovery, and has accordingly described and delineated several new muscles and ligaments.

“ The *ossicula auditus* have the following *muscles*, *membranes*, and *ligaments*, attached to them.

“ 1. *Anterior Mallei*[*] arises by a very small beginning from the extremity of the spinous process of the os sphenoidale, and from thence runs backwards, outwards, and upwards, along the outer surface of the eustachian tube, and through the fissura glaseri and canalis anterior, from the surface of which it receives many fibres, and is

“ Inserted into the whole length of the processus gracilis, and interior surface of the head of the malleus, where there is a small eminence near the cervix.

“ 2. *Posterior Mallei* arises broad, thin, and fleshy, from the sulcus semilunaris and parts above it near the groove of the os annulare, and from thence passes forwards and inwards, and is

“ Inserted into the posterior surface of the manubrium mallei below its processus brevis.

“ 3. *Internus Manubrii Mallei* arises by a very small beginning from the posterior cartilaginous extremity of the eutachian tube, and broad and fleshy from the sinus muscularis. The fleshy fibres from these origins terminate in a slender round tendon, which runs directly outwards from the sharp osseous ridge at the termination of the anterior extremity of the processus fallopii, and is

“ Inserted into the inner surface of the manubrium mallei behind, but a little below its processus brevis.

“ 4. *Anterior Capitis Mallei* arises from that part of the outer surface of the fossa navicularis just before the head of the malleus, and from thence runs obliquely backwards and inwards about the twelfth part of an inch, and is

“ Inserted into the depression in the anterior surface of the head of the malleus.

“ 5. *Superior Capitis Mallei* arises fleshy from that part of the superior surface of the fossa navicularis just above the head of the malleus, where it is articulated with the body of the incus, and from thence descends, and is

“ Inserted tendinous into the superior and inner part of the head of the malleus, close to the margin of the articulating surface.

“ 6. *Obliquus Incudis Externus Posterior* arises fleshy from the external surface of the fossa navicularis just before the beginning of the mastoid cells, and from thence descends obliquely forwards and inwards, and is

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[*] The author asserts that this muscle was discovered by Sir E. Home. Perhaps Sir Everard may have been the first to attribute to it muscular properties, but the middle membrane of the tympanum had long been known to anatomists, and described by them as fibrous. The propensity of Sir E. Home “ to describe every structure as muscular, is well known, even though the fibres should be inconceivably minute, and united together by an elastic substance of the consistence of mucus.” Analogy would lead us to suspect this middle membrane to be fibrous, for every structure which vibrates for the production of sound in the animal body, is of a fibrous nature.—Ed. L.

“ [*] I have named the muscles more from their situations than their apparent actions. The names already in use are very objectionable, particularly the laxators and tensors; for the membrana tympani is always kept tense, after the os annulare becomes amalgamated with the entire bone, by the action of its own muscular fibres, which are more powerful than the combined strength of all those belonging to the cavitas tympani.”

"Inserted into the middle of the superior surface of the processus brevis incudis.

"7. *Triangularis Incudis** arises broad and thin from a part of the sulcus semilunaris on the inner side of the posterior mallei, and from the root, and sometimes from the whole length of the posterior surface of the manubrium mallei. From these origins the fleshy fibres run in a converging manner, the posterior obliquely forwards and inwards, the superior obliquely downwards and inwards, and the anterior obliquely downwards, backwards, and inwards; and the whole are

"Inserted broad and thin into almost the whole length of the outer surface of the processus longus incudis, and the anterior surface of the cervix of the stapes.

"8. *Stapedius Posterior* olim *Stapedius* arises fleshy from the sulcus stapedius, and from thence passes forwards, and a little upwards and outwards, in the form of a pyramid, and is

"Inserted tendinous into the depression in the posterior surface of the cervix of the stapes, and membranous into the posterior surface of the button-like process of the incus, and a small portion of its long process.

"9. *Musculus or Structura Stapedius Inferior.*—This muscle or structure, (for I have not as yet been able to discover muscular fibres in it sufficiently clear to warrant my calling them by that name, although it looks more like muscular texture than any other,) arises broad and thin from the middle of the outer surface of the eminentia pyramidalis, and from thence extends obliquely upwards and outwards in the form of a pyramid, and is

"Inserted into the under surface of the cervix and crura of the stapes.

"There is another structure apparently muscular extending from the external surface of the fossa navicularis directly opposite to the head of the malleus and body of the incus, and fixed to those parts of the bones.

"The *membranes* are,

"A broad, thin, and very delicate membrane extending from the whole length of the outer surface of the body and short process of the incus, directly outwards to the adjoining surface of the fossa navicularis.

"A broad, thin, and very delicate membrane, which extends from the inner surface of the curved margin, which is formed by the union of the long and short processes of the incus, from the corresponding part of its button-like process, and from the superior surface of the cervix of the stapes and membrana stapedis, directly inwards to the adjoining surface of the septum tympani.

"A broad thin membrane which extends from the inner surface of the head of the malleus, and adjoining surface of the body of the incus, directly inwards to the opposite surface of the fossa navicularis.

"A broad thin membrane which extends from the surface of the internus manubrii mallei as it runs towards its insertion, obliquely upwards and outwards to the surface of the processus gracilis and cervix of the malleus.

"The *ligaments* are,

"A capsular ligament which connects the articulating surface in the head of the malleus to the corresponding surface in the body of the incus.

"A capsular ligament which connects the articulating surface of the button-like process of the incus to the corresponding surface in the head of the stapes.*

"A strong broad ligament which connects the outer surface of the processus brevis incudis at the apex to the outer surface of the fossa navicularis, close to the beginning of the mastoid cells.

"A strong broad ligament which connects the inner surface of the processus brevis incudis at the apex to the adjoining surface of the fossa navicularis.

"These two broad ligaments are each about the sixteenth part of an inch in length, and the external is situated immediately below and behind the obliquus incudis externus posterior muscle.

"These broad ligaments, with the processus gracilis, form the pivots to the various motions of the ossicula auditus.

The Motions of the Ossicula Auditus and the probable Actions of their Muscles.

"When we press the external surface of the head of the malleus directly inwards, the body and short process of the incus are also moved in the same direction, whilst the malleus appears to move as if it was suspended by the cervix like a steelyard; for while its head is pressed inwards, its manubrium and processus brevis are forced outwards, so as to cause that portion of the membrana tympani which is fixed to the former to be less concave, and that which is attached to the latter to be more convex externally. But these are not the only actions which we observe on pressing the head of the malleus inwards; for whilst we are forcing its manubrium and processus brevis outwards, we are at the same time

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"* This muscle is noticed by a Dr. W. Holder in the year 1668. Vide Philosophical Transactions, vol. iii. p. 666."

"* When we dissect the cavity of the tympanum by elevating the inferior part of the os annulare, we almost invariably rupture the capsular ligament of the stapes, unless it be done with great care."

drawing the processus longus incudis and the stapes in the same direction.

"When we press the internal surface of the head of the malleus directly outwards, we force its manubrium and processus brevis directly inwards, and make the whole surface of the membrana tympani more concave externally, and at the same time force the processus longus incudis and the stapes directly inwards, so as to cause the whole surface of the base of the stapes to press upon the membrane of the fenestra ovalis.

"When we press the anterior surface of the head of the malleus backwards, its manubrium together with the processus longus incudis and stapes, are forced forwards, so as to cause that portion of the membrana tympani which is posterior to the manubrium to be less, and the other portion anterior to it to be more concave than usual; and at the same time to cause the anterior half of the base of the stapes to make a greater pressure on the membrane of the fenestra ovalis than its posterior half.

"When we press the body of the incus downwards and forwards, we force its long process and the stapes, and also the manubrium mallei, directly backwards, and produce in the membrana tympani and fenestra ovalis actions the reverse of those stated in the preceding section.

"When we press the body of the incus directly downwards, we force the manubrium mallei inwards, and at the same time cause the inferior or straight margin of the base of the stapes to produce a greater degree of pressure upon the membrane of the fenestra ovalis than its other portion. And when we press the incus from the extremity of its long process directly upwards, we cause the semicircular margin of the base of the stapes to make the greater pressure upon the membrane of the foramen ovale.

"We also observe in every motion which the ossicula auditus make, or are capable of making, that the chorda tympani is moved along with them.

"All these motions which we have described, and have produced by violence, are performed by the muscles of the ossicula auditus. For when we draw in the natural direction of the anterior mallei forwards, and in that of the posterior mallei backwards, at one and the same time, we force the manubrium mallei and its processus brevis directly outwards, and the remaining bones follow. When the internus manubrii mallei contracts, which it does inwards, the manubrium follows it, and the motions of the incus and stapes take the same course.

"The anterior capitis mallei draws the head of the malleus forwards. The superior draws it upwards and outwards. The obliquus incudis externus posterior draws the incus and head of the stapes obliquely upwards and backwards. The triangularis incudis draws the processus longus incudis and the manubrium mallei in the direction of its fibres. The stapedius posterior draws the head of the stapes and processus longus incudis backwards and inwards; and the stapedius inferior (if muscular) draws the stapes downwards and inwards."

The account of the labyrinth and semicircular canals agrees generally with that given by authors; but Mr. Tod says, he has never been able to discover the aqueducts of Cotunnius. He closes this portion of his work with the following directions for exposing the apparatus tympani.

"Take the temporal bone of an infant or fœtus, and after injecting it, remove all the soft parts from its surface, keeping the membrana tympani entire; then introduce the point of a small scalpel between the os annulare and the petrous portion on the under surface, and separate the one gently from the other, so as to raise the two inferior thirds of the former about a quarter of an inch from the latter, and put a small piece of wood between them until the parts become quite dry. The different parts will then remain permanently *in situ*, only a little on the stretch. Again, take another temporal bone of the same age, and gradually remove the osseous shell which forms the upper part of the fossa navicularis and the inferior part of the cavitas tympani, and put the preparation aside to dry. Every texture will then appear *in situ*, and in a state of integrity. By repeating these dissections with slight alterations, every structure will be demonstrated in a variety of ways."

We shall, in a future number, notice the second part, which relates to the physiology and diseases of the ear.

THE LANCET.

London, Saturday, September 8, 1832.

A PESTILENTIAL disease of strange and overwhelming malignity has raged for nearly a year in various parts of Great Britain, and up to the period at which we write, we find that the ratio of the mortality to the total amount of cases, is not diminished, either from a decrease of intensity in the epidemic, or by the practical experience in its treatment which our medical men have yet acquired. Despite the discoveries of specifics daily trumpeted to the world, despite the exulting statements of successes thereby atchieved, the reckless, unsparing plague still ravages some of the fairest spots of our once salubrious islands, usurping the place of our older and milder epidemics, and causing a daily amount of *sudden* mortality, more like the desolation of warfare than the progress of disease.

During the preceding week, it has been our unhappy task to witness examples of domestic distress such as should urge all men of the least humanity to rack their faculties for the devising of some means of staying the progress of this sad calamity. We have been accustomed to the typhus and dysenteries of Great Britain, we have known tents pitched for the sick on the public commons, and we will not deny that from these diseases we have witnessed full many a scene of bitter domestic woe. But aught to parallel the ruin which the existing malady creates by the sudden annihilation of family ties, we know not, nor have read of, in the progress of any other epidemic, since the cessation of the old "sweating sickness." We do not mean to deny that as yet, comparatively speaking, we have been mercifully dealt with; nevertheless, this comparative mercy but proves the innate malignity of the pest, and, even as it is there is scarcely a member of society, moving in any extended circle, who has not already felt the loss of some acquaintance, relative, or friend.

Under these circumstances we repeat, it becomes an urgent duty for us all to bestow our deep attention on the subject, and this duty is rendered still more imperative by the fact, that the fatal epidemic has already made its re-appearance for the second and third times in the various parts of the continent it first visited last summer; neither can we rationally hope that a few months or years will rid us of the causes of the malady, when we find by the latest arrivals from India, that six months have not elapsed since the garrison of Colombo, in Ceylon, was decimated by a sudden irruption of the disease.

It seems then to us, after deep reflection on the subject, that the IMPROVEMENT *in the method of treating the malignant cholera*, should be the paramount object in our future exertions. Of the remote causes of the disease we know nothing, absolutely nothing. But we confess that the chances of elucidating these causes do not seem to us to present an encouraging field for the investigator. The symptoms of cholera and its pathology, whether anatomical or chemical, are now sufficiently well known; anatomy has scrutinized every fibre and tissue of innumerable bodies, and chemistry has not been idle in the analysis of the blood and various fluids peculiar to the disease. And what is the result? The anatomical pathologist confesses that he can discover nothing; the chemist, although he has made many interesting observations, still is forced to admit that the most remarkable features of the disease remain unexplained by his experiments.

The *treatment* then being the grand object now before us, let us inquire briefly into the causes which interfere with the discovery and propagation of methods of more than ordinary value. We hear much of the success of *venous injections*, the *saline treatment* is equally extolled, the Austrian system of *ice* and *emetics* is praised with peculiar vehemence. Yet, on the other hand, we are told by numerous writers, that these remedies are altogether worthless, or positively injurious. Between such conflicting authorities, the mass of the profession is unable to decide; and thus, instead of any uniform mode of treatment, every town and village has its different system or systems, while the daily lists of mortality proclaim the general inefficacy of the whole.

Now, notwithstanding these facts, it is

not *impossible* that a *specific* for cholera has already been discovered. It is *certain*, moreover, that some of the vaunted systems are really of very great value, and it is indisputable that a proper and well-conducted investigation into the mode of treating cholera, *must* lead to the most important and salutary results. But how is this *proper investigation* to be conducted? How are we to avoid the exaggeration or knavery of the traders in specifics? How to avoid the envy of others unwilling to contribute to the fame and emoluments of some discoverer of a successful method? How are we to ensure that the proposals of any individual are carried into practice with all the minute observances which, in the proposer's mind, contribute most materially to success? And, lastly, How are we to secure the general adoption of the methods which this investigation would show to be most beneficial? We proceed to lay a plan before the public, deliberately and carefully devised, calculated, we believe, to embrace all these objects. To this we entreat the attention of our readers.

An inquiry of this kind should be instituted at the expense of the *nation*, and conducted by medical men of indisputable integrity, acknowledged ability, and ample experience of disease in general, and this disease in particular. The *clinical investigations* should be pursued in the localities where the epidemic presents itself in its worst forms.

To meet these objects, a travelling commission, consisting *at least* of three medical gentlemen, should be formed, and placed under the orders of the Government Board of Health. Their business should be to repair to the worst scenes of the epidemic at a moment's notice, and there put in practice any method of treatment directed by the Board. To afford a fair field for their investigations, the expense of the temporary experimental hospital should be defrayed from the *national* treasury.

In giving directions to the moveable commission for the institution of these clinical trials, the Government Board should not be altogether invested with a discretionary power. We must explain.—No proposal of treatment, for example, should be carried into extensive trial at the public expense until some evidence of its probable value is previously obtained. Thus the Board should not be permitted to institute a method never before tried, and merely proposed on uncertain theoretical data. Sufficient evidence should be first laid before them of the promising nature of the proposal.

The moveable commission should be in every respect the agents of the Board, and be totally deprived of any discretionary power in the treatment of the cases, ample and minute directions being in every instance given by the Board. As the proposer of a practice would probably desire to see his intentions exactly carried into effect, he should be permitted, or rather invited, to inspect the treatment of the cases, and see that everything was accurately done. The expenses of the proposer should of course be defrayed from the public treasury, should a favourable report be made on his method by the moveable commission.

As the peculiar service of the commission would expose them to constant and intense danger, provision should be made for their families in the event of their falling victims to their public labours.

In the management of any system of treatment, the cases should be visited *at least every hour*, and minute reports forthwith entered in a book, and signed by the officer in attendance. Each method of treatment should be carried into trial on from ten to fifty patients. A report should be made on the termination of each general experiment, and such report published in the medical and other public journals.

In illustration of the operation of the

preceding plan we will suppose a moveable commission already formed. The disease suddenly breaks out at Bilston, Bristol, or Plymouth. The commission is dispatched to the spot, supposing the parochial authorities to consent to the offer of Government to defray the hospital expenses. The commission having arrived, and a house of reception being in readiness, a mode of treatment is to be tried. Dr. Hardwicke Shute, of Gloucester, addresses memorials to the Central Board detailing a peculiar method which he has found *in practice* eminently successful. The commission is forthwith directed to give this practice a trial on twelve or twenty cases, and report the result. Dr. Hardwicke Shute is invited to attend, and the report of the commission (with the purport of which, previous to its publication, he should not be made acquainted) is accompanied by Dr. Shute's certificate of his practice having been fairly tried in all its details.

It is almost needless to enumerate the important benefits which the operation of such a plan would confer upon society. The moveable commission having no *private* interest whatever in the result of their inquiries, would not be exposed to the temptation of exaggerating or detracting from the success of any particular method. Professional jealousy would be completely avoided. The public would be shown the fallacy and danger of relying on inert nostrums, and be taught what they should spurn and what they should confide in. All this too would be done in the shortest possible space of time. The desultory and imperfect trials of new systems all over the country, whenever a novel or attractive remedy or system is pointed out, would be at once superseded. The all-important good of showing the best method of treatment, and securing for it the general confidence of the public and profession, would be attained; and all these advantages would be accomplished at an expense too insignificant to be taken into consideration. But even supposing the possible expenditure considerable, how insignificant is any pecuniary outlay contrasted with the advantage to be obtained! Even supposing the contingency of wealth and title to be the reward of the discovery of some proved specific, does there exist the wretch who would grudge the outlay for such deserts?

But to the preceding plan, as to all others, we must expect objections to be started. These will proceed probably from the Government, and probably from a certain portion of the public. Governments are ever averse to take the advice that is *publicly* given. The doing so is apt to be thought a confession of their own want of prescience and sagacity. But are such motives to operate when our fellow-citizens are perishing by an appalling pestilence,—when families rising in rude and merry health, are torn asunder or swept away before the setting sun,—when, in short, events are hourly occurring, such as even our profession, familiar as it is with sorrow in many shapes, has never witnessed in this hitherto favoured country?

It may be stated, too, in opposition to our views, that the Government Board of Health has too much occupation already. Were we to admit this fact to be correct, we must question the great utility of their present employment, when compared with that to which we would apply their services. What good, we ask, is there to be achieved by the collection and daily publication of columns of places and figures, if no *effectual* means be taken to diminish the abundance of each? Are the services of this most well-meaning and competent body to be restricted to the supervision of the accounts of parochial boards of health, and to the aiding the Lords of the Council in the construction of pecuniary orders? They ought to possess a nobler and more generous ambition.

It may also be objected, that teasing

claims would perpetually be made by empirics and pretenders, whether in or out of the profession, for the purpose of bringing their quackeries into greater notoriety. It is easy to meet this difficulty. Let the Privy Council give orders that no proposal be received by the Board, unless it be accompanied by the approving testimonials of two or more regularly-educated practitioners, and then this difficulty is at an end. The last difficulty which might be started, may be founded on the danger of popular clamour being excited by the mere idea of an *experimental* hospital. With respect to this, we must, in the first place, state, emphatically, that, up to this moment, all public and private practice in this disease, is strictly and exclusively *experimental*, aud still more, that the populace being *now* well aware of the fact, feel and admit its necessity. They offer no objection to the trial of new remedies and systems. What they cry out against and fear most is, the possible occurrence of such events in the hospitals as have just disgraced that of Manchester. Let there be an order in council forthwith issued, threatening the utmost penalties of the law to all medical men who will dare to convert any part of deceased cholera patients to anatomical purposes within the precincts of an hospital. The body should not even be examined without the written permission of the relatives or friends of the dead. Let this order be printed and posted on the gates of every cholera-hospital, workhouse, and church-door, and we are satisfied that all clamour will cease, and that the people will receive the proffered assistance with satisfaction and gratitude. Besides, there is no necessity for any parade in conducting such a solemn inquiry. Let the use of the word "experimental" be carefully avoided in all reports and publications on the matter, and our knowledge of the people justifies us in promising that no theatre of the labours of the proposed commission will prove the scene of riot, violence, or confusion.

Deeply impressed as we are with the importance of the foregoing proposal, we urgently invite the profession and the public to give it at once their most serious and speedy attention.

Having in a previous number presented our readers with a copy of the Anatomy *Bill*, we lay before them in this LANCET a verbatim transcript of the Anatomy *Act*. The document may elicit from us some observations on another occasion.

TREATMENT OF
MALIGNANT CHOLERA.

EXTERNAL.

1. *Bleeding.*
   General.—*Arterial or venous.*
   Local.—*Cupping, leeches.*
2. Bandaging *for relief of spasms.*
3. Cold.—*Affusion, application, ice.*
4. Cautery.—Moxa, blisters, epithems.
5. Dry cupping.
6. Friction.—*Simple, medicated.*
7. Warmth.—*Air bath, vapour do., water do., sundry applications.*
8. Galvanism.—Electricity.

INTERNAL.

1. Alkaline salts.
2. Antispasmodics.
3. Astringents.
4. *Drinks.—Cold, warm, simple, medicated.*
5. Emetics.
6. Enemeta.—*Simple, medicated.*
7. Inhalation.—*Steam, simple or medicated, and gases.*
8. Purgatives.
9. Narcotics.
10. Sedatives.
11. Stimulants.
12. Tonics—*especially febrifuge t., or those used in intermittent diseases.*

Injection of saline substance by the veins.

Attention must always be paid to the periods when the remedies are used, whether during the premonitory stage, the collapse, reaction, or convalescence.

AN INVALID SUBSCRIBER TO
THE LANCET.

DIPLOMA
OF THE
COLLEGE OF SURGEONS.

To the Editor of The Lancet.

Sir,—Your last number contained rather a pathetic complaint against Mr. Andrews, for having presumed to insinuate that a pupil had passed an examination in Lincoln's-Inn-Fields, which was neither creditable to himself nor to his examiners. This *might possibly* be true, and the opinion not intended to injure the gentleman who supposed himself reflected on, though I am far from believing that it was really the case. The friends of the examinee would have done well to have avoided appealing to the justice or sympathy of your readers on the grounds of its being "too bad" to make so scurvy a return for the £22 so unwisely bestowed. I have some reason to know the party complaining, and I believe nothing to the disadvantage of his accomplishments nor to the integrity of his principles; but I really cannot accord him my commiseration for sufferings which he brought on himself, and for which he deserves no compassion. His partiality for examination will excuse me for subjecting him to one; and if he were near me, I would propound to him several inquiries, which, however well he may have responded to the queries of the court of examiners, will, I am sure, puzzle him to answer to my satisfaction or his own. I would ask him whether he seriously believes there is any value, real or presumed, attached to the college diploma, among sensible men? Whether the examination is a test, or the diploma a testimony, of ability? Whether he could not dispense with the diploma, and yet become a legally-qualified surgeon? Whether, if his good fortune destines him for either of the "services," the presidents of their medical departments would take his diploma as a proof of his fitness, without a farther examination and another inroad on his pocket? Whether he is weak enough to value his diploma and vain enough to be proud of it? Whether he thinks the London College would protect him against the rivalry of an undiplomaed practitioner in the shape of a charlatan, or a scientific man whose wit was more abundant than his money? Whether he can prove that the diploma is not an evidence ONLY of the gullability of one party and the rapacity of another? Whether young men are not awakening to a sense of the worthlessness of the diploma of the London College of Surgeons; and many of them, not having more bowels than brains, declining to waste their money on its purchase? I am, Sir, One

Who has not yet, nor ever intends to exemplify in this purchase of the diploma of the London College of Surgeons, THAT A FOOL AND HIS MONEY ARE SOON PARTED.

September 2, 1832.

LONDON HOSPITAL.—MR. ANDREWS.

To the Editor of The Lancet.

Sir,—A letter which appears in The Lancet for the 1st inst. relative to a misunderstanding stated to have taken place at the London Hospital between Mr. Andrews, one of its surgeons, and a gentleman who has lately passed his examination at the College of Surgeons, leads me to request you will give insertion to the following statements.

I passed my examination at the College the same evening as the gentleman above alluded to, as did also a third pupil of the London Hospital. I met Mr. Andrews the following morning at the hospital, when I heard him pass many remarks, in the presence of other pupils, upon our examinations; but, I unequivocally assert, I heard no remark of his that could justify the observations of "Junius," no animadversion disreputable to himself as an examiner of the College, or that could detract from the character of the gentleman in question. I have seen this gentleman since his examination, who told me that he felt sure Mr. Andrews had shown no improper feeling towards him, and that he disbelieved all report to the contrary. I remain, Sir, your obedient servant, H. J. Brown.

21, Vine-court, Spitalfields.

* The real name and address of "Junius" were forwarded to us in authentication of his letter.—Ed. L.

TO CORRESPONDENTS.

Medicus could not have offered a worse reason for the publication of his paper. The contemporary in question is continually in the habit of inserting communications which had been previously declined by us. We could point out scores of pages in the periodical mentioned by our correspondent, thus filled, and a month rarely passes without an addition being made to the stock.

Mr. J. Peele. They are *not* doing their duty, and have been told so a thousand times.

Mr. G. Jones. The party may *style* himself what he likes, but he must *practise* according to law. We never heard of the "nuts of the Grey Nicker."

An Injured Party, Worcester, must send us his name and address in confidence.

Is the signature *J. Bolus* anonymous? If not, and he will add his address, the letter shall be inserted. Otherwise the testimony is of no value.

THE LANCET.

Vol. II.] LONDON, SATURDAY, SEPTEMBER 15. [1831-32.

PRACTICAL REMARKS

ON

ERYSIPELAS,

AS IT APPEARED ON BOARD

H. M. SHIP PRINCE REGENT,

IN 1831-32.

With Observations on the different Remedial Measures employed.

By John Gooch, *Esq., Surgeon, R. N.*

Remarks on erysipelas may be thought superfluous in the present day, when so much has been written, and so ably written, by some of our most eminent surgeons, on the subject; but I hold that every man who sees much of any particular disease, will always have something novel or interesting on the subject to communicate to the profession. This consideration, and this alone, induces me to offer the following observations on what I consider to be peculiarities in the disease during its appearance as an epidemic on board the *Prince Regent*, 120 guns, in 1831-32. In addition to this I shall offer some practical remarks on the different modes of treatment adopted by me.

I shall first premise, that H. M. ship *Prince Regent* previous to her sailing in May, 1831, for Spithead, to join the summer squadron under Vice-Admiral Sir Edward Codrington, had been employed about nine years as flag-ship to the Commander-in-Chief in the river Medway; and as ships in this situation, i. e. flag-ships, are always considered "show ships," it is necessary that their decks should be very white and clean. To accomplish this, there are, unfortunately, many commanding officers, who think that nothing but "water and holystone" will do, and thus it often happens, that on board those ships you have, from morning till noon, a chance, in moving about the decks, of having either your shins broken by the ropes, or your shoes filled with salt-water. This plan being followed up for a succession of years, the ship must necessarily become saturated; the drainings must go somewhere.

Send a ship of this sort to sea, fully manned, with 400 of her men, or thereabouts, sleeping on her middle and lower decks, with the ports closed; and although all is beautiful and clean to the eye, emanations of an unhealthy character will take place, diseases will be engendered, and then the question is, "How could they originate?—They cannot be spontaneous!—They must have been brought into the ship!" On the other hand, there are many commanding officers who are extremely careful to have a dry ship and dry decks, and they are rewarded for this by the healthy state of their crews, and the comparative lightness of the sick list. It certainly occasionally calls forth an exclamation of "D—n the dust," but this is all the harm known to be produced, beyond, perhaps, the exciting of a tickling cough in a delicate youngster during the time the clouds are flying.

The disease made its first appearance on board the *Prince Regent* after her arrival at Spithead, on or about the 20th June. Previous to our leaving Sheerness, phlegmasiæ had been common, and some cases of intermittent, with obstinate glandular swellings, had occurred. After our arrival at Spithead tertians and remittents were abundant; the germ of which diseases had, combined with a damp ship, no doubt been imbibed from the marshes in the Isle of Sheppy prior to her sailing.

As to what I consider to be the peculiarities of the disease as it occurred on board the *Regent*,—one was its disposition (when attacking idiopathically) to seize on the glandular structures of the body, and on particular sides. If, for instance, it attacked the throat, it was the *left* tonsil; if the femoral glands, it was those on the *right* side; if the mamma, it was the *right* side; if the testicle, it was the *left*. From this there was *no deviation*. If the disease was produced locally (i. e. by wounds or injuries of any sort), then the glands of each side became swollen, and often suppurated, according as they lay within reach of the disease.

As to the premonitory symptoms, the rigors were often so severe, that it was difficult to say at the moment if the patient was attacked by a paroxysm of intermittent, or by the prevailing epidemic. In the latter case, the stage of excitement always called for the free use of the lancet, and the bowels to be freed by a bolus of calomel, washed down with ℥ss sulph. magnesia. The next day there would be a perfect remission of the febrile symptoms, and the patient would declare himself convalescent. He would then get a solution of neutral salts, combined with the tartrate of antimony, *ter die.* The next day he would be still better, and often beg that his grog might be allowed him, but at the evening visit he would be sure to complain of a sore throat and difficulty of swallowing. On examination, the *left* tonsil would then be found swollen and inflamed, and we at once knew what was to follow, viz. erysipelas of the face, and probably of the hairy scalp, and the next three days would determine whether the man standing before us should live or die, so rapidly did it spread after this to the mucous membranes of the nose and throat, and over the integuments of the face and scalp. In these cases, a large blister to the external fauces—keeping the bowels free by a solution of neutral salts, combined with the tartrate of antimony,—and scarifications of the face, with incisions through the integuments of the hairy scalp, afforded the most relief.

Employment of Lunar Caustic.

And this brings me, in the first place, to Mr. Higginbotham's practice of applying lunar caustic in cases of erysipelas, which I do not hesitate to affirm will be found a remedy of great value as an external application in erysipelas of the head and face. That we saved three lives by it in the *Prince Regent* I am morally convinced, and for the truth of this, I appeal to my two very able assistants, then in the *Regent*, viz., Mr. Watt, now of Greenwich Hospital, and Mr. Samuel Browne, at present in the *Victor* sloop of war. But—with all deference to Mr. Higginbotham—to produce these beneficial effects it must not be applied after his manner, that of "washing the parts, and then drawing the stick of caustic over them;" but a very strong solution, strong enough just to vesicate the parts, must be applied by means of a small piece of sponge or soft lint over every part of the face and scalp affected with erysipelatous inflammation; and that very effectually too, as a great deal depends on every part of the surface affected by the disease being fully vesicated by the solution. The patient will probably roar out lustily during the process, or if comatose, will struggle against the application, from the pungent pain it produces; but this only lasts about fifteen minutes, when a calm takes place, followed by a good night, and in the morning you will probably find your patient in a state of perfect apyrexia, collected, and free from pain; every part of the head and face, however, touched by the caustic solution, will be perfectly *black*, but desquamation takes place in about a fortnight, and the skin resumes its natural colour, and your patient, from the relief he has experienced, is very well content to be a "south down" for so short a period.

The first case in which I used this application was that of a stout lad of the name of Tulle, 18 years of age, who had extensive erysipelas of the face extending over the frontis, and making its way rapidly to the hairy scalp. He had been bled, purged, had cold washes constantly applied, and had taken salines with antimonials, but still the disease progressed, the pulse fell, and at times intermitted, and he was also becoming comatose. I now determined to apply the lunar caustic, and, with a strong solution, fairly and fully pressed every part of the erysipelatous surface, the patient crying out lustily all the time, from the pain caused by the application. This was at the evening visit. In the morning there was scarcely a vestige of fever remaining; he had slept well and was quite collected, and from this time hardly required any medicine beyond a slight tonic during convalescence. In a fortnight desquamation of the cauterized surface had taken place, the face resumed its natural appearance, and he returned to his duty.

Another man, of the name of Poole, ætat. 22, after a severe paroxysm of fever, had the peculiar affection of the *left* tonsil, followed by inflammation of the mucous membrane and integuments of the nose, spreading upon each side over the face. We attacked this with nitrate of silver solution, and stopt the inflammation, so that there might be seen about the *Regent's* decks one man with a black snout, another with a black face which would defy the recognition of his nearest friend, another with a black ear, and so on, according to the locality of the disease, causing a great deal of conversation among the ship's company how these things should be, after which the remark would follow that the "doctor was always either cutting or burning somebody;" alluding to the incisions necessarily made in very many cases of erysipelas of the extremities.

Sir Richard Dobson's Mode of Scarification or Puncturing.

This was extensively tried by me in what may be called circumscribed cases of erysipelas, (I know the objection to which the term is liable,) but I mean those cases in which the surface over the patella is attacked, only extending partially around the joint. Of these we had great numbers, with others attacking the middle third of the anterior part of the leg, and connected with inflammation of the femoral glands. In all these cases I scarified freely with the shoulder of a bleeding lancet—more freely indeed than recommended by Dobson; and although the parts would often bleed so copiously as to produce partial syncope in strong athletic seamen, I know of no one case that did not go on to suppuration. That great benefit was derived from the hemorrhage I allow, but the disease was never stayed.

In extensive erysipelas of the face this method has proved, and always will prove, extremely valuable when used freely and fearlessly, but in the extremities I must say it always disappointed me, for with us the cellular substance was almost invariably disorganised, and out it must come sooner or later.

The French Method of Mercurial Inunction.

This we tried in some cases, and I am disposed to think highly of it when judiciously employed; but my method was to apply it thickly, spread on linen, enveloping the whole of the erysipelatous limb, and renewing it every 24 hours. In one severe case of the upper, and one of the lower, extremities, it was decidedly beneficial, but I should be disposed to place reliance on it more where the disease is *cutaneous* than where affecting the cellular tissue. If it prove useful, you will, in about 48 hours after its application, find the limb becoming soft, and the skin corrugating, with desquamation following in its train. During its application, purgatives, with salines and antimonials, must not be omitted. Indeed the bowels should always be kept in action until the disease has reached its crisis.

As regards the general treatment of the disease in the *Prince Regent*, it was active in the highest degree while we remained in the Channel and off Scilly. Sailors full of beef and grog are not to be treated like old ladies or young misses, with potions and diaphoretics. Venesection, frequently repeated, free purging (and here a large dose of calomel must always have precedence), salines with antimonials, cold lotions and *free* incisions (when called for by the tension of the parts or the state of the sensorium), were all had recourse to with the best success; for although many were a good deal crippled by contractions, the result of the necessary free incisions made on the extremities attacked by the disease, the mortality only reached one in every fifteen cases.

But this ratio was soon to be frightfully increased. On the 10th of September, 1831, we sailed from Spithead, bearing the flag of Rear-admiral William Parker, C.B., for the river Tagus, in company with the *Asia* 84. At this time we had not a single case of erysipelas on board, nor had there been one for several days, and it was hoped we had got rid of the disease. A large quantity of chloride of lime, supplied by order of Sir William Burnett, had been expended in purifying the ship from "stem to stern," and from the hold to the upper deck. However, when we got to the southward, past Cape Finisterre and on the coast of Portugal, with warm weather, the disease reappeared with more virulence than ever; the premonitory fever assumed a typhoid type (it had hitherto been inflammatory), and patients were brought forward to the "Sick Bay," staggering, and with drunken countenances. On inquiry, perhaps you would find that they had been ailing for a day or so, but had not applied for medical aid, and on stripping them to put them in a sick cot, you would find erysipelas on some part of the body, arising from the slight bruise of a rope across the legs, or from the ratlines of the rigging in going aloft. In two or three of these cases the prostration was so great that nothing could rally them, and they died almost before you could look around.

We now lost one in every four cases, and the men became as it were panic-struck,—every bruise and every wound, however slight, taking on the erysipelatous inflammation, and some of them proving rapidly fatal.

In this dilemma Admiral Parker—whose solicitude for and anxiety to do anything and everything that could alleviate the sufferings or add to the comfort of the sick, is beyond all praise of mine—at my suggestion and recommendation, backed by the medical officers of the squadron, looked out for a ship that could be appropriately fitted up as an hospital ship, and the *St. Patrick* steamer being unemployed in the Tagus, was engaged for that purpose at a very trifling expense, her spacious cabins forming excellent accommodations for the sick. To this vessel, then, all the erysipelatous cases were removed, and every case occurring afterwards was always instantly sent to the same place. This effectually checked the disease, and rendered the cases controllable, for in a few hours after being removed to the St. Patrick, a decided change for the better took place. Indeed so obnoxious had the air of the ship become, that neither wound nor sore, however slight in itself, however healthy the subject of it, would heal on board, but were all obliged to be sent to the hospital ship, where they healed kindly and readily, with the most simple dressings. For some months we had no instance of a wound healing by the first intention on board the *Prince Regent.*

In January 1832, we were ordered home, and on the 24th February following were paid off at Portsmouth, the disease clinging to us to the very last, a marine being attacked by it on his march to Chatham after disembarkation, and a seaman being sent to Hasler Hospital not many hours before the ship was paid off.

P.S. I understand, that on the *Prince Regent* being cleared at Portsmouth when in ordinary, an amazing quantity of foul black mud was found in her hold, which probably accounts for all our troubles.

Sept. 1st, 1832.

REMARKS ON

BERIBERI,

WITH CASES.

By John Whitehead Macnee, Esq.,
Surgeon, Glasgow,
Member of the Med. and Phys. Society of Calcutta.

This disease is, in a great measure, peculiar to the island of Ceylon, and the Malabar coast. It has likewise been observed in the neighbourhood of Madras; and I have heard it asserted, that it has been seen in Bengal; but of this, I believe, we have hitherto had no certain information. It is most prevalent during the change of the monsoon, and has been generally supposed to arise from checked perspiration. In warm climates both soldiers and seamen are in the habit of exposing themselves during the night, while asleep, without the protection of any covering, to the comparatively chilly atmosphere, after having been bathed in perspiration during the greater part of the preceding day. I had an opportunity in August 1830, while near to Ceylon, of seeing two cases of the disease, and they were both attributable to this cause. The men were likewise of weak and debilitated habits of body—one of them much addicted to the use of intoxicating liquors, which propensity his employment, as one of the cuddy servants, afforded him better opportunities of indulging than either a sailor or soldier while subject to the rigid discipline of a well-regulated ship.

To Mr. Hamilton we are indebted for much of the little we know of this singular disease. Till his paper was published, I am not aware that we were in possession of any accurate knowledge of its pathology, if we except the unconnected, and, in many respects, unsatisfactory accounts contained in the work of Mr. William Hunter, on the diseases of Indian seamen. Till lately, it was generally treated as a disease of debility; but the result of Mr. Hamilton's dissections will amply satisfy any one of the treatment that should be pursued. Had I been so fortunate as to have seen this paper before the two cases I am about to relate came under my observation, blood-letting should have been employed to a much greater extent than I at that time deemed it prudent to venture upon.

That the disease arises from checked perspiration admits of but little doubt; but, in addition to exposure, Mr. Hamilton deems previous residence in India necessary; and goes as far so to say, that he knows of no instance on record of an individual being attacked immediately on his arrival in India. There may very possibly be no such case published, but it so happened, that one of the individuals who came under my care, had never before been to the eastward of the Cape. But even although we must allow sudden chill to be the immediate exciting cause of the complaint, it is still in many respects but a very imperfect explanation. For, were we disposed to admit the justness of this conclusion, there is no reason why the disease should be confined to Ceylon and its immediate vicinity. The disease, however, has scarcely, if ever, been seen elsewhere, from which it is evident enough, that there must be some peculiar modifying influence in the atmosphere, or it would not, in a manner, be limited to one particular situation.

One of the cases I saw was much more distinctly marked than the other; for, it should be remembered, that beriberi frequently attacks those who are already suffering from, or it combines and partly identifies itself with, other diseases. In its more advanced stages, it often terminates in hydrothorax or paralysis. In my second patient confirmed hemiplegia took place—the first was what we may term one of uncombined beriberi, that is, with symptoms referable to no other complaint.

CASE I.

William Firs, aged 28, was attacked on the afternoon of the 25th August 1830, the ship being at the time in 5 30 south latitude and 81 35 east longitude. Had during the greater part of the voyage been in the habit of sleeping on deck, without any covering; was of a pale, heavy, and unhealthy appearance, much addicted to drinking, and had twice suffered severely from scurvy. His appetite was bad. About three in the afternoon, without any assignable cause, he began to feel faint and languid, with disinclination to any exertion. In this state he had some brandy-and-water, which only afforded him temporary relief; for in about an hour afterwards, a thrilling pain was felt extending over the extremities, which in a short time began to swell. The intumescence extended by degrees to the abdomen, and by five o'clock the whole integuments were tense, and pitted on pressure. Severe dyspnœa was experienced, with great anxiety of countenance. The pulse was small and frequent. The bowels had been opened in the morning.

Hot fomentations were immediately applied to the abdomen, thirty drops of laudanum administered internally in some hot brandy-and-water, and ten grains of calomel, with ten of compound ipecacuan.

By six o'clock, the difficulty of breathing had increased—the integuments of the thorax were swollen and œdematous; the pulse was small and rapid, with very harassing cough. Some pain was now experienced in the abdomen. The integuments throughout were by no means rigid, but readily yielded to, and for some time afterwards retained the impression of the fingers.

The fomentations were continued; and about nine the calomel and ipecacuan, were repeated. Hot brandy-and-water, with a few drops of the tincture of opium, was given occasionally. Under this treatment copious perspiration took place; and a little after midnight the swelling began to decrease gradually. It first disappeared from the chest, then from the abdomen, and finally, but more slowly, from the extremities. During the night several profuse evacuations of urine took place.

By noon of the following day scarcely any swelling remained. He was able, although much exhausted, to walk about, and three days afterwards returned to his duty.

CASE II.

P. P., aged 27, was seized on the morning of the 27th of August. At nine o'clock I found this man pale and stupid, with great anxiety of countenance, and suffering severely from dyspnœa; his face was swollen; he felt chilly, and complained of nervous tremours in the extremities; on examining which I found them tumid and pitting on pressure. The abdomen was at this time by no means enlarged. He had cough and slight nausea; the pulse was small and 96; bowels rather costive.

Sixteen ounces of blood were immediately taken from the arm, and were neither buffed nor cupped. During the day he had a calomel purge, which acted freely, and two pills, each composed of three grains of calomel and one of squills.

On the following morning the swelling of the extremities had not increased, but the countenance was unusually vacant, and somewhat flushed. The irides were natural, and his only complaint was referred to the kidneys; from which, he said, there was a dull pain extending along the course of each spermatic chord to the testicle. No urine had been passed for upwards of twelve hours. The catheter was, therefore, immediately introduced, and the bladder found empty. Pulse 110; the bowels had been freely opened.

An active purge of calomel and jalap was immediately given, and a blister applied to the lower part of the back. The pills of calomel and squills were continued as before, with a diaphoretic draught of liquor ammoniæ acitatis.

At about six in the evening he passed some urine without any assistance, but in other respects remained in nearly the same condition. On the morning of the 30th, numbness with some spasmodic twitches were experienced, extending along the left arm. The pulse was feeble and 115. A blister was applied to the neck and back of the head; but in the evening, the entire left side was found to be powerless. He was now treated for paralysis, but unsuccessfully; and on the 6th October was sent to the general hospital at Calcutta, but returned to the ship on the 14th December in no respect improved. He was, I believe, on the 6th May following sent on board the Grampus hospital ship at Greenwich.

The symptoms in this case bore from the first some resemblance to those of paralysis, and it was only after repeated and careful examination that I was induced to pronounce it the disease in question. It seems a matter of some doubt, from the complicated nature of beriberi, whether it ever is, what may be styled, an idiopathic disease; and I need scarcely add, that the case I have just related would go far to support such an opinion. Latterly it terminated in hemiplegia, but in the first instance the anasarcous state of the extremities, and the difficulty of breathing, left little doubt as to the nature of the disease.

Centre-street, Glasgow, Sept. 3, 1832.

DR. CONQUEST'S

HYDROCEPHALIC OPERATIONS.

To the Editor of The Lancet.

Sir,—Knowing your readiness at all times to give publicity to any case that may afford interest to the profession, or reflect due honour upon any one of its members, I am induced to send you a case of hydrocephalus, twice operated upon two years ago by Dr. Conquest, but which, from a desire of ascertaining the consequent results, was not then presented for publication.

The child, William Wilmer, aged four months, came under my care, at the City Dispensary, in the month of July 1830. His head was of an enormous size, and had been so from his birth; the forehead was large and prominent; the eyes heavy, and somewhat convulsed; frequent hiccup, vomiting, &c.

Several gentlemen of the profession had seen the case, and they all gave it up as hopeless. Believing that no other chance remained for saving the child but an operation, I recommended the parents to Dr. Conquest, who had about that time operated upon one or two of a similar nature.

In the beginning of August, Dr. Conquest performed the operation upon this child, by introducing a small trocar into the right anterior fontanel, directed towards the ventricle to the depth of an inch or more. Immediately the fluid issued forth in a stream, at first clear, and afterwards a little tinged with blood. The pulse becoming weak, the canula was withdrawn. The cutis, frontal, and parietal bones, being now remarkably loose, they were supported by myself and others, until slips of adhesive plaster were applied in different directions round the head, in order to produce uniform pressure, while a little weak brandy-and-water was administered with advantage to the patient. The fluid extracted measured twelve ounces.

During the remainder of the day the child continued rather weakly, but was more lively than he had ever previously been, and for some time afterwards the intensity of all the former symptoms greatly diminished.

When a month, however, had nearly elapsed, it was considered requisite to repeat the operation, and on the 3rd of September, Dr. Conquest introduced the instrument as before, and again extracted a clear liquid to the amount of twelve ounces more.

Dr. B. G. Babington had the goodness to analyse this fluid, and stated the results as follows:—"It resembles," he says, "in its general characters, all the specimens of fluid from the ventricles of the brain which I have examined. Its specific gravity is 1004. It does not coagulate, or, in the least degree, lose its transparency by the application of heat, or by the addition of acids or alcohol; consequently it does not contain any albumen. Tinct. of galls scarcely produces any precipitate, it therefore is nearly free from gelatine. It is neither acid nor alkaline in the least degree. Its taste is very saline, and it furnishes a copious precipitate with the nitr. of silver. I have no doubt, therefore, that two muriates predominate, and are the cause of its taste. I consider it a favourable circumstance as regards the case, that its sp. gr. is so low, and that it contains no albumen, as I have always found that there is a deposition of fibrin somewhere, to correspond with the quantity of albumen contained in the effused serous fluids. Had there been present albumen in this instance, I should have expected a deposition of a gelatinous consistence, which is fibrin in a divided and less concentrated form, at the basis of the brain."

The results of this operation seem to coincide with Dr. Babington's favourable opinion of the case, as expressed in his analysis. But still the head continues remarkably large, measuring from back to front 24½ inches, and from the one meatus over the head to the other, 16½ inches, being about the same dimensions as when first operated upon. However, while the head has thus remained almost stationary, the size and strength of the body have gradually increased in proportion to its age. With the exception of the extraordinary size of the cranium, the hydrocephalic symptoms appear to have vanished. The child sleeps well, eats heartily, is very lively, and is in the full enjoyment of all its mental faculties.

I am, Sir, your obedient servant,
H. S. Caldwell, M.D.
Camberwell, Sept. 6, 1832.

STATEMENT OF FACTS RELATING TO A

STROKE OF LIGHTNING,

Which happened on the 13th of April, 1832.

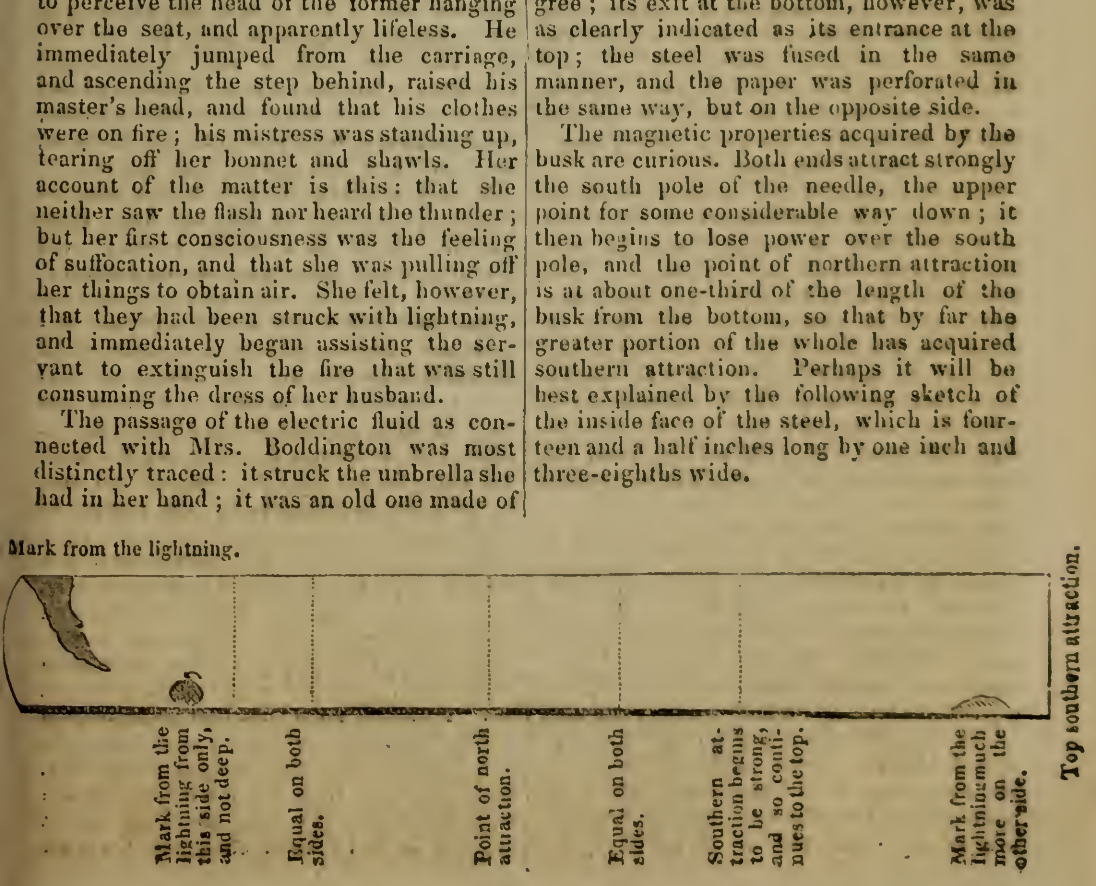
By Benjamin Boddington, *Esq.*

Epitomised from the "London and Edinburgh Philosophical Magazine." Communicated by Dr. Faraday.

On Friday, the 13th April 1832, Mr. and Mrs. T. T. Boddington, having partaken of some refreshment at Tenbury, placed the servants inside their post chariot, and themselves mounted the barouch-seat behind, that they might enjoy the scenery on the road to Bromyard through the ramifications of the Abberley hills. It was about half-past three when they left, the sun shining and the sky serene; but before they had proceeded far, they observed a dark and singular-looking cloud to arise, nearly in the direction of their route; and at the end of about three miles and a half a few drops of rain began to fall. They debated whether they should get inside the carriage, but agreed that the storm, for such it appeared to be, was passing off to the right, and that it would in all probability be only a slight shower, as the cloud in their immediate vicinity, though peculiarly dark and angry-looking, was of very small dimensions. At this time a clap of distant thunder was heard, but no lightning seen. Mr. Boddington put up an umbrella; but perceiving it was an old one, somewhat torn, belonging to one of the servants, he gave it to his wife to hold over her bonnet, while he put up another. While in the act of extending the latter, a flash of lightning struck them both senseless, threw the horses on the ground, and cast the post-boy to a considerable distance. The servants inside were untouched, and, indeed, unconscious of the real nature of the accident. The man says that he heard no previous thunder, but that a vivid flash of lightning, proceeding as he thought from the side of the road next to which he sat, was accompanied by an instantaneous report, like the discharge of a highly-loaded blunderbuss; and concluded that some robber or other mischievous person had shot the horses. He acknowledges, that he was so panic-struck, that for a few seconds he sat still; but on recovering from the momentary alarm, he let down the side glass and looked out to see whether his master and mistress were safe. He was shocked to perceive the head of the former hanging over the seat, and apparently lifeless. He immediately jumped from the carriage, and ascending the step behind, raised his master's head, and found that his clothes were on fire; his mistress was standing up, tearing off her bonnet and shawls. Her account of the matter is this: that she neither saw the flash nor heard the thunder; but her first consciousness was the feeling of suffocation, and that she was pulling off her things to obtain air. She felt, however, that they had been struck with lightning, and immediately began assisting the servant to extinguish the fire that was still consuming the dress of her husband.

The passage of the electric fluid as connected with Mrs. Boddington was most distinctly traced: it struck the umbrella she had in her hand; it was an old one made of cotton, and had lost the ferule that is usually placed at the end of the stick, so that there was no point to attract the spark. It was literally shivered to pieces, both the springs in the handle forced out, the wires that extended the whalebones broken, and the cotton covering rent into a thousand shreds. From the wires of the umbrella the fluid passed to the wire that was attached to the edge of her bonnet, the cotton thread that was twisted round that wire marking the place of entrance over the left eye, by its being burnt off from that spot all round the right side, crossing the back of the head and down into the neck above the left shoulder. The hair that came in contact with it was also singed: it here made a hole through the handkerchief that was round the throat, and zigzagged along the skin of the neck to the steel busk of her stays, leaving a painful but not deep wound, and also affecting the hearing of the left ear. It entered the external surface of the busk. This was clearly proved by the brown paper case in which it was inclosed being perforated on the outside, and the busk itself fused for about a quarter of an inch on the upper surface, presenting a blistered appearance. Its passage down the busk could not be traced in any way; there was no mark whatever on the steel, nor was the paper that covered it discoloured or altered in the slightest degree; its exit at the bottom, however, was as clearly indicated as its entrance at the top; the steel was fused in the same manner, and the paper was perforated in the same way, but on the opposite side.

The magnetic properties acquired by the busk are curious. Both ends attract strongly the south pole of the needle, the upper point for some considerable way down; it then begins to lose power over the south pole, and the point of northern attraction is at about one-third of the length of the busk from the bottom, so that by far the greater portion of the whole has acquired southern attraction. Perhaps it will be best explained by the following sketch of the inside face of the steel, which is fourteen and a half inches long by one inch and three-eighths wide.


Mark from the lightning.
Mark from the lightning from this side only, and not deep.
Equal on both sides.
Point of north attraction.
Equal on both sides.
Southern attraction begins to be strong, and so continues to the top.
Mark from the lightning much more on the other side.
Top southern attraction.

There were marks of burning on the gown and petticoat above the steel, and the inside of the stays, and all the garments under the stays were pierced by the passage of the fluid to her thighs, where it made wounds on both; but that on the left so deep and so near the femoral artery, that the astonishment is that she escaped with life; even as it was the hemorrhage was very great. Every article on which she sat was perforated to the cushion of the seat, the cloth of which was torn much more extensively than the clothes. In most cases they were pierced by a hole not exceeding the size of half an inch in diameter, and where the rents were larger they did not extend beyond an inch or two in any direction; but it is worthy of observation, that every article the electric fluid passed through had a singed appearance at the edges, and had a sulphurous smell, as I was informed by those who inspected them immediately after the accident. By the time I reached Tenbury, all trace of the smell had vanished. No ignition, however, took place beyond what occurred at the moment of its passage, notwithstanding the inflammable nature of most of the articles; nor did any of Mr. Boddington's wounds present the appearance of burns. The cushion of the barouch-seat was stuffed with curled horse-hair, through which the stream must have passed, though no sign to indicate its passage was visible; the cloth edge of the cushion, however, immediately behind where Mrs. Boddington sat, was torn outwards, and the leather that covered the iron forced in the same spot, already marking its egress at this place.

As this same iron also received the charge that struck Mr. Boddington, I shall now state the effects of the lightning on him, before I trace its further progress. When first discovered by his servant he was insensible, and he remained in that state for about ten minutes, when he recovered sufficiently to inquire where he was, but relates that he was perfectly unconscious of what had occurred; that he felt his eyesight affected, and pain all over him, but knew not from what cause these sensations arose. The umbrella in this case also was the conductor; it was made of silk, and was but little damaged, a small portion of the upper part only being torn where it joins the stick, and none of the springs or wires being displaced. The main force of the shock, however, appears to have passed down the handle to his left arm, though a portion of it made a hole through the brim of his hat, and burnt off all the hair that was below it, together with the eyebrows and eyelashes. The fragments of the burnt parts falling into the eyes nearly deprived him of sight for two or three days, but the eyes were not otherwise injured. The electric stream shattered the left-hand, fused the gold shirt-buttons, and tore the clothes in a most extraordinary manner, forcing parts of them, together with the buttons, to a considerable distance, and a deep wound was inflicted under its position on the wrist. The arm was laid bare to the elbow, which is presumed to have been at the moment very near his left waistcoat pocket, in which there was a knife; this also was forced from its situation and found on the ground; a severe wound was made on his body, and every article of dress torn away, as if it had been done by gunpowder. From the knife it passed to the iron of the seat, wounding his back, and setting fire to his clothes in its passage. Another portion descended to the right arm, which had hold of the lower part of the stick of the umbrella, was attracted by the sleeve-button, where it made a wound, but slight as compared with that on the left, passed down the arm (which it merely discoloured, and broke the skin off in two or three places) to a gold pencil-case in the right waistcoat-pocket. The great-coat he had on was an old navy watch-coat, commonly called a *pea-jacket*, and of great thickness; this was torn to pieces, and the coat immediately above the waistcoat-pocket much rent, but the waistcoat itself was merely perforated on the external part, where the discharge entered by a hole about the size of a pea, and on the inside by a similar hole at the other extremity of the pencil-case, where it passed out, setting fire to his trowsers and drawers, and inflicting a deep wound round his back, the whole of which was literally flayed.

A very striking difference was observable in the wounds of Mr. and Mrs. Boddington: hers, as I before stated, were fractures of the flesh; his, on the contrary, whether deep or shallow, were all of them burns, and had a white and blistered appearance. The accumulation of force which the electricity acquired at this place deserves particular attention. I have observed that the shock on the right arm was nothing as compared with that on the left; the shirt-button was unchanged and unmoved from its position, and the passage of the fluid down the arm barely indicated; yet when it arrived at the pencil-case, the amount of its intensity was such as to melt one end of it, and displace a cornelian seal at the other extremity, forcing it, I suppose, to some distance, as it has never since been found, though it was carefully sought for. It should seem that this accumulation of strength must have been derived either from the portion that passed over Mrs. Boddington, or from union with that which went down the left arm. In either case it appears to have been strangely diverted from its original source. The whole shock was now collected in the iron that formed the back of the barouch-seat; the leather attached to it was torn off, and the iron itself broken in two immediately opposite the spring, and the ends of the fractured parts bent forwards so as nearly to touch it; by this conveyance it is supposed to have diffused itself over the whole of the under carriage, and to have passed to the earth by the tires of the wheels, four holes being made in the road at the points they touched at the moment of the shock, though the carriage was not standing in them at the time it stopped.

The post-chariot was a new one, and the only injury it received was the fracture and derangement of the barouch-seat, as already stated, the removal of the japan in a line along the bulge behind, and the breaking of the pole; the latter circumstance I conceive to have arisen solely from the fall of the horses, and to have been quite independent of the passage of the electric fluid. The horse the postilion rode was found to be dead, the other was evidently panic-struck but unhurt, as he rose as soon as the harness was cleared from him, and though in a profuse sweat and trembling, he soon recovered; and not only was rode back for assistance, but returned again in the chaise that conveyed the poor sufferers to Tenbury, where they were detained at the inn for a month, before it was thought safe to remove them.

On inspecting the dead horse no wound was visible, nor any apparent cause for his death; the brass front of the bridle was observed to be indented inwards, as if struck with a hammer; and when he was skinned, a corresponding mark was found on the bone of the head; and from that spot to the termination of the spine, the flesh was quite black and putrid for about the width of three inches, and there were diverging marks of the same nature on each side of the head passing under the throat, and similar but much wider ones on the flanks. The post-boy was thrown some yards off, but this I conceive to have been by the spring of the horse when he was struck dead, and that spring doubtless jerked the carriage beyond the holes where the lightning had passed into the earth. The boy was shaken by his fall, but was in other respects unhurt. I inspected the spot nearly three weeks after the accident happened, found it was elevated ground, but by no means the summit of the surrounding country; on the contrary, there were many higher hills in the neighbourhood; the road itself was so much hollowed out, that the banks must have been nearly equal to the height of the carriage. In a fen to the right, within a few yards of the hedge, and exactly opposite to where the shock took place, was a very high pear-tree, it however bore no trace of injury. The carriage appears to have been passing close to that side of the bank, as the holes to which I before alluded were still distinctly visible; indeed, the two to the right had undergone very little change, as they were nearly off the road; they were about fifteen inches in diameter, perfectly round, and nearly as deep as they were wide, the stones appearing to have been thrown out as if by a miner's blast.

The collateral facts must now be mentioned. The landlord of the inn at Tenbury informed me that he was sitting in his parlour, talking to another person, when he saw the flash of lightning that must have caused the accident; he observed to his companion that he had never before seen so singular a flash, as it appeared to divide into four parts when it came within about thirty yards of the earth; this statement was confirmed by the person who was with him. It should seem, therefore, they were not struck by a single discharge of electric matter, but were enveloped in a mass of electricity; and this is the more probable, from the traces of the different strokes being so distinct, and yet taking such opposite directions. The fluid seems to have pervaded the whole atmosphere, as many things were magnetised that were not in the line of any of the tracks that could be traced. For instance, Mr. Boddington's watch was in his fob, and quite out of the line described by either of the shocks that passed over him. After the accident, it was found necessary to send it to a watchmaker, and when taken to pieces, parts of it were dicovered to be highly magnetised, the balance-wheel in particular. This was shown to Dr. Faraday when at Oxford, who set it afloat on a cork, and found the poles to be so well defined, that I have since had it mounted as a compass. Two pair of scissors also in Mrs. Boddington's work-box, inside the carriage, were by mere accident, two months after the event, discovered to be magnetic.

I certainly now very much regret that more minute researches were not made at the time as to these facts; but whoever has watched over the sick-bed of a beloved son with but faint hopes of his recovery, will not be surprised that philosophical investigations were all absorbed in the deeper interest of the affections.

Badger Hall, July 16, 1832.

HYDROSTATIC BED FOR PATIENTS AND INVALIDS.

(*Abridged from the 5th Edition of Dr. Arnott's "Elements of Physics," now in the Press.*)

It is to mitigate all, and entirely to prevent some, of the evils attendant on the necessity of remaining in a reclining posture, that the hydrostatic bed described in the following account is intended. It was first used in the case of a lady, who, after her confinement, passed through a combination of low fever, jaundice, and slight phlegmasia dolens of one leg. She rested too long in one posture, and the parts of the body on which she had rested all suffered: a slough formed on the sacrum; another on the heel; and in the left hip inflammation began, which terminated in abscess. These evils occurred while she was using means to invigorate the circulation, and while her case was watched with affectionate assiduity. She was placed upon the bed contrived for invalids by Mr. Earle, with pillows of down and air, and out of the mattress of which portions were cut opposite to the sloughing parts. In spite, however, of all endeavours, the mischief advanced, the chief slough enlarged, another slough and a new abscess were produced, and her life was in imminent danger.

Under these circumstances, the idea of the hydrostatic bed occurred to me. Even the pressure of an air pillow had killed her flesh, and it was evident that persons in such a condition could not be saved unless they could be supported without sensible inequality of pressure. I then reflected, that the support of water to a floating body is so uniformly diffused, that every thousandth of an inch of the inferior surface has, as it were, its own separate liquid pillar, and no one part bears the load of its neighbour, and that the patient might be laid upon the surface of a bath over which a large sheet of the water-proof India-rubber cloth was previously thrown, she being rendered sufficiently buoyant by a soft mattress placed beneath her. Such a bed was immediately made. A long trough, a foot deep, was lined with metal, to make it water-tight; it was about half filled with water, and over it was thrown a sheet of the India-rubber cloth. Of this sheet the edges (touched with varnish to prevent the water creeping round by capillary attraction) were aftewards secured in a water-tight manner all round to the top of the trough, the only entrance left being through an opening at one corner, which could be perfectly closed. Upon this dry sheet a suitable mattress was laid, constituting a bed ready to receive its pillow and bedclothes, and not distinguishable from a common bed but by its surpassing softness. The patient was laid upon it; she was instantly relieved; sweet sleep came to her; she awoke refreshed; she passed the next night much better than usual; and on the following day all the sores had assumed a healthy appearance; the healing from that time went on rapidly, and no new sloughs were formed. The down pillows were needed no more.

In choosing the mattress, if unusual positions are required, by having different thicknesses in different parts, or by placing a bulk of folded blanket or of pillow over or under the mattress in certain situations, any desired position of the body may be easily obtained. If the water be about six inches deep, which in general will suffice, the person standing upon any part of the bed, or sitting with the knees raised, will cause the part of the mattress on which he rests, gently to touch the bottom. On lying down, he as completely floats as if the Atlantic were under him.

This bed is a warm bed, owing to water being nearly an absolute non-conductor of heat from above downwards, and owing to its allowing no passage of cold air from below. From this last-mentioned fact, however, less of the perspiration, sensible and insensible, will be carried off by the air than in a common bed, and unless the patient can rise or be lifted daily, to allow the bed to be aired like a common bed, there will be a necessity for using some such means as the following, to prevent the condensation of perspiration on the water-sheet below—an oiled silk laid over the mattress, or a blanket, to be occasionally changed, laid under it, or a set of flexible tubes of spiral wire laid under it, with their ends open to the atmosphere, to ensure a constant ventilation of the mattress; or, similarly placed, and producing the same effect, a layer of cork cut into square pieces, with spaces left between them, to serve as conduits of air. This bed is in itself as dry as a bed can be, for the India-rubber cloth (of which bottles can be made) is quite impermeable to water, and the maker is now preparing cloth expressly for this purpose. Unlike any other bed, it allows the patient to change his position almost like a person swimming, and so affording the kind of relief which, in constrained positions, is obtained by occasional stretching. It exceedingly facilitates turning for the purpose of dressing wounds, for by raising one side of the mattress or depressing the other, or merely by the patient's extending a limb to one side, he is gently rolled over, nearly as if he were simply suspended in water; and it is possible even to dress wounds, apply poultices, or place vessels under any part of the body, without moving the body at all; for there are some inches of yielding water under the body, and the elastic mattress may at any part be pushed down, leaving a vacant space there, without the support being lessened for the other parts. It may be made so cheaply, that even in hospitals, where economy must prevail, it may at once be adopted for many of the bed-ridden. The bed has been introduced into St. Bartholomew's and St. George's Hospitals, and elsewhere. With it sloughing need never occur again, and by alleviating distress through the earlier stages of disease, it may prevent many cases from even reaching a degree of danger. It is peculiarly applicable to cases of fractured bones, and other surgical injuries; to palsies, diseases of the hip-joint, and spine; and universally where persons are obliged to pass much time in bed. Also in all cases of curvature of the spine, existing or threatened. If used without the mattress, it becomes a warm or a cold bath, not allowing the body however to be touched by the water; and in India it might be made a cool bed for persons sick or sound, during the heats which there prevent sleep and endanger health. There are numerous other modifications of it, which will readily occur to practitioners sufficiently versed in hydrostatics. Before reflection a person might suppose a resemblance between it and an air-bed or pillow, but the principles of the two are perfectly distinct. An air pillow supports by the tension of the surface which encloses the air, and really is a hard pillow; but in the hydrostatic bed, there is no tense surface at all: the patient is floating upon the water, on which a loose sheet is lying, merely to keep the mattress dry, and every point of his body is supported by the water immediately beneath it. The bed is better described by the appellation of *hydrostatic-bed* than of *water-bed*.

The author has given no exclusive right to any person to make this bed. He has hitherto employed the carpenter nearest to his own residence in Bedford Square, and the London manufacturers of the waterproof cloth; but any carpenter may learn to supply them, and he gives free permission to all.

At present the medical attendant generally leaves whatever regards the bed to the judgment of friends or nurses; but he who has been led to reflect how much the course of a malady may depend on the patient's being so supported that no pain shall arise from local pressure, and little muscular weariness, will deem the bed-management worthy of his own attention.

MALIGNANT CHOLERA,

TREATED WITH

COMPOUND INFUSION OF HORSERADISH.

To the Editor of The Lancet.

Sir,—Although pressed hard by time, I cannot allow another moment to slip without giving you the information I obtained in a conversation with Dr. Stevens of Ely, relative to his *latter* treatment of cholera. Knowing the pages of your journal are ever *filled* with matter of intrinsic value, I will speak, for the present, of his plan as concisely as possible, as I may hereafter have to request your indulgence in some further communication upon the subject of cholera; till then I shall defer all remarks.

Dr. Stevens states, that out of 32 cases of cholera in its most malignant forms, four only died; one of these last refused all medicine, and another was actually dead when the Doctor arrived at the poor fellow's home; twenty were in the stage of collapse, vomiting and dejecting characteristic matter; the whole body and tongue and breath were as cold as ice; extremities shrivelled, and in some as if dyed in indigo; some suffered from violent cramps, in others it was absent; eight, though in the state of asphyxia, could hardly be allowed to have advanced so near death. In all these cases, after an injection of brandy and opium, and a bolus of calomel and opium, he commences with two drachms of spiritus armoraciæ compositus, repeating in half an hour or an hour, as his judgment might lead him. The effect of this medicine he expresses as most wonderful after a dose or two; the patient slumbers profoundly, he becomes bedewed in a profuse hot transpiration, wakes as from a long and comfortable sleep, the nervous energy is restored, and secretion has again commenced; in a short time urine in a large quantity is evacuated, the liver also has not been idle, for a dose of rhubarb will bring away a well-tinged bilious dejection; the patient is recovered as if by magic; his debility lasts but a short time; ℈j of carbonate of soda three times a day, restores him to a state of perfect health.

The Doctor remarks, that could the stomach retain the quantity, he should prefer the infusum armoracia compositum, on account of the mustard seed it contains.

I remain, Sir, your obedient servant,

G. F. Le Neve,
Surgeon.

Barrow, near Bury St. Edmunds,
Sept. 5th, 1832.

TRIAL OF
SALINE VENOUS INJECTIONS
IN
MALIGNANT CHOLERA
AT THE DRUMMOND-STREET HOSPITAL, EDINBURGH.

(Communicated by the Author.)

To the Secretary of the Medical Board, Madras.

Sir,—In my last communication for the information of the Board on the subject of injecting saline fluids into the circulation during the stage of collapse in cholera, for which purpose I forwarded an improved apparatus, I mentioned the extraordinary effect it had in rousing the whole system, and acting upon the patient almost as a charm. I may now state that Dr. Mackintosh and myself have for a considerable time given the practice a fair trial at the Drummond-Street Hospital in this city, and have apparently snatched many, as it were, from the very jaws of death.

The very remarkable effects of this remedy require to be witnessed to be believed. Shortly after the commencement of the injection the pulse, which was not perceptible, gradually returns; the eyes, which were sunk and turned upwards, are suddenly brought forward, and the patient looks round as if in health; the natural heat of the body is gradually restored; the tongue and breath, which were in some cases at the temperature of 79° and 80°, rise to 88° and 90°, and soon become natural; the laborious respiration and oppression of weight at the chest are relieved; the sickness, thirst, and inclination to vomit, cease; the deafness vanishes, the obscured vision becomes clear, and the whole countenance assumes a natural, healthy appearance; the tongue, which the patient sometimes describes as having fallen back into the throat, resumes its proper place, and the vox cholerica is heard no more. The whole feelings of the patient become for a time natural, they express themselves as being perfectly easy, and feel as if in a new world, and their spirits are exhilarated, and they converse familiarly upon all matters connected with themselves or their families.

Unluckily, this favourable state does not remain long, and very often within three or four short hours, we find them sunk again into a state of collapse or bordering upon it, and the same operation is to be gone through, with perhaps less success. I regret to say, that latterly I have tried it upon many without the least benefit, except, perhaps, a slight temporary change at the commencement of the operation. I have selected for the Board a case from my journal, in which 612 ounces of saline fluid were injected, and the patient is up to the present time continuing well. This case shows that the practice is not only perfectly safe, but that a great quantity can be thrown into the circulation in a very short time; it also shows how we must persevere in some cases. Had the injection not been repeated a fourth time, the poor woman would to all appearance have sunk within an hour, and after four days it seems that the repetition of the injection alone saved her. During the first fourteen hours after she was brought into the hospital, there were 474 ounces of fluid injected. She was in a complete state of collapse for hours before admission. The litter was sent for her a little after six o'clock p.m., when the porters had carried her partly down-stairs she was seized by the mob, and taken back to her miserable habitation; at half-past seven o'clock Dr. Mackintosh, Dr. Racey, and myself, visited her, when the mob again interfered, and obliged us to make rather a hasty retreat down-stairs, so that another hour and a half elapsed before she was brought to the hospital.

The whole of the animal functions depend so very essentially upon the blood, that the introducing into the circulation, either directly or indirectly, a quantity of fluid resembling the serum which has been abstracted from it by the copious dejections of some of the secretions, is certainly a most rational mode of treating cholera, and being able to produce such rapid changes in the whole system, is very much in favour of the practice. The operation of injecting requires the greatest care and attention, and the patient ought never to be left for half an hour even by the medical attendants afterwards. It brings a patient almost invariably out of the state of collapse, and gives you time to employ other remedies. The effect not being permanent, may arise from the patient labouring under some organic disease at the time of the cholera accession. That the consecutive fever is aggravated by injecting, is to me doubtful. So few cases having recovered in this city from a complete state of collapse by any other means, it is impossible, with our limited knowledge, to do more than hazard an opinion. It must be borne in mind, that we have only made use of this remedy in the most desperate cases. Cholera has scarcely been here as an epidemic, and with the exception of a few sporadic cases, it has been almost confined to the debilitated, aged, intemperate, ill-fed, and the most miserable portion of the community; more particularly to those labouring under some chronic affection. Some few families have been nearly all swept away, who did not come under any of the above denominations. Dr. Mackintosh and myself will shortly publish the result of our practice in the Drummond-Street Hospital.

The disease appears to be different every month, and we know very little more of the curative process now than we did many years ago. For my own part I confess, that after years of attentive observation of the symptoms, and careful watching of the practice of others, as well as my own, I am as far from having a proper knowledge of the disease, as when I was first seized with it on returning from Hindostan after the Mahratta campaign in 1818. I have, however, been collecting facts and making observations upon them, which may ultimately lead to something beneficial. My next communication will, I trust, be more worthy of the acceptance of the Board. I have the honour to be, Sir,

Your most obedient servant,
George Meikle,
Surgeon E. I. C. S. Madras Estab.
39, Albany Street, Edinburgh,
Aug. 3d, 1832.

CASE.

Grace Lindsay, ætat. 38, a washerwoman, residing in Cox's Land, St. Mary's Wynd, was admitted into the hospital, May 28th, 1832. Has been subject to disorder of the stomach and bowels for several years. Two days since she was attacked with diarrhœa, which continued until twelve (noon) to-day, when the characteristic vomiting and purging commenced, followed by cramps, not at first very severe. At half-past seven p.m. she was visited by Drs. Mackintosh, Racey, and Meikle. The cramps were then very violent, with frequent vomiting, urgent thirst, restlessness, &c. She was willing to come into the hospital, but was at this time prevented. At half-past nine p.m. she was admitted; her eyes were sunk, face livid, skin cold, and blue; the respiration hurried and laborious, and the voice of the well-marked choleric character; pulse imperceptible; tongue white and shrivelled; vomiting and cramps severe, with constant jactitation. She was placed on the steam bed, and an enema with turpentine administered. Five minutes after admission the transfusion was commenced, and 9½ lbs. of the following mixture were injected in eighteen minutes:—

Carb. sodæ ℈iv;
Mur. sodæ ʒiv;
Albumen ℥iij;
Aquæ (temp. 105° to 110°) 10 ℔s.

After the first three minutes, the impulse and sounds of the heart, before very feeble, became distinct, and the pulse perceptible at the wrist. After the operation, the pulse was 92, and full; face flushed; surface warm; respiration easy; cramps and sickness relieved.

Hab. acet. plumbi gr. iij;
Opii gr. ij; *2dis horis.*

Midnight. She continued easy, with a good pulse, for nearly two hours after the injection; since then the thirst has returned, and she has been troubled with severe cramps and vomiting. The surface is now cold and clammy; the pulse almost imperceptible. She complains of excessive heat, and of a sense of fainting; within the last half hour she has become very restless. She has had some wine-and-water for drink, and she expressed a desire for gin-toddy, which she got, but immediately vomited.

At one a.m., May 29. The injection was repeated to the amount of ten pounds of the saline solution, with four ounces of albumen. During the operation her face became flushed, and she fell asleep; her pulse afterwards was 100 and full; she entered into cheerful conversation and expressed herself much relieved, and free from cramps, thirst, and sickness.

Calomel gr. iv;
Pulv. opii gr. i. *M. quam primum sumend.*

Half-past five a.m. Shortly after the last report she vomited a large quantity of watery fluid, and had a stool of the same character. The stimulant effects of the last injection continued for about three hours; for about an hour she was entirely free from cramps, but they have returned, and been very violent since, with much vomiting; the pulse has gradually sunk, and is now almost imperceptible; the face covered with clammy perspiration; her thirst is very urgent, and she complains of debility, and of a sense of heat, although the surface is below the natural standard. She had a draught containing acetate of morphia, but it was immediately rejected. She has had two greyish-coloured liquid stools. She passes urine involuntarily, and in great quantity.

Half-past six a.m. Pulse quite imperceptible; has vomited and purged frequently since last report; cramps very severe; tongue remarkably blanched; surface cold.

At half-past seven the injection was repeated to the amount of ten pounds, with the addition of ten grains of sulphate of quinine. After the operation the pulse was 94, and of good strength; no thirst; face flushed; heat of surface natural; she has no pain, and says she feels quite revived. Continue

Calomel c. opii. Hab. vin. hollandensis ℥ss.

Half-past eleven a.m. The pulse is again imperceptible; vomiting very severe; surface cold; is very restless; continues to pass some urine. The injection was repeated to ten pounds, with the same effects as in the last instance.

Nine p.m. Pulse 80, and irregular; vomiting and thirst continue; purging rather less, but of the same character; skin natural; passes urine.

Calomel gr. iv;
Opii gr. j; *nocte meridieque sumend.*
Hab. enema commune.

May 30. Nine a.m. Pulse 84; thirst, vomiting, and purging, continue; tongue foul, but rather cleaner than yesterday; thinks she passed urine during the night.

Hab. statim enema sennæ c. sulph. magnesiæ. Hab. pil. calomel c. colocynth. ij, *2da quaqua hora.*

Nine p.m. Pulse 92; skin natural; has had no vomiting or purging since morning; has passed a small quantity of urine; tongue furred, but cleaner and moister than at last report. Repeat

Pilulæ et enema.
Hab. spir. æth. nitrici ʒi *subinde.*

31. Half-past nine. Pulse 80; has vomited once since last report; had two dark-coloured stools from enema; has passed no urine since last report; complains of slight dry cough. Continue *Pilulæ et sp. æth. nitr.*

One p.m. Pulse 76; tongue cleaner; skin and countenance natural; has had no stool since last report; has passed about ℥xvj of urine, and is much easier. Continue.

Nine p.m. Pulse 80, feeble; tongue moist, but furred; skin natural; complains much of thirst, and vomits frequently; had one stool since morning; passed urine. Continue *Pilulæ. Hab. vin. rub.* ℥ss *subinde.*

June 1. Half-past nine a.m. Pulse 80, feeble; tongue furred; vomiting was very severe in the night, and she was ordered

Capsici gr. iij;
Calomel gr. iss;
Opii gr. ss. *2da qq. horâ.*

After the third or fourth dose it abated somewhat, and is now better; has had no stool; passes urine freely. Continue *Pilulæ capsici. Applic. hirud.* xx *epigast.*

Nine p.m. Pulse 80; tongue foul; has had a blister applied to her back, and taken three drops of hydrocyanic acid without any effect on the vomiting; has had one stool. Continue *Pilulæ capsici omni hora. Abrad. capill.*

June 2. Half-past four a.m. Pulse 68, and low; a good deal of restlessness and anxiety, with constant moaning. Six pounds and a half of the saline fluid were injected; it had no effect on the rapidity of the pulse, but increased its strength very considerably. To continue the pills, and to have soda powders with ginger.

Eleven a.m. Pulse 78, and full; tongue clean; heat of skin natural; she rested for some time after the transfusion; passed two or three bilious fluid stools; vomits green bilious fluid frequently; urine natural; is altogether much easier and more tranquil. Continue *Pil. et haust. efferv. App. emp. opii epigast.*

Half-past four p.m. The vomiting has continued almost incessantly since last report; it was relieved for a few minutes by ice and the opium plaster, but shortly returned. She complains constantly of thirst, and, latterly, of faintness, and desires her head to be sponged with cold water her pulse is 70, and tolerably full; skin of natural temperature; constant restlessness, and slightly delirious.

At her urgent desire the injection was repeated. Five pounds of water with six drops of liq. mur. morphiæ were thrown in. She seemed to become more collected, and the pulse became fuller; no other change was perceptible.

Half past nine p.m. Pulse 80, and pretty full; skin natural; tongue cleaner; vomiting has abated since injection; has had one brownish stool; passes urine freely. Omit *Pilulæ capsici. Hab. pil. calomel c. colocynth.* ij, *2da quaqua hora.*

June 3. Three a.m. The vomiting continued almost incessantly till within the last hour, when it seemed to be relieved by a suppository of opium gr. v, and a small quantity of pure brandy. She has had one or two stools, with a good deal of tenesmus, which was also relieved by the suppository; pulse 76.

Six a.m. Pulse 72, and soft; purging and vomiting less since last report. Continue *Pilulæ calomel c. colocynth.*

Twelve. Noon. Pulse 70, pretty full; skin cool; vomiting of dark bilious fluid continues; has passed several slimy evacuations in bed; urine natural. Continue *pilulæ.*

Nine p.m. Pulse 76; tongue clean; skin cool; vomiting continues; there is considerable fulness of abdomen. Continue

Pilulæ. Hab. enema purgans, et rep. post hor. ij *si opus sit. App. emp. opii epigast.*

Midnight. The enema was returned almost immediately, with some mixture of feculent matter; the vomiting is less urgent, and she has slept a good deal.

4. Ten a.m. Pulse 70; tongue clean;

skin cool; had several dark-fluid motions during the night; passes urine.

Hab. pil. calomel, colocynth, et sulphat. quinæ.

Seven p.m. Pulse 76; tongue pretty clean; skin natural; has had several dark-fluid feculent stools. Complains of fulness, and slight pain on pressure of the abdomen. Continue *Pilulæ. Hab. enema purgans statim.*

5. Eight a.m. Pulse 94; tongue clean; skin natural; has had one bilious motion during the night.

Hab. pil. calomel c. colocynth ij, *2da qq. hora.*

Nine p.m. Pulse 68; tongue dry; skin hot; vomiting less urgent; has had one more natural stool; passes her urine in bed. Continue *Pil. Hab. enema statim.*

6. Ten a.m. Pulse 68; tongue clean, but dry; heat of surface natural; had no stool during the night; passes urine freely. Continue *Pil. Rep. enema.*

Eleven p.m. Pulse 76; skin and tongue natural; has had three fluid feculent motions; mouth slightly sore. Omit *Pilulæ. Hab. ol. ricini* ℥ss, *cras mane.* Reported convalescent.

7. Ten a.m. Pulse 64; tongue clean; rather drowsy; had two stools since last report. *Hab. vin. rub.* ℥j, *2da qq. hora.*

Nine p.m. Pulse 76; tongue and skin natural; bowels open. Omit *Medicamenta omnia.*

9. Complains of diarrhœa; in other respects well.

Hab. haust. rhei statim, et post horas tres tinct. opii gtt. xx.

Continued well in the convalescent ward for ten days longer, when she left the hospital. G. M.

Drummond Street Hospital,
Edinburgh.

PREVENTION OF CHOLERA.

To the Editor of The Lancet.

Sir,—Having had the opportunity of observing this disease in every degree during its progress in Lynn I have endeavoured to ascertain the *juvantia et lædentia*, both in diet and medicine.

In July last cholera made its appearance in that part of Lynn which is called "North End," where the fishermen reside,—a situation most conducive to a rapid extension of the disease, and there certainly was (judging from the first few cases) an expectation of such a result. Fearing this, and explaining my views to our dispensary surgeon, whose assiduity in this epidemic I am in justice bound to declare has been unremitting, I ordered that whenever a case should present itself, every individual residing in the house, or having communication with the patient or family, should take the following medicine.

℞ *Misturæ cretæ f.* ℥viiss;
Tincturæ catechu f. ℥ss;
Olii menthæ piperitæ, guttas iv. Misce.
Dosis pro ætate adulta cochlearia duo ampla ter quotidie.

This medicine is one of every-day use in diarrhœa; but the novelty of practice consists in the time of administering it, a time, I grant, very unusual for a medical man to prescribe. Still, under all circumstances I consider the practice defensible, whether we look to the great susceptibility of the whole alimentary canal to morbid impressions at this season, or the importance of inspiring confidence and firmness of nerve, a thing very difficult at the present juncture. It was therefore with no ordinary pleasure that I found this last effect realized. The progress of the disease was soon and sensibly arrested. I can adduce many instances of families, in each of which a death from the most malignant type had taken place, without any of the others (who had regularly taken the medicine) becoming affected with it, or even experiencing premonitory symptoms. I am not so dogmatical as to assert, that this was a sequence resulting from the use of these means, but there seems great probability of it; and I can safely aver, from numerous cases, that no ill consequence has followed the practice. The practice may seem tame and inert, but I venture to anticipate, that a trial of it in any place will be found greatly conducive in checking the progress of this most pestilential disease.

Having closely attended a malignant and fatal case on the 30th of June, I experienced on the following morning some nausea with slight diarrhœa, a peculiar sensation of numbness in my legs, inability in the muscles to perform their function well, and a tendency to cramp. I immediatly procured a cretaceous astringent mixture with an increase of creta and gum, over that which the pharmacopœia prescribes, and have taken it regularly more or less up to the present time, without the least inconvenience as regards its constipating effect, having only on two ocasions required a simple enema of half a pint of water, in preference to taking any kind of aperient.

The difficulty of procuring reaction under extreme evacuations, induced me to attempt the prevention of the preliminary stage.

During the time this epidemic has prevailed amongst us, the cases of diarrhœa, or diarrhœa combined with vomiting, have been exceedingly numerous and severe in the dispensary practice. They have all yielded to the cretaceous astringent medicine alone, or united with moderate doses of laudanum, without one particle of mercury in any form, and not one case has subsequently been attacked with cholera.

Several of my medical brethren in Lynn, of extensive practice, have assured me that the same has obtained with them; one of them, however, prefers the *infusa catechu* to the combination of creta and catechu. It strikes me, however, that creta is more particularly beneficial at the present period, because that alkali is preferable which in union with an acid will not form a purgative compound, and such unquestionably is the carbonate of lime, in preference to the salts of soda or potash.

Although the treatment of *cholera* is foreign to this letter, I wish to observe, that the suggestion thrown out by Mr. Searle, of injecting *serum lactis* into the veins, in lieu of saline solution, was tried by me on the 6th of July last, and I am sorry to say, not with success; the patient, however, was, in fact, moribund, and quite unconscious of what was passing. On this account, therefore, I am not disposed to relinquish the plan, but purpose repeating it earlier in the disease, not by injection but gravitation, under the regulating power of a stop-cock, for which purpose I have contrived an instrument.—I have the honour to be, Sir, your obedient servant,

John Wayte, M.D.

Lynn, Sept. 3, 1832.

CASE OF
MALIGNANT CHOLERA
THREE DAYS AFTER PARTURITION.

To the Editor of The Lancet.

Sir,—As a member of the profession, I deem it my duty to lay before you an account of the treatment adopted by me in a case of malignant cholera (from which the patient is happily restored to health), inasmuch as the applications confirm the views and treatment of other satisfactory reports recorded in your valuable work.

M. D., aged 20, residing in a central part of London, was delivered after a natural labour, and the secondines expelled soon after by nature, on Sunday, the 26th of August, and continued in every respect doing well during the two succeeding days, taking as a laxative some castor oil early on the morning of the 29th.

I shall therefore proceed, from this date, to give you a history of the precursory, deceitful symptoms, and their treatment, as they daily developed themselves.

Aug. 29. Complains of pain, increased by pressure, in the uterine region. Had severe rigors; pulse 130, sharp; skin hot; nausea and restlessness; lochia and milk ceasing; tongue clean; thirst; oil had operated three times. Dreading inflammation of the uterus, or its appendages; bled her to sixteen ounces. *Calomel* gr. ij, *opium* gr. $\frac{1}{3}$, every four hours. Warm fomentations to the abdomen.

Evening, ten o'clock. Blood cupped, and strongly buffed. The nausea and pain on pressure had increased; pulse quicker but smaller; the bowels gently acted upon. *Perstat in usu pil. Hirudines x parti dolenti, et emp. lyttæ scrobic. cordis.* To relieve the nausea—soda water in small doses.

Morning, Aug. 30. The disease now developed itself; eyes sunk; tongue and breath cold; incessant purging and vomiting of rice-coloured matter; pulse small, and hardly to be distinguished; coldness; non-secretion of milk or urine; cramps and prostration of vital powers; incessant thirst. *Calomel* gr. j, *opii* gr. $\frac{1}{3}$, *camphor* gr. ii, *and capsici* gr. i, every $\frac{1}{4}$ of an hour, and one ounce of soda water occasionally, with bottles of hot water constantly applied.

Evening. Symptoms unmitigated.

Morning. Sept. 1. Symptoms the same, with the exception that the pulse and heat of body are improving. Continue the pills.

Evening. Warmer; pulse improving; nausea less; purging less; sense of heat in the stomach; no ptyalism; continue the pills every two hours, omitting the capsicum and camphor; nausea and purging less, and green.

Sept. 2. Pulse stronger; biliary stools; urine and milk secreted, and signs of improvement in every respect. Slight soreness of mouth.

3. Has feculent stools; increased secretions of milk and urine; ptyalism continues mild, although she has taken 160 grains of the submuriate.

The infant has been rather poorly during the night, and is this day cold and collapsed. Gave it two grains of calomel and some carminative mixture, and directed the warm-bath and friction.

4. The child in every respect better. The mother improved, and requiring merely laxatives and nutriment; ptyalism trifling.

During my attendance on this lady, I have had under my care a gentleman afflicted with nearly the same train of symptoms, and quite as bad, who was treated in the same manner, and on the appearance of ptyalism recovered, but it did not go to so great an extent; he also took calomel largely. I am yours, &c.

H. E. Church, M.R.C.S.

94, Chancery Lane, Sept. 5.

TREATMENT OF

MALIGNANT CHOLERA

(*When Collapse has taken place*)

WITH THE

NITRIC ACID BATH, &c.

By W. DUFTON, *Esq., M.R.C.S., Birmingham.*

T. BARRATT, aged eight years, was seized with vomiting and purging while at school a little before noon on Wednesday the 29th of August. I saw him at 5 o'clock p.m.; his eyes were then sunk, lips livid, hands blue, and rice-watery evacuations passed involuntarily, with continual vomiting of the same kind of fluid; tongue cold, breath cold, pulse imperceptible, frequent sighings, the whole surface of the body quite cold, and the voice extremely feeble; in fact when I first saw him, the direct impression upon my mind was that he was dying,—he was in a complete state of collapse. I ordered a hot bath to be immediately got ready, and in the meantime gave him a little brandy-and-water, with a few drops of tincture of opium, both of which were immediately ejected from the stomach. The bath was ready in about an hour; it consisted of about six gallons of water, and three ounces of nitro-muriatic acid. I had him placed in this up to the chin, and rubbed well with the hand of an assistant during the time he continued in the bath,—a period perhaps of twenty minutes. His pulse became perceptible when he had been in the bath about ten minutes, and before he was taken out, beat with considerable force; his tongue which was dry and cold, became moist and warm, though still below the natural temperature; he expressed himself better, and desired to be removed to bed.

My little patient appeared now to have recovered from the dreadful state of collapse which had threatened immediate death.

Five grains of calomel with five drops of tincture of opium, to be given immediately, the dose to be repeated in five minutes; both doses were immediately rejected. I now ordered an ounce of mercurial ointment to be rubbed upon the thighs at three times, one third immediately, another in an hour, and the remainder at the end of two hours, and I left him about midnight. Thursday morning saw him at eight o'clock. He had vomited occasionally during the night; tongue much the same heat; skin warm; pulse 120, and of moderate volume; sighing continued; had passed no urine; one evacuation during the night of the same character. Put him into a bath as hot as he could bear, containing about ten gallons of water, and half a pound of strong nitric acid; he had not been in five minutes, before the tongue became quite warm, the skin red, and the carotid arteries beat so violently as to be perceptible at a distance from the bath, and the pulse rose both in frequency and in volume. After ten minutes he was removed to bed, and bled to the amount of two ounces. The blood drawn was red like arterial blood, and after standing two hours contained but little serum.

Mercurial friction to be repeated to the amount of half an ounce immediately, effervescing draughts with a view of allaying the sickness, and one tablespoonful of cold water to be given when thirsty. Noon. Sickness not so severe; one grain of calomel, made into a small pill, to be given every five minutes. Evening. Vomiting took place after each pill, and they were consequently discontinued; mercurial friction to be repeated. Friday morning. Mouth affected by the mercury, tongue natural warmth, and my patient better in every respect. Noon. Much improved, pulse 100 and of good volume; extracted three ounces of blood. Afternoon. Mouth not so sore; mercurial friction to be repeated, and as he had made no water since his attack, and the irritability of the stomach had in some degree subsided, ordered 20 drops of nitrous æther to be given in a little water every hour, and to have carbonate of soda dissolved in water to drink when thirsty. 11 o'clock p.m. Had made water freely, a secretion which had now been suspended for sixty hours. Saturday morning, Sept. 1st. Not quite so well; had got out of bed early in the morning, and drank a *great quantity of water*, which produced vomiting; mouth not so sore; mercurial friction to be repeated. Noon. Complained of pain in his head; extracted four ounces of blood, and ordered the body to be sponged with nitric acid and water (warm). 5 o'clock p.m. Had a copious green watery, biliary evacuation, and had vomited some of the same sort of fluid, which continued during the greater part of the evening, but was very much allayed by large quantities of carbonate of soda.

It is unnecessary to state the subsequent treatment, which consisted chiefly of carbonate of magnesia, soda, &c., and light diet. He required great attention during the three following days, at the end of which time he began gradually to improve, and is now, with the exception of debility, quite well.

Birmingham, Sept. 10, 1832.

SUCCESSFUL TREATMENT OF
CHOLERA WITH MUSK.

To the Editor of THE LANCET.

SIR,—I have lately employed musk in several cases of cholera, with a success so uniform and decisive, as to make its introduction desirable, without loss of time, to the notice of the whole profession. I address you at so late a period in the week, that I must confine my observations to a mere announcement of the fact, but I purpose soon to forward to you cases in detail. It may not, however, be irrelevant now to state, that the results obtained by myself have been rather extensively corroborated in the practice of a friend with whom I communicated on the subject.

The salutary influence of the first dose of musk will be found to become manifest, by greatly mitigating, in a very few minutes, and, in many cases, by effectually removing, the cramps, the purging, and the vomiting. My plan has been to give at once fifteen grains, rubbed into a draught with a lump of sugar and a wineglassful of cold water; and I am justified in reporting that this first step, if taken promptly, will scarcely ever fail so to arrest the progress of the disease, as to leave the patient to easy and ordinary convalescence. This treatment has been proposed through one of its members to the City Board of Health, by whom it will be submitted to further probation; but as general deductions can be safely drawn only from extensive trials under various circumstances, I hope, through your instrumentality, that it will be adopted simultaneously throughout the country. So evident is the action of musk in cholera, that the practitioner will experience no difficulty in determining whether he need repeat its exhibition, or whether, having subdued the immediate cause of the disease by the first dose, he should direct his attention to the removal of its consequence by the ordinary means. I am, Sir,

Your obedient servant,
RICHARD LAMING.

48, Finsbury Square, Sept. 12, 1832.

CHOLERA AFTER INTOXICATION.

Not only does *habitual* intoxication strongly predispose to the cholera, but a fit of intoxication, during the prevalence of the epidemic, is extremely apt to be immediately followed by an attack. Of this M. Brierre de Boismont has given the following remarkable illustration:—"Three butchers visited a tavern, abandoned themselves to every sort of excess, and drank till they were intoxicated, so that they were carried home senseless. A few hours scarcely elapsed when the miserable men were seized with all the symptoms of cholera, which advanced with such rapidity as to prove fatal to the whole three within four hours."

THREE LETTERS
ON THE TREATMENT OF
MALIGNANT CHOLERA
WITH PURGATIVES.

By JOHN M'DIVITT, *Esq., Surgeon, Kegworth, Leicester.**

LETTER I.

To the Secretary of the CENTRAL BOARD OF HEALTH.

SIR,—Having attended, in conjunction with Mr. Day, the cases of cholera which have lately occurred at Barton, in the county of Nottingham, I beg leave to offer, for the consideration of the Central Board of Health, one or two improvements in the treatment of the malady, to which we have been led by almost incessant attendance on every severe case, and by diligently observing the effects of the different remedial measures employed.

The subject to which I would first solicit the attention of the Board, as being one of vital importance, is the propriety of using in the early stage of the disease, and even after collapse has commenced, the most powerful purgatives, instead of attempting to restrain the action of the bowels by opiate and other astringent medicines. In every one of our fatal cases, we had been able both to check completely the purging and vomiting, and to raise the body to its natural temperature; but no sooner had we effected these objects, than the respiration invariably became laborious and impeded, and death in a very short time ensued. Reflecting on these facts, we were led to consider whether, by the exhibition of large doses of purgative medicines, our practice might not be rendered more successful. To the adoption of such a plan of treatment we were besides encouraged, by certain appearances discovered on inspecting the body of a patient who died of the disease on Saturday the 11th ult. The stomach and bowels were found filled, almost to distention, with the peculiar serous fluid of which both the vomitings and the stools during life had consisted; and the mucous membrane of the intestinal tube, throughout its whole extent, was covered with a thick coating of muco-albuminous effusion. From these facts, taken in conjunction with the ones already mentioned, it appeared evident that it was a fatally erroneous idea to suppose that the danger of the disease was diminished, when the purging and vomiting were restrained. The true indication of cure, on the contrary, seemed to be, to assist the natural efforts of the system to expel the accumulated contents of the stomach and bowels. The thick coating with which the mucous membrane was covered, showed that, before it could be properly acted on, a large dose of some strong purgative would require to be administered.

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* Communicated by the Central Board.

Impressed with these truths, we resolved, whenever another case should occur, to give the purgative plan of treatment a full and a fair trial. An opportunity soon offered itself. On that very night we were called to a child, aged seven years, whose father, mother, and sister, had all, within the space of four days, died of the malady. She had been indisposed during the whole of the day, but had refused to acknowledge her illness; and she was then rapidly sinking into the stage of collapse. An emetic was immediately given, and as soon after its operation as the stomach was quieted—which object was speedily attained by placing the patient's feet in hot water, and by administering a few drops of the black drop in peppermint water—a large dose of calomel was exhibited. This brought away during the night several copious rice-water stools, after each of which our little patient continued for a considerable time greatly relieved from the indescribable feelngs of distress which are usually attendant upon this awful disease. About seven o'clock on the following morning, the stomach, which had remained easy during the night, became again affected with nausea, on account of which symptom half a grain of opium, combined with two grains of calomel, was given with the desired effect. Two hours afterwards, respiration becoming very laborious, and symptoms of collapse again making their appearance, five grains of calomel were administered. This was followed by a plentiful dejection, in which it was remarked that there was contained a very large proportion of the flocculent matter, an appearance which experience had taught us to regard as highly favourable in its import.

Between 10 and 11 o'clock, the patient was seen by Dr. Blake, of Nottingham, the author of the valuable essay on delirium tremens, who concurred in the propriety of giving another dose of calomel, together with a drop of the croton oil; and who prescribed, in addition, a drachm of tartarized soda, to be taken in an effervescing draught every half hour, till the bowels should be once more thoroughly acted on. In the second evacuation which was passed after these medicines were taken, a slight tinge of bile could be discerned; and in the succeeding one there were seen floating on the surface, several little round lumps of mucus, the shaggy surface of which was covered with bile. A striking change was now visible in the patient's countenance. The lips had become quite red; the cheeks and forehead had lost much of their dingy, leaden appearance; and the eyes did not seem so deeply sunk as they had previously done. The voice, too, was more natural; the pulse was not merely perceptible, but somewhat full and regular; and respiration was performed with comparative ease. Thus she continued until near 6 o'clock in the afternoon, about which time she suddenly began to sink, and very soon she appeared to be moribund. From this state, however, we succeeded in raising her, merely by giving, every now and then, when she was capable of swallowing it, a teaspoonful of arrow-root with brandy. Subsequently, she fell into typhus fever, under which she still labours.

I have entered pretty fully into the detail of the above case, because I feel thoroughly convinced, that, had it not been for the system of active purgation which was followed in it, the little patient never could have got through the stage of collapse into which, when we were first called to her, she had already begun to enter.

Another case has since occurred in which the purgative plan of treatment, owing to its having been instituted at an early stage of the disease, appeared even to greater advantage.

The next point to which I would advert is the immense importance of upholding the patient's strength, which cannot but be greatly exhausted by the excessive secretion into the alimentary canal. This was well exemplified in the case just related, in which death must undoubtedly have very soon ensued, had we not, by cautiously and perseveringly supplying the organs of nutrition with material to work upon, tempted them to resume the exercise of their functions. Indeed, I have no doubt that numerous lives have been lost merely from this one essential particular not having been duly attended to. I would, therefore, strongly recommend that in every case the patient should be allowed, from the very commencement of the disease, as much chicken-broth as he pleases. By this means the operation of the purgative medicines would be encouraged; the intense thirst, which is one of the most distressing symptoms of the malady, would be relieved; the bowels would be soothed, and induced to renew their office of absorption; and thus the system would be enabled to bear up against the violence of the disease. This measure has in its favour the recommendation of Sydenham, an authority of no small weight in anything relating to practice.

While we continued to look upon the purging as a very formidable symptom, we were in the habit of employing a mixture which Mr. Day had long found singularly efficacious in the common cholera. It is composed of one drachm of nitrous acid, from 40 to 60 drops of laudanum, and half a pint of camphor mixture. The dose is two or three tablespoonfuls every second or third hour. This medicine we still continue to use, not because it has a tendency to check the purging, but because it seems to exert a beneficial influence on the lining membrane of the intestines, probably by its action on the thick muco-albuminous coat with which it is covered. We have thought, too, that in conjunction with the nitro-muriatic acid bath, it has had considerable effect in re-establishing the biliary secretion.

When I speak of the above measures as having proved eminently advantageous, it is not, of course, my intention to represent all others as either useless or injurious. There are several, on the contrary, which may, under certain circumstances, be not merely admissible but absolutely necessary. Blood-letting is one of these; but of its effects, when employed in union with the purgative plan of treatment, I cannot speak from experience. The propriety of giving stimulants, too, even while abstracting blood and administering purgative medicines, is unquestionable.

In venturing to lay before the Board the above observations, I have been actuated solely by one motive—the wish to procure a sufficiently extensive trial for measures which, both from reasoning and from experience, I conscientiously believe to be extremely beneficial. Some of these measures may not be altogether new, but that is a matter about which I do not concern myself. I see in every newspaper, in every medical periodical which I take up, methods of cure the most absurd or the most pernicious, confidently recommended to the public. I consider it, therefore, a sacred duty which I owe to humanity, to come forward and make known a plan of treatment from which we have experienced the most excellent effects. In doing this I have mis-stated nothing—I have kept back no important fact—I have explained the grounds upon which our practical conclusions have been founded; and, should further experience not confirm the truth of the principles laid down by me, I shall not be backward either in rejecting them myself, or in persuading others to do so.—I am, Sir, JOHN M'DIVITT.

Aug. 18th.

LETTER II.

SIR,—In reply to your letter of the 20th ult., which you did me the honour to address to me, I regret to have to state, that both the cases of cholera in which I represented the purgative plan of treatment to have proved so beneficial, have since terminated fatally. The child (Chamberlin) whose case I detailed in my former communication, and who, I said, was still labouring under typhus fever consequent upon the malady, died at seven o'clock on Sunday morning last. For three or four days previously, there had been symptoms of cerebral congestion, for which a blister had been twice applied to the nape of the neck, and other suitable remedies used. On Saturday morning she began to reject by vomiting whatever was given her, whether food or medicine; and on the evening of that day the woman in attendance upon her remarked, that when drink was offered to her, her hand wandered widely from the cup which she attempted to grasp. Early on the following morning she was seized with a severe rigor, which she did not long survive.

The second case was that of Thomas Glover, aged two years and eight months. This child was attacked on Monday the 13th ult., about eleven o'clock a.m., and was seen by both Mr. Day and me a few minutes afterwards. The purging and vomiting were violent, and, short as had been the interval from the moment when the little patient was first discovered to be ill, marks of deep-felt distress were evident in its countenance. A *hippo* emetic was administered on the spot, and, when its operation had ceased, the child was placed in a warm-bath. This measure having failed completely to tranquillize the stomach, three drops of the black drop were given in a teaspoonful of peppermint-water. This was followed, in the course of fifteen or twenty minutes, by an eight-grain dose of calomel, which was repeated an hour afterwards. At first the dejections consequent upon the exhibition of this medicine, consisted merely of the rice-water fluid so often mentioned as characteristic of the malady. Towards night, however, they began to alter; some traces both of bile and of fæces making their appearance, and on the following morning they were of a bluish slate colour. The condition of the little patient was now wonderfully improved. It was apparently quite free from pain; and except that the forehead had not yet quite lost its dingy leaden hue, and that the eyes, heavy and sunken, and having the under half of the conjunctiva completely covered with a net-work of injected blood-vessels, an appearance first pointed out to us by Dr. Blake, and which we have never yet found wanting in any genuine case of cholera, still retained a livid circle around them. There were no symptoms present, by which even an experienced person could satisfy himself that the child had been, and indeed still was, labouring under cholera. On the afternoon of this day, the nitro muriatic acid bath was used with considerable benefit, and towards night the evacuations from the bowels having ceased, while the abdomen felt full, and the respiration was laborious, another dose of calomel was given. Several copious stools, containing an abundance of bile, were passed during the night, and, on the following morning, we found our little patient in a state which might have well justified us in entertaining the most confident hopes of his recovery. From this period until Sunday last (the day before that on which he died), there was not a single symptom which could lead us to anticipate an unfavourable result. All that we did, therefore, was to endeavour to keep up a healthy condition of the hepatic and intestinal secretions by a small dose of the hydr. c. creta, given every night and morning; and by a little saline aperient medicine, administered twice or three times a day. On Sunday morning the little patient appeared drowsy, and on being roused, was exceedingly peevish and irritable. Towards evening coma came on, and the eyes were turned up, leaving nothing more than the under half of the conjunctiva visible. During the night two or three convulsive fits occurred, and on the following morning the child expired.

It will readily, I think, be admitted that the fatal termination of these two cases cannot, in the slightest degree, detract from the merits of the purgative plan of treatment. In neither case did death take place before the eighth day, long before which time the original malady might very fairly have been said to have entirely ceased. The true cause of death, in both patients, was cerebral congestion, an affection which, in such young subjects, is very likely to supervene upon any violent disease whatever. But any comments on the facts of the cases are, I believe, unnecessary.

I shall now briefly detail a case which was under treatment at the date of my last communication, and to which, as the event was doubtful, I did not then allude.

Mary North, aged 78 years, grandmother to the last child, had been in attendance upon it from Monday, the day on which it was attacked, until Wednesday night. She then went to her own house, where she remained during the next day much indisposed and affected with diarrhœa. On Friday morning the symptoms of her illness becoming alarming, we were called in. We found her vomiting, and suffering from cramp about the heart and stomach. The pulse was exceedingly feeble, and her skin was partially covered with a cold clammy perspiration. A little brandy and hot water were given, and very soon afterwards a scruple dose of calomel. When dread was no longer entertained of the calomel being rejected by the stomach, a little more brandy and hot water were allowed, and the patient was permitted to drink freely of any warm, bland, and nourishing liquid. In about three quarters of an hour she passed a very large watery evacuation, which was followed by others at different intervals during the whole of the day. The patient was not a little surprised to find that, instead of being rendered weaker by these evacuations, she felt as if she gathered strength from each. She passed a pretty favourable night, and on the following morning took, with great pleasure, another dose of the calomel, which consisted, however, only of ten grains. On Sunday morning she felt quite relieved; her stools had become of a natural colour, and she complained merely of debility. For this, gentle tonic medicines were prescribed, and she is now, I am happy to say, convalescent.

It will be remarked, that in this case, as well as in that of the grandchild Glover, calomel was the only purgative employed. Our reason for this was, that both patients were exceedingly feeble, and we had an opinion that calomel, while it would prove sufficiently active in its operation, would be considerably less irritating than any other energetic purgative. Under other circumstances we would have joined, as an auxiliary to the calomel, the croton oil.

Besides the cases above related, we had, within the last few days, two others. Of these, one was very mild, and the patient, without owing much to medicine, is already convalescent. In the other, which was an exceedingly severe case, we are at this moment making a further trial of the plan of treatment by purgatives. Should the Board not think me tiresome, I shall probably trouble you with the result of this case. I have the honour to be, &c.

Aug. 22nd. JOHN M'DIVITT.

LETTER III.

SIR,—In the last communication which I had the honour of addressing to you, I promised to acquaint the Board with the termination of a case of cholera, in which we were at that time making a further trial of the purgative plan of treatment. That promise, as the issue of the case is no longer doubtful, I now hasten to fulfil. But, in the first place, it will be necessary for me to enter somewhat into its details, in order to enable the Board to form a satisfactory opinion respecting the merits of the plan of treatment pursued in it.

John Glover, aged 26 years, a labourer,

and father of the child of that name, whose case I have already reported, began to feel indisposed on Sunday the 19th ult. He had diarrhœa, together with a feeling of uneasiness about the stomach, for which he took a calomel and opium pill, and also a dose of the nitrous acid mixture. By these medicines the above-mentioned symptoms were relieved, and on the following morning he went out as usual to his work. We saw him at noon, when he complained of thirst, pain of the head, and general feverishness. He was persuaded to confine himself to the house, and as his pulse was full and resisting, about sixteen ounces of blood were taken from his arm. The blood flowed in a continued stream, but was, with the exception of the last two or three ounces, of a completely atramentous appearance. On the next day he felt considerably better, and wished again to go to work, but he was induced to defer doing so. Early on Wednesday morning the diarrhœa returned, together with severe spasmodic pain about the præcordia, nausea, and slight vomiting. When we visited him about noon, we found that the powers of life were rapidly yielding to the violence of the disease. The pulse was exceedingly weak and flagging; the respiration was performed with great labour; the skin was partially covered with cold clammy perspirations; the eyes were deeply sunk and surrounded with a livid circle; the face, hands, and feet, were of a dirty leaden colour, and the most intense thirst was complained of. At this time the cramp about the stomach had entirely ceased, and the patient assured us that he had no pain whatever. While mentioning this fact, it may not be amiss to remark, that in two of our fatal cases, little or no cramp or pain was felt throughout the whole course of the malady. Indeed from what I have seen of the disease, I have no hesitation in saying that it may exist, and in a highly malignant form too, with scarcely any vomiting or purging, and altogether without cramps.

A scruple dose of calomel was given to the patient on the spot; and he was allowed, after the lapse of about half an hour, to drink as much chicken-broth as he pleased. Except the external application of heat, these were the only means of cure which were had recourse to. No brandy, no opium, no stimulants whatever were administered; and yet in a few hours, during which time several copious stools were passed, the condition of the patient was evidently very much improved, the pulse having become fuller and steadier, the complexion as well as the voice more natural, and the countenance much less expressive of suffering and distress. A slight tinge of bile, too, began to be visible in the evacuations. But the bowels now ceasing to act, while the breathing continued rather laborious, another scruple of calomel was given, together with one drop of croton oil. These medicines operated briskly during the night, and a repetition of them on the following morning brought away a large quantity of dark green bile. From this time the patient might be considered to be almost out of the reach of danger. He remained, however, for two or three days low and feeble; and, subsequently, there appeared symptoms of determination to the head, for which a cold lotion was applied, and a little aperient medicine given. During the whole of the period of convalescence, we were particularly attentive to the state of the stomach and bowels, but we could not discover in either the slightest mark of inflammation. A single fact, such as this, is sufficient to outweigh all the assertions that speculative men may make, respecting the danger of employing active purgative medicines when such an irritable state of the alimentary canal exists.

Let us now briefly advert to another case, the only one that has occurred to us since the date of the foregoing. The patient was Elizabeth Musson, ætat. 14 years, who was attacked with purging, vomiting, &c., on the afternoon of Thursday, the 23rd ult. The stools, in the commencement, contained both bile and fæces; but it was not long before they put on the rice-water appearance so characteristic of the malady. It is unnecessary to enter fully into a detail of the treatment, which was, in almost every respect, similar to that employed in the case of Glover. Nearly thirty hours had elapsed before the peculiar flocculent matter ceased to be visible in the evacuations, and not until then was purgation discontinued. The result has been altogether satisfactory. The patient is already completely recovered.

Eight days have now elapsed since the occurrence of the last of these cases; and it is not impossible that, in so retired a part of the country, we may not have any further opportunities of applying to our practice the certain test of experience. We have already, however, seen enough of its good effects to convince us of its superiority over every other plan of treatment which has been hitherto proposed. In speaking thus, I give expression not merely to my own sentiments, but to those of Dr. Blake, who witnessed some of the cases, and of Mr. Day, who had a principal part in the management of them all. They believe, as I do, that the doctrine which prescribes powerful astringents, and large quantities of energetic stimulants, is at once false in theory and fatal in practice. They believe that active purgation, by carrying off the unnatural contents of the bowels, and with them, possibly, the poison which produced the disease, relieves the breathing, quickens the circulation, and rouses the energies of the whole system. Such being the case, I am anxious that the practice should be generally known; and I feel assured that, if once fairly tried, it cannot fail strongly to recommend itself. I can scarcely, however, flatter myself that, whatever I may say in its favour, it will receive that notice which it undoubtedly deserves. The public mind has been so often disappointed by the failure of plans of treatment which have been held out as most efficacious, and of which the most pleasing hopes were entertained, that it is now almost disposed to succumb quietly to the overpowering evil. Even the treatment by saline injections into the veins, so confidently recommended by its first proposer, whose "*Eureka*" re-echoed throughout the country, was hailed almost universally as the harbinger of a splendid triumph to science, has turned out on trial to be injurious rather than beneficial. In such a state of things it behoves, I conceive, the Central Board of Health, in particular, to use every exertion that may tend to keep awake a spirit of investigation, and to procure a fair and satisfactory trial for every rational plan of treatment, no matter by whom it may happen to be proposed. I have the honour to be, Sir, your most obedient servant, JOHN M'DIVITT.

Kegworth, Leicestershire,
Aug. 31st, 1832.

CIRCULARS ADDRESSED TO THE PROFESSION BY THE CENTRAL BOARD OF HEALTH.

No. 1.—Council Office, Whitehall,
Sept. 3, 1832.

Sir,—The Central Board of Health being anxious to obtain, from authentic practical sources, short outlines of the different plans of treatment in cholera, which may have been considered most successful, I am directed to request that you will have the kindness to submit the enclosed (No. 2.) to the medical members of your board, and to any other medical gentlemen in the neighbourhood who may have had extensive practice in the disease. You will also request the medical gentlemen in charge of cholera hospitals to fill up, and forward through you, a return of the following form, for each of these establishments within the district under the superintendence of your board. I am, Sir, your most obedient servant,

(Signed) WM. MACLEAN.

To the Secretary of the Board of Health at ——.

No. 2.—Council Office, Sept. 3, 1832.

Medical gentlemen who have had experience in the treatment of cholera, and are of opinion that they have been successful in their practice, are requested to forward to the "Secretary of the Central Board of Health," under cover to "The Clerk of the Council in waiting, Whitehall," a short account of their respective methods of treatment of the epidemic:—

1st. When in the form of bilious diarrhœa.

2nd. In that of rice-water evacuations.

3rd. In the stage of collapse.

RETURN *of Patients admitted into the Cholera Hospital*

at ————————

From ———————— to ———————— 1832.

TOTAL NUMBERS.			
Admitted.	Died.	Recovered.	Remarks.

(Signed) ————————

AUSCULTATION AND PERCUSSION.

To the Editor of THE LANCET.

SIR,—Auscultation and percussion do not appear to have gained so many followers as their great importance would have led one to suppose. What can be the reason of indifference to the valuable assistance which the stethoscope is enabled to afford? Men are averse to introduce into practice innovations, as they are pleased to call them. They have done, they say, without the stethoscope for many years; they have been enabled, they aver, to detect peripneumonia and the other diseases within the thorax, by the pulse, &c.; they do not know, they say, what a piece of hollow wood is to tell them. These and many other objections are brought forward by men who, I am sorry to say, are too indolent to investigate the facts of the case. * * * *

If the following chart should be of the slightest service to any of your numerous readers, you will much oblige me by inserting it:—

DISEASES OF THE LUNGS,

With their Physical Signs Alphabetically arranged.

BRONCHITIS.
Percussion—Natural.
Respiration—Sonorous rale; sibilous rale; mucous rale.

BRONCHIAL HEMORRHAGE.
Percussion—Natural.
Respiration—Mucous rale.

CATARRH.
Percussion—Natural.
Respiration—Respiratory murmur extinct.
*** The respiratory murmur reappears at intervals, especially after coughing.

DILATED BRONCHIAL TUBES.
Percussion—Natural.
Respiration—Bronchial respiration; cavernous respiration.
Voice—Bronchophony.

EMPHYSEMA.
Percussion—Tympanitic.
Respiration—Feeble respiratory murmur; crepitous rale; sonorous or sibilous rale.

EMPYEMA.
Percussion—Dull.
Respiration—Feeble respiratory murmur; and only heard at intervals.

GANGRENOUS ABSCESS.
Respiration—Cavernous respiration; mucous rale.
Voice—Pectoriloquy.

HYDROTHORAX.
Percussion—Dull.
Respiration—Feeble at the root of the lung; extinct elsewhere.

ŒDEMA.
Percussion—Dull.
Respiration—Respiratory murmur feeble; subcrepitous rale.

PNEUMOTHORAX.
Percussion—Tympanitic.
Respiration—Respiratory murmur feeble, and only heard at the root of the lungs.

PLEURITIS.
Percussion—Dull.
Respiration—Feeble.
Voice—Œgophony.

PERIPNEUMONIA.
Percussion—At first natural, afterwards dull.
Respiration—In first stage feeble; respiratory murmur and crepitous rale. In second stage no respiratory murmur. In third stage, bronchial respiration.
Voice—In third stage, bronchophony.

PHTHISIS.
Percussion—Natural at first, dull afterwards; if the vomica be *very* large, tympanitic.
Respiration—Natural at first, extinct afterwards; first stage, bronchial respiration.
Voice—In first stage bronchophony.

PULMONARY APOPLEXY.
Percussion—Natural.
Respiration—Extinct at the spot, but surrounded with a crepitous rale.

PNEUMONIC ABSCESS.
Respiration—Cavernous respiration, and mucous rale.
Voice—Pectoriloquy.

PLEUROPNEUMOSOTHORAX.
Percussion—Tympanitic above, dull below.
Auscultation—Metallic tinkle.
Succussion—Fluctuation.

TUBERCULOUS ABSCESS.
Respiration—Cavernous respiration; mucous rale.

VOMICÆ.
Percussion—Dull when full of fluid, clea when full of air.
Respiration—Extinct respiratory murmur

Stethoscopic signs and other symptoms should go hand in hand, side by side, and thus armed at all points, surely we shall be better prepared to combat—to weaken—to subdue, the numerous, and oftentimes dangerous, affections within the chest. I remain, Sir,

Your constant reader,
PYE HENRY CHAVASSE.

Birmingham, Sept. 4th, 1832.

THE LANCET.

London, Saturday, September 15, 1832.

WITHOUT calling in question the zeal or integrity of Dr. HARRISON, and his co-petitioners in the cause of medical reform, we must be permitted to express our regret that the matters in the petition, which they presented to the House of Commons by the hands of Mr. HUME, were so confusedly, not to say defectively, arranged, that the abuses which they reprobated failed in that House to excite an interesting, spirited discussion. It does not appear to us that the *prayer* of the petition is at all in keeping with the *objects* which the petitioners had in view; however, let us quote the entreaty :—" And your petitioners humbly pray that your honourable House will take into consideration the constitution and conduct of the College of Physicians, and investigate the nature of the charter and acts of Parliament under which they have claimed to act, and provide, in justice to the community at large, and the British physicians in particular, in such manner as shall in the wisdom of your honourable House be deemed best. And your petitioners," &c. Here there is a request that the House would inquire into the constitution and conduct of the College, and the nature of the charter and acts of Parliament under which the Fellows allege their proceedings are regulated. Thus, in short, an investigation of the *law* is sought for, although the petition is crowded with statements all tending to prove that the law has been violated. But, it may be contended, that Dr. HARRISON and his co-operators complain of the defective state of the law, as well as of the abuses which have sprung up under its mal-administration. If this objection were well founded, then we should admit that the prayer of the petition was in keeping with the general tenour of its contents ; so far, however, from there being any complaint made against the law—that is, that the charter granted by that lustful monarch Henry the Eighth, is an imperfect instrument—that it is incompetent to provide for the sound government of the medical body—so far, we say, from the petition of Dr. HARRISON having even any tendency to prove that the charter is such an ill-constructed, inefficient, instrument of Government, the Doctor and his coadjutors insist upon its adequacy to accomplish all those great objects which the petitioners themselves profess to contemplate. But let us quote the words of the document itself. " Your petitioners have, they presume, *clearly proved* to your honourable House, in the *foregoing details*, that (now mark, reader) *the charter is in itself admirably fitted* to protect the metropolis against dangerous and unprincipled pretenders, and to provide the apothecaries' shops with genuine drugs."

Nothing certainly can be more extraordinary than an admission of this description in a petition for a redress of grievances. The House of Commons is one of the branches of the legislature ; it is quite beyond its functions to *administer* the law or to punish the abusers of the law ; it is its duty to see that proper laws are enacted, and to leave the *execution* of them to other persons connected with the public service. If the charter of the College of Physicians be " admirably fitted " to protect the public from quacks, why does not Dr. HARRISON proceed against the college by *mandamus* in the Court of King's Bench ? If the charter be good, in God's name let us have the benefit of it ; but let it not be said that medical reformers are so imperfectly acquainted with the chief institutions of their country as to confound right with wrong, cause with effect, and a dirty stream with the pure fountain from which the current originally flowed. It is quite clear, that if the charter be "admirably fitted" to govern the physicians of this metropolis, the petitioners had no business on the floor of the House of Commons; on the contrary, they should have taken the *abusers* of the law to the Court of King's Bench, there to receive their award of punishment, for those high crimes and misdemeanors of which they might have been found guilty. The time, probably, would not be ill-bestowed, if Dr. Harrison and his fellow labourers would undertake to explain to the profession, in what consists the "admirable fitness" of the charter for the government of the medical profession.

We insert a letter addressed by the Central Board of Health to practitioners in medicine, requesting details of their mode of treatment of cholera, with the results thereby obtained. This document, we are given to understand, was drawn up some time *before* the publication of our proposal for the institution of a "moveable clinical commission."

After careful and deliberate reflection on the amount of benefit to be attained by the circulation of the letter in question, and after considering how far it is calculated to effect its own object, we regret to say that, in our opinion, this document will totally fail in improving, in the slightest degree, the treatment of the malignant cholera. We proceed to state the grounds of this opinion. Let us suppose that from every hamlet, town, or city in the kingdom, answers to this circular have arrived. Let us suppose these replies read, classified, and their prominent features laid open to public inspection, what then does it all amount to? Dr. ——, of Bristol, has found "practice No. 1" attended with the most gratifying results, and he furnishes a battalion of statistical tables in proof of his assertion. Mr. ——, of Plymouth, on the other hand, found "system No. 2" never to fail when duly and correctly carried into effect. Thus runs the list through a series of fifty categories. At last we, perchance, find some solitary respondent to the circular, admitting that he has progressively tried all the practices from "one" to "fifty" inclusive, but that in his hands they successively and totally failed. The reader of these classified reports of success and triumph over the demon of the pestilence, will then, perhaps, turn to the sums total of cases and deaths, when lo! instead of a decrease of fatality proportionate to the list of pretended specifics, he finds, by the evidence of obstinate columns of figures, that the proportionate mortality has been recently augmenting nearly 25 per cent.

The sad and sober truth we feel is this; no reliance ever has been placed, or ever will be placed, on the reports of great success advanced by individual practitioners. As we have over and over again repeated, where fame and fortune are to be gained by the lucky discoverer of a successful mode of practice, it cannot be expected that the statements of an individual will be implicitly relied on. Nor can it be supposed that professional controversies will be silenced by such reports. The result, in short, will be, that after some weeks or months have been consumed in the transmission of these circulars and receiving and arranging the replies, we shall be precisely in the same position we now hold, having abundance of the most discordant evidence before us, in which information of practical value cannot be distinguished from worthless speculations, and from which society will derive no benefit whatever. Such a mass of contradiction and perplexity was never accumulated as these reports will turn out, and the sequel will be, that public confidence in the medical profession will sustain the most serious injury.

How widely different would be the results if the proposal advanced in the last Lancet were carried into effect, must be obvious to every dispassionate and reflecting being. We there pointed out the manner in which each practice might be fairly, and once for all, put to the trial, and a solemn, unimpeachable verdict delivered on its merits. We showed how professional jealousy and local prejudice, how the operation of private interests, of exaggeration, and of detraction, would be avoided. We showed how time would be economized, how the best mode of treatment could be ascertained, and though last not least, how public confidence could be directed to such a system. The paramount value of the last effect need not be explained to any one who remembers the opposition which "vaccination" had to contend with, and who recollects the caricatures which once figured in our print-shops, representing the infant "victims" of the cowpox, as reaching maturity with the ornamental appendages of horns and tails!

Since the publication of our last number, we have had abundant opportunities of ascertaining the favourable manner in which the public and the medical profession have received our proposal. Men of all parties agree in the opinion, that by such a plan alone can we arrive at the grand object of "*improving the treatment of cholera.*" We have not heard a solitary argument advanced against it, except an allusion to the national pecuniary outlay contingent on defraying the expenses of the hospitals to be placed under the care of the moveable clinical commission! We are scarcely disposed to keep temper with such paltry economists. The maximum of the expense could not exceed 10,000*l.*, an amount of expenditure, be it remembered, which must be incurred by the infected parishes if no such commission were instituted. Indeed, under any circumstances of epidemic disease, we doubt the justice of permitting parishes to defray their own expenses. The calamity is a national one, and should be met by national resources. It is surely enough for a plague-stricken district to be decimated by a pestilence, without having to contend with the additional miseries of cholera-rates and the jobberies to which they give origin. We have shown too, how from this expenditure the nation can reap an ample profit, and how by the wise and proper investigation of the treatment of the epidemic, further outlays may be avoided in the districts as yet uninfected.

We shall return to this topic again and again, until we have effectually excited the public attention. We have not taken up the matter on light grounds, and we pledge ourselves that we will not lay it down until the proposal be shown to be either impracticable or absurd. In the mean time, as an incentive to the apathetic, and as a practical proof of the urgent necessity of learning something more about the treatment of the malignant cholera, we subjoin an extract from the *Limerick Chronicle* relative to the havoc and misery produced in Sligo by the working of that disease, the means of successfully treating which, it is our object as well as our duty to investigate.

"We regret to state that the cholera still continues to rage to a frightful extent in Sligo. Many of the inhabitants have fled and taken shelter in the woods. The town is quite clear of the labouring classes, they having repaired to the woods; those who remain are very badly off for provisions, the country people being afraid to come in. Every house is closed, with the exception of one shop, which is kept open to supply the people with groceries. A medical friend of ours favoured us with a letter yesterday, giving a melancholy account. He says, 'The disease is still on the increase. The persons who die during the night are not interred until next morning, when thirty or forty are often buried in one pit, thrown promiscuously in without coffins or shrouds. What an humbling lesson!' A correspondent this morning says, 'Yesterday morning I spent in looking for a joint of meat, but could not get one for love or money. I then went to my farm to drive home a sheep, but there was none there; they had strayed away, not one to mind them. If the Lord does not stay the plague I fear few will live to tell the sad fate of our once happy but now unfortunate Sligo.'"
—*Limerick Chronicle, Sept.* 5.

SIR H. HALFORD, MR. BRODIE, AND "LITTLE BOBBY KEATE."

[*From a Correspondent.*]

Sir Everard Home, the serjeant-surgeon to the King, died three weeks since at an advanced age. Mr. Keate, whose long, and, it is believed, gratuitous, attendance on almost all the branches of the royal family, and particularly on the King, is well known, communicated this event to his Majesty. This gentleman's sense of delicacy prevented his doing more, but it was expected that his Royal Master would have embraced with alacrity such an opportunity of requiting his faithful services. Mr. Keate did, however, wait on Sir Henry Halford, who immediately told him, with much apparent warmth, that no person but himself could receive the place—that it would be shameful were it, under the circumstances, given to another, and promised to recommend his appointment to the King. Mr. Keate therefore was not a little astonished at receiving, on the same day, a note from Mr. Brodie, stating that Sir Henry Halford had recommended him, Mr. Brodie, to apply for the office, and, in his application, to mention that he, Sir Henry Halford, knew it to have been the intention of his late Majesty to confer it upon him, and soliciting Mr. Keate's favour in bringing the matter before the King. Upon receipt of this letter Mr. Keate wrote to Sir Henry Halford, that he had learned by a note from Mr. Brodie, a copy of which he enclosed, how great Sir Henry's anxiety had been to fulfil the task he had undertaken in the morning, but that he would dispense with Sir Henry's future services, and make his application through the official channel, Sir Herbert Taylor. To the disappointment, however, of Mr. Keate, it seems the office has been bestowed on Mr. Brodie; and it is said that since the appointment has been made, Sir Henry Halford has ventured to write an exculpatory letter to Mr. Keate, in which he affirms that he was not in the least degree accessory to, and did not at all assist, Mr. Brodie's canvass for the place, but that when he saw the King he found the matter had been already settled in favour of Mr. Brodie. This last letter having been communicated to Mr. Davis, whose constant access to his Majesty is well known to the profession, that gentleman immediately wrote to Mr. Keate, informing him that but for Sir Henry Halford's interference, the appointment would have been disposed of in his favour!!

FEES OF HOSPITAL SURGEONS.

To the Editor of The Lancet.

Sir,—I am about to relate a splendid example of right royal rapacity in one of the pure surgeons of this metropolis. I shall forbear to comment on it, because it were impertinent to anticipate any man's judgment in a matter whereon all should think for themselves.

A widow, of very moderate—nay, even straitened circumstances, had a scirrhous tumour in the breast. The gentleman who attended her, although a very able man, and competent to the discharge of any duty that accident or necessity might devolve on him, was, in this instance, unwise enough to eschew the responsibility, and forego the profit and reputation that would have attended his success in operating. Mr.——, an official in that institution where ignorance and cupidity vie with each other to earn the laughter and scorn of the country, was consulted, and performed the operation. When asked the amount of "his fee," guess the alarm and astonishment of all parties at the reply, "*A hundred guineas, and ten guineas for my assistant!!!*" The most animated remonstrances only effected this merciful mitigation:—"Well, as you say the parties are distressed, (although *we hospital surgeons have only two fees, you know, 50l. or 100l.,*) I'll take 60l. for myself, and 5l. for my assistant." This modest amount was raised by the patient only with the greatest difficulty, and with serious injury to the circumstances of the family. Mr. G—t received 15l. for the after-dressing and medicine.

This case may suggest some curious speculation, interesting both to the public and the general practitioner. I am, Sir, your obedient servant.
R. G.

Sept. 1832.

CITRIC ACID IN SCURVY.

To the Editor of The Lancet.

Sir,—You might very well apply the words "dangerous and unwarrantable" to the observations of Dr. Stevens on the use of citric acid, as a preventive of scurvy. I know not to what cause to attribute such ignorance and misrepresentation. He has ventured to condemn a remedy which the experience of nearly half a century has proved of the highest utility, because it happens not to agree with his theory, and to oppose his individual opinion, unsupported by a shadow of proof, to the united evidence of hundreds of talented and experienced men in its favour.

Dr. Stevens will greatly oblige many of your readers, by giving his authority for stating that citric acid is fortunately no longer in general use as a preventive of scurvy, and hence the variety of the disease, although salt meat is as much used now as formerly. Being an officer in his Majesty's navy, I can positively say that the reverse of this is the case. When a man-of-war has been fourteen days at sea on salt provisions, lime-juice is served out to all the men daily, being mixed with their spirits, with a due proportion of sugar, and has been attended with the most beneficial effects in preserving them from a disease which was so common and fatal before its introduction. The diet of the men is also very much improved. Pease and flour are liberally supplied, and the bread is far superior to what it was formerly. Large quantities of preserved fresh meats are also placed under the charge of the surgeon for the use of the sick, and immediately the vessel arrives in port, fresh meat and vegetables are purchased for the crew, whatever they may cost. The scurvy is, I am happy to say, under these circumstances, almost unknown in the service.

I remain, Mr. Editor,
Your very obedient servant,
Richard Birtwhistle,
M. R. C. S. L. and Asst.-Surg. R.N.

Skipton Craven, Sept. 10, 1832.

LONDON HOSPITAL PUPILS AND MR. ANDREWS.

To the Editor of The Lancet.

Sir,—I am sorry to trouble you again, but I trust you will be able to give me a small space in your valuable publication for the insertion of this letter. Mr. Brown in your last week's Lancet says, that he heard no animadversion from Mr. Andrews disreputable to himself as an examiner of the College, or that could detract from the character of the gentleman referred to. To this I give a most positive denial, and I most faithfully pledge myself that my communication was correct, and that Mr. Brown knows it to be so, for in my presence he made some severe remarks upon Mr. Andrews' conduct. The gentleman who has been the cause of this correspondence, immediately after the occurrence stated the subject to me and three or four others; he then laboured under feelings which you, Sir, may better imagine than I can describe. Independently of these circumstances, it has been the common conversation with the medical officers attached to the hospital, who one and all say, that "having granted the diploma, it would have been better to have said nothing about the examination." So much for Mr. Brown's letter.

But, Sir, this is not a solitary case. About six months ago, a Mr. H—— passed the College, and on the following day Mr. Andrews said, that "he had made himself look like a fool at the College on the preceding night," thereby implying that his examination was anything but creditable to him. This gentleman left the hospital in disgust.

I shall close these remarks with asking you if it be consistent in an examiner to affix his name to a diploma in the evening, and in the morning to make a statement, that the individual who holds it is unworthy of possessing such a document. I remain, Sir, your obedient servant,

Junius.

September 11th, 1832.

MR. HUDSON'S SYRUP OF SARSÆ.

To the Editor of The Lancet.

Sir,—I have just read in The Lancet some remarks respecting the effect of the Concentrated Syrup of Sarsæ, prepared by Mr. Hudson, and that sold by the Worshipful Company of Apothecary Druggists, and I perfectly agree in the justice of your remark, as regards the efficacy of the one preparation over that of the other. The difference of expense is great, but the value of the material (Jamaica sarsæ), the very great care taken, and the length of time expended in the production of a superior and efficacious article, must be taken into account, and the "dear success" complained of, will not be found very alarming. But my eye was directed to the paragraph at the bottom of the page, respecting a percentage, and I felt satisfied that the Editor could not know the party, or he would not have inserted an insinuation so degrading, and yet so utterly groundless.

I have lived in Mr. Hudson's house as an assistant, and all who have had a like advantage must join me in vouching, that such a vile system is positively incompatible in one, whose conduct as a tradesman is most honourable, and whose private worth is the admiration, and ought to be the guide, of all who know him. Mr. Editor, I repeat that such conduct is contrary to Mr. Hudson's principles; and remain, Sir, your constant reader,

Juvenis.

NITRE

AS A THERAPEUTIC AGENT.

It is well known, experimentally, that dark and grumous blood is, by a minute portion of nitre, rendered florid,—of the appearance of arterial blood, and when thus impregnated, it is doubtless more stimulating to the solids. Muriate of soda is similar to it in effect, but a larger quantity of this salt is required. Nitrate of potash is much more powerfully antiseptic than this substance, as a very small proportion will preserve meat from putrefaction, and impart a fresh and lively red colour. Its diuretic power is also greater, especially when combined with opium; and it is remarkable, that when these two are used in combination, they produce a salivary discharge for a few hours, similar to mercury. If to this be added a few grains of calomel, it becomes a potent sudorific, or, rather, we might call it, a universal secerner.

Nitre is generally considered to be antiphlogistic, or of a cooling quality; but this idea is probably erroneous—indeed it is even palpably the reverse, which may be proved by any person who will take thirty or forty grains of this salt three times a day, by which the body becomes so much heated as to be in a semi-febrile state. At the same time I regard this salt as a most valuable remedy, both in inflammation and fever, whether adynamic, or depending on phlogistic diathesis; the refrigerant principle ascribed to it has probably been inferred from its effect upon the tongue, and the sensible degree of cold produced by its solution. It is one of the most useful remedies in typhus, but for which disease it has long since been proscribed, from its reputed sedative virtues, doubtless on the old maxim, that contraries are the cure of contraries, as though all diseases depended on excess or defect of phlogiston. Opium was formerly considered to be in its nature cold, but now the contrary opinion of its property is entertained. Conceits and theoretical views regarding the modus operandi of medicines, act in numerous instances as clogs rather than as auxiliaries to the healing art. In proof of this position, it is but necessary to consider that we can judge of the merits of any remedy only by observing the results of its employment in actual disease. Some there are, indeed, who perhaps vainly imagine, that man is capable of comprehending the innate and essential properties of matter, and that unless he can explain the nature and most recondite operations of any remedy, he ought not to adopt it in practice. Of such fancies, the reply of an American Indian may be taken as the true specimen of their character. Being asked why he gave such an herb or root for such a disease, the *savage* briefly replied, "Because it cures the disease."

The nitrate of potash is found greatly to moderate the excessive thirst which proves so distressing in cholera. Should ordinary remedies be rejected in this disorder, I have no hesitation in saying they ought to be promptly administered by glyster, in small form (as in four or five ounces of mucilaginous broth), so as to be retained. I have been surprised to find this application of remedial agents so much neglected in the treatment of this disorder, even in hospitals, and when most urgently indicated by the excessive vomiting and purging: how it is so neglected it is difficult to conceive.

J. Hancock, M.D.

CONTRACTILITY OF THE SPLEEN—VENOUS INJECTIONS.

Majendie in a course of lectures on experimental physiology delivered in 1828, in endeavouring to prove the contractility of the spleen, injected a quantity of tepid water into the jugular vein of a dog. He states, that the abdomen being opened, he observed the liver, kidneys, and *spleen*, increased in size, thicker, and more turgid than natural. The spleen, which was at first three and a half inches in length, became reduced to three inches, its breadth and thickness diminishing still more perceptibly in proportion as the blood was expelled by a slow contraction like that from elasticity. Now in some of the cases of death where venous injection had been previously employed, the existence of the phenomena alleged by Majendie to take place, might, I apprehend, were the attention directed to it, be determined. If confirmed, it would materially strengthen the hypothesis advanced by him as to the structure of the spleen, and at the same time elucidate, in some measure, the anomalous function of that organ. Doubtless the opportunities of some of your readers will enable them to furnish some interesting information with reference to the above point. C. N. *To the Editor.*

Nitrate of Iron.—Nitrate of iron may probably be very beneficial as a preventive, in removing slight diarrhœas, which are apt to terminate in cholera. I have advised a number of my patients, whose bowels are easily affected, to adopt the precaution of taking this medicine, and I believe all of them have now less tendency to diarrhœa.—*Mr. Ker.*

WESTMINSTER HOSPITAL.

ADIPOSE TUMOUR.—POLYPUS NARIUM.—FISTULA IN ANO.

Saturday, June 23, 1832. There was this morning a tolerably full attendance of pupils and visitors. The only surgeons who attended in the operation theatre were Mr. Lynn and Sir Anthony Carlisle. Rachel Bysson, ætat. 23, a pale leuco-phlegmatic girl, was brought in by the apothecary for the purpose of having a tumour excised by Mr. Lynn. The swelling was situated over the cartilaginous terminations of the ninth and tenth ribs, and was about the magnitude of a boy's fist. Mr. Lynn, though an octogenarian, made his incisions with the utmost steadiness and precision. A pupil who was assisting observed, during the progress of the operation, that the surgeon "*would soon arrive at the neck of the tumour.*" Sir Anthony replied, snappishly, "*Fatty tumours have no neck, Sir.*" Mr. Lynn—"Oh yes; most tumours have a point where they communicate with the adjacent substance by a blood-vessel, an absorbent, and a nerve, and that point is properly called a neck. In this case that point of junction is very obvious." The wound was closed by suture and strapping, and ultimately did well. The patient ascribes the origin of the tumour to a blow.

Sir Anthony Carlisle then called forward an apparently healthy young man about 30 years old, having polypus of the nose. He said, "The mucous membrane of the nose is liable to three kinds of polypi which grow from its vascular and not from its secerning surface: 1st. A vascular polypus which affects principally the young; 2d. An indolent or gelatinous, or rather albuminous, polypus, which chiefly attacks old subjects; and 3rd. A malignant polypus, to which the middle-aged and old are most, or perhaps exclusively, obnoxious." Sir Anthony then used the ordinary polypus-forceps, and twisted out several portions of the fungus which had a yellow colour, and fatty consistence, but in which blood-vessels were distinctly traceable. The polypus existed in both nostrils, and grew from the superior turbinated bones, and from the sides of the vomer. The operator remarked that the surgeons should always tear away a few of the marginal cells of the spongy bones when the polypus arose from them, as a proof of the perfect eradication of the disease. The patient had been subject to anorexia, indigestion, and torpid bowels, which had always been indicated by irritation of the nares. Mr. Lynn subsequently performed the operation for harelip on a child with fissured palate; and the operative business of the day was concluded, *as usual*, by Sir Anthony's cutting a patient for fistula in ano! The man had marked symptoms of pectoral disease. The fistula was complete, communicating with the rectum and with the external air.

FUNGOID TESTICLE.

June 30. This day, a man of the name of P. Cormack, ætat. 50, affected with paralysis, was brought into the theatre for the purpose of having a fungoid testicle examined, and, if necessary, cut out. The patient had been affected a few years before with syphilis, from which he was supposed to be cured without mercury. He had twice been affected with paralysis of right side, and each time the right testicle became swollen, inflamed, adherent to the integument, and suppurated. Dr. Roe, the patient's physician, was of opinion the disease was benignant, and might be cured by alterative topical applications, and suitable general treatment; but Mr. Lynn thought the disease radically malignant; the fungus might be thrown off, and the ulcer healed, but the good result would last for a season only, and the disease would break out again in a worse form. He was induced to rest in this opinion from the general appearance of the patient, and from the *feel* of the tumour, which had a characteristic hardness of texture, and a *lobular* form. In this determination of operating Mr. Lynn was overruled, and the patient was managed according to the suggestion of his physician. Tonics were exhibited internally, and a strong ointment of hydriod. of potass was applied topically. Under this treatment the ulcer has rapidly improved in appearance, and presents a clean granulating surface, but the patient's general health is now (1st of September) not so good; nocturnal pains infest him, and a corona veneris is formed in the anterior part of the calvarium. Frequent doses of mercury are given with a view to produce ptyalism, and decoction of sarsaparilla ad libitum.

Mr. White united the harelip of a child two years old, and which had been previously operated upon without success in the ordinary manner. The wound healed with but little suppuration.

BOUGIE IN THE BLADDER.

July 7.—On entering the operating theatre to-day, Mr. Lynn inquired, "Which of you has read the LANCET of this week? I see Brodie has been figuring away at St. George's about a case in which a flexible catheter was broken and left in the bladder, and for which he performed the operation of lithotomy. He says he never heard of a case like it, and nothing analogous to it except in the instance of a gardener affected with stone who passed a flower-stalk along the urethra in order to push the stone away from the neck of the bladder, where its presence, you know, occasions so much pain at the glans and along the whole passage. In this the fellow was successful for some time, but at last he was caught, the flower-stalk broke, and remained in the bladder. Now Brodie is a clever fellow, a *very* clever fellow; but if he had asked me I could have told him of a case I had, perhaps before he was born, and in this very hospital. In the year 1786, a young fellow was a patient of this charity under the care of Mr. Justamond. He had been in the habit, according to the instructions of his medical adviser, of passing daily a bougie for himself, I believe for stricture. On one occasion it slipt from his fingers into the urethra, and he could not by any means get hold of it again; the more he poked and fumbled, the further the bougie slipped in. A surgeon was called and attempted to extract it, but without success, and the bougie escaped beyond reach. Pain and irritation were excited in the bladder by the presence of the foreign body, and the patient was consequently brought into this hospital. Some discrepancy of opinion existed amongst the surgeons as to what ought to be done. Justamond thought one thing, Watson another, and I a third. It was resolved finally, however, that I should be permitted to perform the lateral operation of lithotomy. Accordingly I performed the operation in the usual way, but when I introduced the forceps, I could feel nothing in the grasp, and was a little discomfited by the circumstance, but on withdrawing them I found they had fast hold of the bougie coiled up into four folds. The wound healed within a week, and the patient did well in all respects."

LONDON HOSPITAL.

PHAGEDENIC ULCER OF THE GLANS PENIS.

T. J., ætat. 27, a printer, admitted April 13th, stated that about seven weeks previous to his applying here, and a short time after coition, he perceived a small pimple on the glans penis, and a few days subsequently a discharge from the urethra, with scalding, on passing his urine. The pimple degenerated into a sore as large as a pea, with elevated hardened edges; he took pills which made his mouth sore, and in less than three weeks from the first appearance of the chancre it healed, but in the course of a very few days after healing, it again ulcerated, and had been spreading, up to the time of his admission. The discharge from the urethra had entirely ceased.

April 13. The ulcer occupies the right side of the glans, with the adjoining base of the prepuce. It forms a large and deep hole, being about three quarters of an inch in length, and about a quarter of an inch wide and deep, having destroyed the substance of the glans penis down to the corpus cavernosum, of which the obtuse anterior extremity is seen at the bottom. It is bounded towards the penis by the attachment of the prepuce, into which the ulceration has not yet extended. The surface and margin of the sore have a whitish appearance, and a phagedenic character, but there are some red granulations to be seen at the end of the corpus cavernosum. There is a good deal of discharge, of a reddish purulent character. The prepuce is a good deal swollen.

He was ordered to apply eight leeches to the prepuce, and take a dose of house-medicine, to observe the recumbent posture, support the penis, and apply a solution of the chloride of lime. To take an anodyne at night.

Mr. Scott considering he did not improve so much as he could wish, directed him to apply a bread-and-water poultice to the part, and take a drachm of the extract of sarsaparilla in the decoction of the same, three times a day. On the adoption of this plan of treatment, the character of the ulcer immediately improved, and healed regularly, but very slowly; he remained in the hospital some time after the ulcer had healed up, owing to the copper-coloured eruption having appeared on the arms and body, but without any sore throat; he was ordered to rub in night and morning, but this only slightly affected his mouth, until he took the pil. hydr.; he was kept for nearly six weeks under the influence of mercury, and has left the hospital cured.

TO CORRESPONDENTS.

An answer in the affirmative may be returned to each of *H.*'s questions.

A. M. M. B. understands his subject, but his paper does not contain anything which has not already been said in this journal.

The paper of *Mr. J. B.* on "Spasmodic Cholera" requires more revision for publication than we will venture to give it.

There is no evidence before us that strychnine would be beneficial beyond that furnished by *Mr. Mason* himself which is not ample enough.

The papers of *Mr. Smethurst* and *Mr. Busvine* do not demand publication.

We hope there will be many such electors as our friend at Houghton-le-Spring.

The request of *An Injured Party* shall be complied with. We are glad to hear of the facts contained in his last.

A Medical Student. New regulations *have* been issued by the Apothecaries Company, and we will reply to the questions of our correspondent in some general remarks which shall appear in our ensuing number.

THE LANCET.

VOL. II.] LONDON, SATURDAY, SEPTEMBER 22. [1831-32.

INJURY OF THE PENIS,

RETENTION OF URINE, AND, FINALLY,

AMPUTATION;

With Remarks on the Propriety of leaving a Bougie in the Urethra after that Operation.

By WM. WILLIAMS, *Esq., M.R.C.S.L., Llandovery.*

ON the 22nd of July last, a farmer brought his son, a lad about eleven years of age, to me, labouring under retention of urine, occasioned by an accident which happened to the penis, of which he gave the following short history. He stated that, about three months ago, a brood mare bit the boy's penis, through his trowsers, just behind the glans; though no solution of continuity appeared at the time, yet shortly after the occurrence mortification took place, so that when the slough separated, the glans was attached to the body of the penis, only by a small neck at the under part, and the urine passed through the gangrenous wound. The dressing of the sore was entrusted to an old woman till it healed and cicatrised.

The penis now was perfectly free from any inflammation, and presented no great deformity; but the urine escaped, *guttatim*, through an opening in the cicatrix, hardly large enough to admit the point of a bristle, situated just behind the corona glandis, on the dorsum penis, and concealed by the prepuce. The bladder was very considerably distended, and the hypogastrium very tender and painful. That part of the urethra which lies in the glans had become closed during the inflammation which succeeded the injury, and was now perfectly impervious, forming a stricture of about three-fourths of an inch in extent. The state of the bladder required that its contents should be evacuated as soon as possible. How was this to be accomplished? A choice of three methods presented themselves. 1st. To make good the natural passage with the caustic bougie; 2ndly. To dilate the narrow opening in the cicatrix; or, 3rdly. To amputate the penis just behind where the injury had been received.

The first method appeared objectionable on the score of its tediousness; and as it would take some days before it could be effected, by which the bladder would suffer from the accumulation of urine, it was thought advisable to relinquish it.

The result of the adoption of the second plan seemed very questionable, owing to the callosity of the cicatrix, through which the narrow opening passed; and its forming rather an acute angle with the natural urethra, was likewise an additional objection.

The third method, that of amputation, would be followed by effectual and immediate relief to the bladder, and severe as it may appear, was upon the whole judged the most eligible; but being anxious to have my opinion corroborated, it was deemed expedient to consult Dr. Bowen of Carmarthen on the case, who, to a note which I had sent by the lad wherein I had detailed my intended mode of proceeding, returned the following answer:—

> "Dear Sir,—I have examined the boy, and my opinion so entirely agrees with yours, that I will only remark, that the operation of amputation cannot be performed too soon, as I fear the bladder may be seriously injured from constant distention."

The operation was soon after performed in the following manner:—

The root of the penis being held by an assistant, the glans and prepuce were grasped between the fingers and thumb of the left hand, and gently drawn forward, so as to elongate the penis; then, with a straight-bladed bistoury, the whole of the parts that constitute the organ were extirpated at one stroke, directing the incision from below upwards.

The urine was immediately evacuated in a full stream, and the force with which it was propelled, showed that the bladder had not lost its power of action. After making pressure for a short time on the bleeding vessels, the flow of blood was arrested without having recourse to ligatures, and the wound was dressed simply with dry lint. I found the next morning that a slight hemorrhage had taken place in the night, but it had now completely stopped; the urine freely passed, and a poultice was applied to the wound, which had now become considerably inflamed and swelled. A slight hemorrhage returned two or three times, generally in the night, but the inflammation and swelling soon subsided, the bleeding disappeared, and the surface began to granulate.

With regard to the necessity of introducing a bougie or tube to prevent contraction of the orifice of the urethra after the operation, in ordinary cases, surgeons do not appear to be unanimous. The late experienced Mr. Hey, of Leeds, advocates the *occasional* introduction of a bougie; vide "Practical Observations in Surgery," chap. xiv, on cancer of the penis. Sir A. Cooper says—"When the surface begins to granulate, a piece of bougie, two inches long, is to be worn *constantly* in the urethra, to prevent its contraction, otherwise it gradually closes as the wound heals, and produces retention of urine." Vide his lectures by Tyrrell,[*] vol. 2, p. 324. Mr. Lawrence, no mean authority, in his 75th lecture, as reported in The Lancet, vol. 2, p. 771, advances a different opinion. These are his words:—"I have never found it necessary in amputation of the penis to do that which is recommended by most systematic writers; that is, to introduce a bougie or catheter to keep the remaining portion of the urethra open. The truth is (continues Mr. Lawrence), that this proves an additional irritation; the patient makes water very well after the operation, and the introduction of such a substance creates an uneasiness which is not at all called for, so that you may dispense with the bougie with great safety." This I considered a favourable opportunity of ascertaining the merits of the treatment inculcated by either of the above eminent surgeons, whose opinions are likely to influence the practice of the rising part of the profession, and not liking to give my young patient any unnecessary pain, I resolved to adopt Mr. Lawrence's practice.

The stump was simply dressed with spermaceti ointment, which was changed each time it became necessary to pass urine; and as the boy seemed to long to see his parents, who lived about four or five miles in the country, he was allowed to go home. When he returned home, however, at the end of about nine days, I found the wound nearly healed, and the orifice of the urethra so contracted, that the point of a very small probe could hardly be introduced, and had the above treatment been persisted in a few days longer, I am convinced that complete retention would have taken place. With much difficulty a very small bougie was introduced; after this had been kept constantly in the orifice till the next time of making water, it was replaced by a larger bougie, and in four days the urethra admitted a common-sized bougie. At the end of a fortnight this was discontinued for 24 hours; at the close of which short time the urethra was again found very considerably contracted; the bougie was again recurred to, and it has been constantly retained till the 12th of September, a period of nearly a month since its first introduction.

From the following passage in the lecture above quoted of Mr. Lawrence,—"The operation is very simple; in the first place, you cut circularly round the integuments of the penis, and when those have retracted as far as possible, you cut through the body of the penis; the integuments will then be loose enough to admit of their being brought together over the stump of the penis,"—it would appear, that he thinks it advisable to save as much of the integuments as possible. As far as the present case allows me to judge, I think the preservation of integuments in amputation of the penis, not only unnecessary, but, as Sir A. Cooper observes, "a great evil, by preventing a free escape of the urine." The late Mr. Pearson was likewise of this opinion. Vide his "Practical Observations on Cancers," page 103. It is due to Mr. Lawrence further to observe, that Mr. Pearson, in the same work, disapproves introducing cannulæ in this operation, as painful and unnecessary.

It may be perceived, that I attempted to preserve no integuments in the above case; yet they appear to overlap the extremity of the stump so redundantly as not to offer any impediment to the complete erection of the penis in future.

Llandovery, Sept. 13th, 1832.

EXTRACTION OF CALCULI

FROM THE BLADDER, BY

WEISS'S URETHRA DILATOR.

To the Editor of The Lancet.

Sir,—I beg, through the medium of your valuable publication, to add my testimony to that of Mr. Brougham, surgeon, of Falmouth, whose communication you published in July last, in favour of Weiss's urethra dilator, in the extraction of calculi from the female bladder.

Mary Thayer, ætat. 20, of chlorotic habit, consulted me in Feb. 1830, with symptoms characterising stone in the bladder, for which I was induced to prescribe various alkaline medicines, but with temporary relief; the symptoms, after two weeks duration, returned in a most aggravated form; therefore, seeing no other alternative, I introduced a sound, and proved the diagnosis to be correct. Having explained the nature of her case, the patient felt anxious to have relief afforded as speedily as possible, I therefore proposed an operation by Weiss's urethra dilator, to which she readily consented, and which accordingly took place on the following morning. The patient being placed nearly in the usual position for lithotomy, I introduced the dilator, and continued gradually to expand the blades, until they were separated to their greatest extent, about an inch and a half, which occupied about forty minutes. Having allowed the dilator to remain in the urethra twenty minutes, I withdrew it, when, to my great satisfaction, the natural efforts of the bladder expelled a calculus of the size of a walnut, without materially increasing the severity of the patient's sufferings. On being placed in bed, forty drops of *tinct. opii* in *camph. mist.* ℥iss were given. On visiting her next morning, I found she had passed a comfortable night, having had several hours refreshing sleep; felt easy; tongue clean; pulse 87; skin moist; bowels not open since the operation. Ordered

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[*] Copied verbatim from The Lancet.—Ed. L.

Ol. ricini ℥vj;
Liq. potassæ gtt. xxx;
Aq. distill. ℥j.

In the evening her bowels were comfortably relieved; passed her urine several times with less pain than before the operation.

From this time her health gradually improved, and for three weeks everything went on in an uninterrupted way towards recovery, when she began to experience pain in the region of the left kidney, increased by pressure and motion, difficulty of making water, pain and numbness of the leg and thigh—in short, every symptom denoting stone in the bladder had again appeared. To enumerate them would be merely a work of supererogation. Suffice it to say they were of an aggravated form. The patient expressing her belief that stone had again passed from the kidney into the bladder, and finding its natural efforts insufficient for its expulsion, I, at her earnest desire, had recourse to the operation in the manner above detailed, which was repeated three different times, at intervals varying according to the urgency of symptoms; the bladder, at each time after the operation, expelled two, and sometimes three calculi, from the size of a walnut to that of a hazel nut. I have not since had occasion to use the dilator, and feel pleasure in saying the patient has still the power of retaining her urine as well as before the operation, consequently, it is reasonable to suppose that Weiss's urethra dilator will, I should say, in every case, supersede the operation by the knife in calculi of the female bladder. I have the honour to be, Sir, your obedient servant,

Joseph Lovegrove, M.R.C.S.
Horsham, Sept. 13, 1832.

ON THE

ANTISEPTIC PROPERTIES

OF THE

HYDROCHLORATE OF TIN.

By Dr. Tauffier, *of Strasburg.*

Alcohol, which has hitherto been generally used in the preservation of animal substances, offers many inconveniences, which, in some instances, may render its antiputrid properties nearly useless. Without speaking of the high price of this liquid, which renders its employment very expensive, especially in putting up of large anatomical specimens, it would be sufficient to point out the almost disorganising action which it exercises upon the animal tissues, to demonstrate the want of a menstruum which does not present these disadvantages.

Various substances have already been proposed to replace the alcohol, but none have as yet been found to merit a very marked preference.

The substance of culinary salt, of salt of nitre, of alum, of sulphate of zinc, which have been recommended as retarding putrefaction, oppose but a feeble barrier to the disorganization of the parts.

Corrosive sublimate preserves, it is true, animal substances from putrid decomposition, but it hardens and discolours these matters, and finally renders them indistinct by a chemical action, which it exercises on them. Many examples have also proved, that the employment of this mercurial salt is not without danger to the operator.

The persulphate of iron in solution has also been recommended as an antiseptic liquor, but it has the inconvenience of covering the parts to be preserved, with a yellow crust of sub-sulphate and oxide of iron. To establish this fact, I used a solution of the trito-sulphate of iron, which had for three hours been exposed to the contact of the air. Some muscular flesh, which I immersed in this liquid, resisted putrefaction for eleven months; but it became covered with a coating of sub-sulphate of iron which prevents the distinguishing of the organic structure. The liquor becomes decolorated, and in lieu of the trito-sulphate of iron, it contains the proto-sulphate only.

The aqueous solution of sulphurous acid preserves much better the animal tissues of which we have just spoken, at least it does not quickly alter the natural texture of the parts. I have been enabled to preserve, till the present moment, almost without alteration, a piece of muscle, which I plunged ten months ago into water acidulated with sulphurous acid. The solution remains transparent, the fleshy fibres have become much more distinct, preserving their red colour; but after the fifth month the tendinous parts and the interstitial cellular tissue began to be transformed into a species of gelatinous and transparent "*bouillie*," having the appearance of white of egg.

Dr. John Davy, who first availed himself of this acid for the preservation of cabinet specimens, confined himself to its employment by immersion. I was anxious to assure myself if this sulphurous solution acted with the same efficacy upon fleshy bodies merely moistened by it. I have with this view tried many experiments at the Amphitheatre of Anatomy of the Faculty of Medicine, and in the presence of M. Lauth, superintendent of the anatomical schools. An arm which had been dissected for the bloodvessels, presented all the signs of putrefaction, as it appertained to a body twenty days dead. A glutinous matter which oozed out incessantly on the surface of the muscles rendered their dissection more and more difficult. Half of the arm was moistened slightly with water acidulated with sulphurous acid, and then exposed to a temperature of twelve degrees. The next day the moistened portion was perfectly smooth, a little red, and nearly in the same state as when fresh, whilst the adjacent parts were black, covered with a viscous matter, and throwing out an infectious odour. A *gastrocnemius* muscle was enveloped in tow, moistened in sulphurous water, and exposed to a constant temperature of twelve degrees: at the end of four weeks it was as fresh as the first day. The sulphurous acid appears then more particularly useful in the cases where it is proposed to arrest for a time only the progress of putrefaction, and in this respect its use becomes infinitely more important to anatomists in facilitating the dissection of preparations, which require minute work. But it appears to be less proper than alcohol for the general preservation of anatomical specimens, because of the kind of maceration to which this fluid subjects the parts immersed in it.

Amongst the different saline solutions, alkaline or acid, of which I have considered the anti-putrid tendencies, that of hydrochlorate of tin appears alone to unite all the conditions which ought to give it a preference over alcohol in the preserving of animal matter, at least in the greatest number of cases. In June, 1831, I placed in a weak solution of this metallic salt, a muscle with cellular tissue filled with fat. These parts retained all their freshness in consequence; those which were coloured by the red blood assumed indeed a slightly brown tint, but in other respects they did not show the slightest alteration, either in consistency or appearance. I have shown these specimens to many anatomists, who have viewed with astonishment their perfect integrity after so considerable a lapse of time.

I have subjected to essays of this kind different sorts of animal tissue, such as the mucous and serous membranes of the lungs, of the liver, spleen, &c. All these substances remained without any alteration for five weeks in a solution of one part of deuto-hydrochlorate of tin in *thirty parts* of water. Another piece of flesh, chiefly composed of muscular parts, which had been immersed some time in a solution of salt of tin, was left in an earthen pan after having been passed through water. At the end of ten days it had not shown the slightest sign of alteration, although it was still completely impregnated with moisture: exposed to the air, it dried without any sort of decomposition. The deuto-chloride of tin then may be employed with success, whether in the conservation of fresh pieces, or to favour their dessication, according to the end proposed. To produce this effect, it is necessary to use a solution of *one part* of this salt in *twenty parts* of water, acidified by hydrochloric acid. The salt of tin of the manufactories should not be employed for this purpose, because the solution is disturbed by the air, and allows a deposition of sub-deuto-hydrochlorate, which is precipitated over the parts. It is requsite to employ a deuto-hydrochlorate which does not contain the proto-chlorate, and which may easily be procured by treating "tin in grains" with *aqua regia*.

As the calcareous substances which enter into the composition of the bones act strongly upon the hydrochlorate of tin, it becomes necessary to leave for some time in the acidulated water such anatomical pieces as have been attached to them, in order to dissolve the calcareous salts; they may afterwards be placed without inconvenience in the metallic solution.

ON THE ADMINISTRATION OF
CROTON OIL
IN
MALIGNANT CHOLERA.*

By T. Ogier Ward, *M.B.*, *Wolverhampton.*

To the Central Board of Health.

Gentlemen,—When I mentioned to the Central Board that we had daily proofs of the efficacy of the croton oil, and of Dr. Stevens' saline powders, I had only tried the oil in a few cases; but since then I have used it very extensively, and I have had no reason to alter my opinion of its utility. I was induced to make trial of this remedy from Dr. Teggart's account of its effects upon himself, and from having observed in the various treatises, &c., which have been published upon the cholera, that the appearance of bile in the evacuations has been hailed as the harbinger of success in the treatment, almost universally, by the Indian practitioners, and pretty generally by those of this country. Now, in our present state of ignorance of the precise nature and seat of this disease, I conceive that any medicine or plan of treatment that has the power of restoring the secretion of bile into the intestines, though this, being merely the renewal of one symptom, may be considered by many as a secondary object to getting rid of the whole disease, is nevertheless deserving of the greatest attention. That croton oil is a medicine possessed of this power, is, I think, satisfactorily proved by Dr. Teggart's case, and by comparison with the effects of other remedies and plans of treatment, which latter may be divided into three classes, as distinguished by their immediate objects or designs, viz.—1st, to produce reaction; 2nd, to change the nature of the blood; 3rd, to excite the liver.

Under the first head I may class stimulants, emetics, venesection, cold affusion, hot baths of air or water, counter-irritants, &c., none of which can claim the power of restoring the flow of bile, though this may result as a consequence of the cessation of the disease. The same may be said of the second class, or those remedies which are supposed to change the state of the blood, viz. the various forms of the saline treatment. As to the third class, whose object is to excite the liver to act, I mean the various preparations of mercury, &c., I cannot say that I have any confidence in their powers during the state of collapse.

Now, in favour of the croton oil I may state, that in almost every case in which I have given it, whether the collapse was perfect or not, its exhibition was followed, after the space of from three to eight hours, by dark-green evacuations, either upwards or downwards, or both ways, but more frequently downwards. And that this effect is produced by the remedy is clear, I think, from none of the advocates of other plans of treatment having claimed for them any such sudden operation upon the liver or gall ducts.

Perhaps this is the place to give an account of my usual plan of treatment, that the Central Board may judge what degree of confidence may be placed in this medicine, or how much of the success or failure is owing to other means which I employ in combination with the croton oil.

In my district, in which the people (colliers) are very poor, ignorant, and stupid, or self-willed, we are seldom called into a case of cholera till some degree of collapse has come on. If the patient be a child, in which case bleeding is impracticable, and mustard and salt emetics cannot be given, I mix up two drops of the oil with some sugar and an ounce of water, and direct a teaspoonful to be taken every ten minutes or quarter of an hour, till the stools turn green or yellow. If bleeding or emetics be practicable I employ both, and the latter as an adjuvant to the former. I always bleed to faintness, or till the blood becomes florid. If the patient faint, so much the better, as the body becomes covered with perspiration; and if he be put into a warm bed, reaction soon ensues. As soon as he is in bed I give two drops, if a boy, or three if an adult, of the oil, upon sugar, which generally allays the vomiting and purging for a time, and prevents the exposure to the cold, attendant upon rising to the chamber-pot. By these means, with hot bricks and frictions, I seldom fail of producing reaction—at least for a time. When the vomiting and purging recur, which they are pretty sure to do, if the evacuations are not changed, I either repeat the oil, or give calomel and opium to assist its action. In every case I give Dr. Stevens' powders as the common drink, to the exclusion, if possible, of other liquids; but from the great extent of my district, and the stupidity of the people, I am only sure those medicines are given which I see the patients take. Children generally refuse the saline powders, and everything else but the oil; it is therefore in them that its effects are most decidedly seen, and I consider what I now state as most particularly worthy of the attention of the Board,—that of fifteen cases of children attacked with cholera in every stage, from the age of five months to twelve years, whom I have treated with the oil, and with that alone, in general, I have lost but one (and he, a boy of eight years, died comatose with secondary fever), while I am told that every child attacked by the disease in Liverpool has died; and upon reference to the returns, I find the same to be the case here, except amongst those treated with the croton oil.

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* Communicated by the Central Board.

With adults I cannot say my success has been so striking, for though reaction be produced, with the desired change of colour in the stools, yet some of the patients fall again into collapse, the stools remaining green, and others have severe secondary fever, attended with a bilious diarrhœa to its termination, whether favourable or otherwise; and two patients, both women, in whom the oil had produced most decidedly good effects, died after a week's constipation, which not croton oil, nor even mechanical means, could overcome, till approaching death relaxed the intestines or sphincters. I have not seen the oil fail in producing what may be called its specific effects in more than five or six instances; nor have I had occasion to give more than three doses for this purpose in any case; for where it has failed, the patient has never completely rallied from the collapse. Neither do I consider that it produces the secondary fever, for many of the patients, both adults and children, were collapsed one day and convalescent the next.

The only inconvenience attending the use of the croton oil that I have observed is, that in adults to whom I give it upon sugar, it sometimes produces soreness of the mouth and fauces, so as to interfere with the use of food or medicine, but I purpose to remedy this, by giving it with castor oil, or as a draught.

I attribute the deaths of many of my patients in the secondary fever to their poverty not admitting their having proper attention paid them as to the regular administration of food or medicines, besides that many were neglected by their friends, through fear of contagion.

The boy above mentioned, who died comatose, had secondary fever, with bilious diarrhœa, for which, as it was late at night, and two miles from the town, I gave him a pill of calom. gr. iv, opii gr. i, divided into three pills, to be taken during the night if the purging did not cease. He took all three, which stopped the diarrhœa, but at four p.m. the next day he went to sleep, and never awoke till his death, twenty-four hours afterwards. This case, together with observing that opium has no effect whatever upon the disease while in the form of cholera, according to Dr. Macann's definition, has induced me to relinquish its use in that disease, though it is highly useful in the subsequent diarrhœa.

I am sorry I have nothing to state to the Central Board with respect to the saline treatment, from my not having the cases sufficiently under my own observation to be certain that the powders are properly administered; but other district surgeons who have such means, place the greatest reliance upon Dr. Stevens's plan, and I understand that both the saline and croton oil treatments, and a combination of both, are becoming very general in this town and neighbourhood.

With respect to the general health of the town and neighbourhood, there is in some parts, near stagnant waters, a great deal of common fever, and where the cholera prevails, a slight fever, with or without diarrhœa, and attended with pains in the head, stomach, and bowels, is very common, independent of the diarrhœa, of which the return of cases is so numerous in our daily report to the Central Board.

Begging the Central Board will excuse my delay in answering their letter, which has been caused solely by want of time, and from a desire to make that answer as complete as possible. I remain, Gentlemen,

Your obedient servant,

T. Ogier Ward, M.B.

Wolverhampton, Sept. 9th, 1832.

ADMINISTRATION OF
COLD WATER
IN THE COLLAPSE STAGE OF
MALIGNANT CHOLERA.*

To the Medical Gentlemen of the Central Board of Health.

Gentlemen,—The following memorial was nearly completed, when the circular, requesting that the particulars of any plan of treatment which appeared to have been unusually successful in the Asiatic cholera, might be transmitted to the Central Board of Health, was put into my hands. It is intended to record the circumstances, favourable and unfavourable, which have attended the cold-water treatment, to state more fully the facts which appear most deserving of attention in the prosecution of that practice, and to suggest certain modifications, by which its success in some particular forms of the disease may probably be enhanced. As such it is, I conceive, strictly consistent with the object of the circular mentioned, and is now transmitted to the Board. I am, Gentlemen, your obedient humble servant,

Hardwicke Shute.

Gloucester, Sept. 15, 1832.

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* Communicated by the Central Board.

Memorial, No. 3.

From the manner in which my communications to the Board of Health, relative to the cold-water treatment of the second and third stages of the Asiatic cholera, have been received by the profession, I can have no doubt that the result of that practice in other hands, will at no distant period be made known. This great object of my former memorials being fully effected, I shall, on the present occasion, confine myself to some remarks which appear calculated to ensure a more strict adherence to the minutiæ of the plan recommended, and consequently a more satisfactory trial of its effects, and also to throw some light ultimately upon the nature of that formidable disease, with which, as professional men, we are now called upon to contend.

To those who have not had an opportunity of witnessing, and perhaps to those most conversant with the disease, it may appear rather extraordinary to speak of patients continuing in a state of collapse for 24, 36, and 48 hours, as a matter of frequent occurrence, and doubts may be entertained as to the identity of the collapse described by others, and alluded to in my former memorials; it therefore becomes necessary to state, that by the term collapse, I mean to express, the cadaverous expression and remarkable sinking of the features, which so strongly characterize the disease, accompanied by a leaden hue of the face and extremities, shrivelling of the skin, a pulse scarcely, and in most instances not at all, perceptible, and a corresponding state of the animal heat. I have seen patients continue in this state for 24, 36, 48, and even 72 hours. From a collapse of 24 and 36 hours many have recovered, some after a collapse of 48 hours, and even after a longer period, but that period has not in my own experience extended to 72 hours. Relying upon these facts, and comparing them with what I had previously witnessed, I hazarded the assertion, that in all cases of collapse the progress towards death is in a remarkable manner retarded by the cold-water treatment; and I shall take this opportunity of repeating, with the view of inculcating the importance of attending to the advice previously given, that the practitioner should be satisfied, and make not the least alteration in the plan of treatment recommended, so long as his patient is merely not getting worse. This fact, as pointing out the compatibility of ultimate recovery, with a prolonged state of collapse, may suggest to those who are pursuing a different plan of treatment, many difficulties which have arisen from an over anxiety to restore the circulation as soon as possible, and a more temperate view of the disease, by which those difficulties may be obviated. Speaking from my own experience, I should say that the most favourable and satisfactory cases of recovery have been those in which the pulse was not restored in less than 12, or 24 hours, and then as gradually as possible. I may also, upon this occasion, urge the necessity of a strict adherence, not only to the plan but the principle of the treatment—the principle of never administering any kind of stimulant which, in degree or extent, exceeds the impaired vital energy of the system. If my opinion on this subject be correct (and unsupported by the accumulated evidence of others, it does not profess to be more than an opinion)—if, I say, the principle of treatment be correct, no practitioner can, in future, witness the death of his patient after the free administration of brandy, or other stimulant in an advanced stage of collapse, and feel quite satisfied in his own mind, that he has not infringed (we mean unintentionally of course) the sixth commandment.

In my second memorial I stated, that when the irritability of the stomach is excessive or impaired, the water taken with eagerness almost immediately rejected, and then again and frequently desired by the patient, reaction is established in a very great proportion of the cases; and I have now to add, that my subsequent experience, which has extended from 48 to double that number of cases, enables me to repeat the statement with increased confidence; and also the importance of not checking, by opiates or similar medicines, the vomiting, which, according to my views of the disease and its treatment, has a conservative tendency. In many instances reaction has been established, when it was necessary to rouse the patient from a state of apparent insensibility, and offer the water frequently and repeatedly, when the water was taken, if not reluctantly, without any evident satisfaction, and therefore the treatment is not at all confined to the former cases; but I felt myself called upon, in my second memorial, to direct the attention of the profession to those cases more particularly, in which I felt assured that the cold-water treatment would lead to the most striking effects, and be attended with the same beneficial results as I had witnessed in my own practice, leaving it to others to ascertain how far the treatment might be applicable to a different form of the disease.

I also stated, that when the natural powers of the constitution are not unusually weak from extremes of age, or other causes (and of these causes previous habits of intoxication and inadequate support are the most prominent), such reaction will be followed by an early convalescence without fever, or the assistance of any kind of medicine. This statement, which has also been confirmed by my subsequent experience, is in itself an acknowledgment that recovery has not always followed reaction; and I shall now state some of the difficulties with which, in the less favourable cases, the practitioner will have to contend, and the reflections to which these cases have given rise, it being distinctly understood that I rely upon the cold water alone in all cases as the agent by which reaction is to be established, and in the more favourable cases as the sole agent by which convalescence is ultimately established.

In the most favourable cases, reaction is accompanied by an improved state of the alvine excretions, a cessation of vomiting and purging, a disposition to sleep, a restoration of the urinary secretions, and an uninterrupted progress towards a healthy state of the constitution. In others the return to health, after the pulse has been restored, is not progressive, and the alvine excretions will, upon examination, be found of an unhealthy character, and sometimes partaking, more or less, of the morbid state so peculiar to the disease. If the bowels are costive (by no means an unusual circumstance), the practitioner will seldom be wrong in suspecting a diseased state of the excretions. I have usually given ten grains of the *pil. hydrarg.*, or of the *hydrarg. c. creta*, with or without castor oil, according to circumstances, and the advance towards convalescence has been subsequently uninterrupted. The patient, however, is most commonly in a state to which the ordinary principles of medical practice are applicable, and therefore the selection of the medicines employed may be left to the judgment of the practitioner, but with the precaution of avoiding such doses as may be too powerful in their operation. The necessity of this precaution arises from the singular fact, and the fact must never be lost sight of, that the constitutional powers seem to be more under the influence of debilitating causes after the pulse has been restored, than it was during the collapse. The same, or a minor degree of muscular exertion, which was apparently innocuous during the collapse, has, in three instances, brought on a relapse, and a relapse is, I am sorry to add, almost always fatal. If purging continue to an immoderate extent, the administration of one grain of the argentum nitratum has proved highly beneficial. In some instances the vomiting has been obstinate after reaction has been established, but this has generally happened when, at the commencement of the collapse, there was no vomiting at all. In more than one instance the urinary secretion was not restored, but my experience hitherto does not enable me to offer any particular observation upon these cases.

The next circumstance which I deem worthy of particular notice, is a tendency to local congestion, and chiefly of the brain. Vascular congestion of the conjunctiva is a frequent occurrence, but has not in itself appeared to require any particular treatment. The case, however, is sometimes otherwise, and in infants and young children more particularly, such congestion being attended with symptoms of pressure upon the brain. As connected with this subject I may also mention the occurrence of opisthotonos in two infants and one female.

This tendency to congestion, after the restoration of the pulse, naturally suggests the question, whether bleeding would in any way assist the cold-water treatment, and at what period of the disease, if any, it should be adopted. From the present state of the disease in Gloucester (there not being a single case in the hospital), there is little probability of my being able to determine the point, and therefore I must throw myself on the experience of others, merely suggesting that the period of the complaint I should myself select for the experiment, would be that when the cold-water treatment had been persevered in long enough to justify the presumption that the fluidity of the blood had in some degree been restored. In one case a small quantity of blood, which was at first dark and venous, and in half an hour afterwards more florid than usual, was taken at this period of the disease. The pulse was restored at an earlier period than I had observed in other cases, but in the course of the night the patient became restless and irritable, was allowed to get out of bed frequently, had a relapse, and died, confirming, as it appeared to me, my former impression, that a very gradual restoration of the pulse was an essential part of the cold-water treatment. I may here mention that in two cases this restlessness and irritability amounted, if not to maniacal aberration, to maniacal impatience and obstinacy, not very different perhaps to the state of mind which is said to occur in extreme exhaustion from exposure to excessive cold or starvation.

The occurrence of opisthotonos in the cases mentioned, naturally suggests the idea that the cold-water treatment might under certain circumstances be rendered more efficacious by the application of stimulants to the spinal canal. The cases recorded by Mr. Whiting are, in this point of view, highly interesting, but when the question is asked how far the application of blisters to the whole course of the spine may be regarded as essential to the cold-water treatment, I can have no hesitation in saying that it is not, inasmuch as the blistering plasters without the water are wholly inefficacious, whilst the cold water is, in a great majority of the cases, a powerful and efficient agent without their assistance. From the state of the circulation on the surface during the collapse, the effect of the blisters must at that time, I conceive, be trifling and unimportant, but I think it highly probable that in some cases, in infants and children perhaps more particularly, their operation, when the circulation begins to return to the surface, may prove extremely beneficial, but were they had recourse to in all cases, and considered as constituting an important part of the cold-water treatment, consecutive fever would, I fear, be a much more frequent occurrence.

I shall close this part of my subject by stating briefly two general principles which ought to regulate our practice after the pulse and animal heat have been restored, the patient must be considered as having passed from a state of depression to that of exhaustion, and the fundamental principle of not allowing any kind of stimulant which is disproportioned to that state, must be strictly adhered to and enforced.

This candid statement of the difficulties with which, in the less favourable cases, I have had to contend, may possibly convey the idea that my confidence in the practice is not what it was, and that farther experience has not confirmed my former statement. It is quite sufficient to say that such is not my own impression, or the impression which this statement, if read with attention, and due allowance be made for the number of patients in which a practice directly opposed to my own principles had been pursued previous to their admission into the hospital, ought to convey to the minds of others. It is wholly unnecessary to say more upon this subject, because the value of the treatment recommended, must obviously be determined by the experience of others, and not by the representations of an individual. That experience, as far as it has hitherto come to my knowledge, is greatly in favour of the practice.

There is one circumstance connected with the pathology of the disease, and which, from the protracted duration of the collapsed state under the cold-water treatment, I may have had more opportunities of witnessing than other practitioners, highly deserving of mature consideration. The circumstance to which I allude, and which constitutes a distinguishing feature, if not pathognomonic symptoms of the Asiatic cholera, is a marked want of correspondence between the state of the circulation, and the functions of the brain. The fact of the mental powers being comparatively unimpaired was noticed soon after the first appearance of the disease, but the remark has not, in my opinion, been sufficiently extended to the powers of voluntary motion as connected with the brain, and to the powers of the muscles themselves. The continued and apparently undiminished sensibility of the nerves of taste has been strikingly exemplified in a great majority of the cases which have come under my notice. At a period of the disease when the circulation is to all appearance arrested—when not more than a teaspoonful of blood escapes from a vein which has been compressed and opened, and that blood is in a state little capable, according to our previous notions, (and these notions are probably correct,) of supporting any function—when the body feels as cold, I may say colder, than any inanimate object in its vicinity, the patient will take with eagerness repeated draughts of cold water, and reject with the utmost pertinacity every other kind of fluid. In one remarkable instance, when the collapse had been confirmed, and the pulse imperceptible for at least 48 hours, the patient complained of her bed, which contained no material more yielding and accommodating than straw, being very hard and uncomfortable. Under precisely similar circumstances the patients will turn themselves in bed, respire deeply and regularly, swallow without the least difficulty, and, if occasion require, get out of and return to bed without the least assistance. In one remarkable case of this kind a female succeeded in getting out of bed in opposition to all the resistance which three nurses could offer. The Asiatic cholera differs in this marked peculiarity from asphyxia, and indeed from any disease which has hitherto been described. The "entasia acrotismus" of Dr. Good approaches nearer to it than any other. This want of correspondence between the state of the circulation and the functions of the brain, as far as sensation, volition, mental action in a higher grade of perfection, and the powers of the voluntary muscles are concerned, demands, on the part of the profession, the most mature and unprejudiced, may I add unbigoted, consideration. It is a fact, which strikes at the very root, and saps the foundation, of the physiological principles most commonly adopted, and if of the physiological principles on which the practice of medicine is based, of medicine as a science, and will, in my opinion, lead ultimately to a greater revolution in that science than is at present anticipated.

The opinion of the profession as to the originality of my views of the disease termed Asiatic cholera and its treatment, is a matter of very secondary importance if put in competition with their correctness and consequent utility; and yet, in justice to myself, I cannot omit throwing out a few suggestions on this subject. In a comparative view of the different modes of treatment it will now be necessary to distinguish between the effects (I allude more particularly to the saline and antimonial treatment) of the medicines themselves, and of the water in which they are conveyed, or which is at the same time allowed. In having directed the attention of the profession to the agency of the water itself, I have materially deviated, as far as I know, from the course of other writers. There is a material difference, I may observe, between not refusing cold water, and urging its use as the agent by which reaction is to be established; between not opposing the desires of nature, and reconciling those desires with a conservative object, and with a long-acknowledged principle of therapeutics. That the vomiting is a conservative action, and as such should not be officiously interfered with or interrupted, is also an important, and, I believe, a peculiar item in the detail of the cold-water practice.

P.S. This memorial being explanatory of those already communicated to the Central Board, and published in The Lancet and another medical journal, I shall feel obliged by its being transmitted to the editors of those publications.

SALT-WATER EMETICS
IN
MALIGNANT CHOLERA.*

To the Secretary of the Central Board of Health.

Sir,—I give a saturated solution of salt, warm, in doses of six or eight ounces every five or ten minutes, till full vomiting takes place. This is generally followed by reaction, and of course the emetic is given in the cold stage. We use it in every case of collapse, or any-thing approaching to that state. It never fails to excite vomiting and to induce reaction. I know of only one instance in which it failed, and that was last night; a large corpulent fish curer, whom I visited for the first time about two p.m. yesterday. Last night we tried the salt emetic, but we could not bring on vomiting. He died this morning at six a.m.

Yours faithfully,
Robert Venables.

Wick, Sept. 11, 1832.

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* Communicated by the Central Board of Health. Condensed.

TREATMENT OF
MALIGNANT CHOLERA
IN THE
PARISH OF ST. GILES, LONDON.*
WITH REMARKS ON THE EMPLOYMENT OF SALT.

To the Secretary of the Central Board of Health.

Sir,—In reply to the Circular No. 2 of the 3rd ult., I have the honour to lay before the Central Board a short account of the method of treatment which I have found successful in cases of cholera. I have the honour to be, Sir, your most obedient servant,

J. Pidduck.

87, Great Russell-street,
Sept. 11th, 1832.

Further experience has confirmed my opinion in the efficacy of this method of treatment, which was stated to the Central Board in a letter dated August the 7th last.

1st. When in the form of bilious diarrhœa, the indications of cure have been, 1st, to dilute; 2ndly, to evacuate the abundant secretion of bile; 3rdly, to allay the irritation which has been excited in the alimentary canal.

For this purpose, I have ordered large potations of very weak chicken, veal, or mutton broth; and afterwards the following medicines:—

Calomel grs. 1 or 2;
Powdered rhubarb grs. 5;
Compound tincture of rhubarb, sufficient to make two pills, to be taken at bed time.

Castor oil 1 or 2 drachms;
Mucilage of gum arabic, 1 ounce; mix, to make a draught, to be taken the following morning.

Chalk julep 1½ ounces;
Sweet spirits of nitre 1 drachm;
Tincture of opium 5 drops; mix, to make a draught, to be taken after the operation of the above.

Bilious diarrhœa I have found uniformly to yield to this method of treatment.

2nd. In the form of rice-water evacuations. I have never seen bilious diarrhœa precede or degenerate into spasmodic cholera, with rice-water evacuations; except when the biliary discharge had been suddenly suppressed by means of opiates and astringents.

That bilious diarrhœa and spasmodic cholera, where they exist together in the same locality, or alternate with each other, as they frequently have done during the prevalence of the epidemic in St. Giles', owe their origin to one and the same exciting cause, there cannot, I think, be a doubt. But the essential difference between the two, seems to consist in the total absence of bile in the evacuations, in genuine cases of spasmodic cholera. As soon, therefore, as bilious evacuations are procured in spasmodic cholera, superseding the rice-water evacuations, the danger is past, and it becomes a case of simple bilious diarrhœa. That is, spasmodic cholera, with rice-water evacuations when it terminates favourably, assumes, I may say, almost invariably, the form of bilious diarrhœa; but bilious diarrhœa never, as far as my experience extends, when properly treated, acquires the dangerous characteristics of spasmodic cholera. I have not, therefore, included any case of cholera commencing with bilious diarrhœa in my returns made to the Board.

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* Communicated by the Central Board.

3rd. When in the stage of collapse.

It appears to me, that the stage of collapse depends upon accidental circumstances, arising neither from the violence of the attack nor from the profusion of the discharges; but more particularly from the weakness of the heart and consequent languor of the circulation. Hence collapse takes place almost instantly, and sometimes when the evacuations have been very trifling in those persons whose circulatory system is in a state of debility from previous disease, or of exhaustion from habits of intemperance; and recovery from this stage of cholera is thereby rendered the more arduous.

Under the second and third head, therefore, in the form of rice-water evacuations, and in the stage of collapse, the indications of cure have been, 1st, to promote the secretion; and 2nd, to procure the evacuation of bile; for, with the secretion and evacuation of bile, the natural secretion of the skin is restored, the cramps and spasms cease, the livid colour of the surface disappears, and in a few hours the function of the kidneys is restored. In order to promote these secretions, it is necessary to restore the equilibrium of the circulation.

For this purpose I have ordered,

1st. The patient to put on a flannel shirt or waistcoat with long sleeves, and go to bed.

2nd. To be wrapped in a hot blanket, and covered with plenty of bed-clothes.

3rd. Large stone-ware bottles filled with hot water and wrapped in flannel, to be laid to his feet and sides.

4th. As soon as the warmth of the extremities begins to return, one or two tablespoonfuls of common kitchen salt, dissolved in half a pint of warm water, has been administered, and the patient's head covered with the bed-clothes, and on no account has he been permitted to rise.

5th. One tablespoonful of common salt, dissolved in four ounces of cold water, has been repeated at intervals, till copious vomiting of yellow bitter bile has been produced, and a profuse perspiration has succeeded.

It has never been found necessary or advisable to repeat the salt and water more than three times; and, moreover, I have borne in mind, that in cases of profound collapse, six hours frequently elapse, after the administration of the salt, before perfect re-action is established. This I have found necessary in order that time may be allowed for the salutary operation of the salt, and for the system to recover from the shock it had sustained.

To allay the excessive thirst which generally attends cases of cholera, even in the cold stage or stage of collapse, I have allowed the patients to drink plentifully of thin gruel or arrow-root, made with water, or even of cold water, which instead of retarding seemed to promote the restoration of heat to the surface, and promote free perspiration, and the abundant discharge of bile, upon which their safety appeared to depend.

The quantity of cold water drunk by some of the patients who recovered from the stage of collapse, and returned almost as soon as it was swallowed, has been enormous, amounting to many gallons.

If there was pain between the shoulders or across the loins, proceeding from the continued action of the stomach and bowels, a sinapism of mustard and water has afforded relief; but when applied to the region of the stomach, the sinapism has had the effect of increasing the spasm and sickness, whilst a large poultice of hot salt and water, laid on in a flannel bag, has allayed those symptoms.

After the bilious evacuations have commenced, the tongue, which was moist, cold, and livid, has become hot and dry, and acquired a brownish or yellow coating, with a red margin.

For the removal of these symptoms, the citrate of soda, with an excess of the carbonate of soda, small doses of castor oil, a slop diet, and a cooling regimen, have been sufficient; except in a few cases where re-action has been attended with acute pain in the ears, which has been removed by the application of a few leeches to the mastoid process.

The only cautions I have been obliged to enjoin are,—1st. To change the flannel shirt as soon as it becomes wet with perspiration; 2nd. Not to rise from the horizontal posture; 3rd. Not to drink any ardent spirits or any stimulants; 4th. Not to eat any solid food for at least four days after convalescence is established.

In the last return I had the honour to transmit to the Central Board, of the 81 cases reported, 65 patients had recovered, and 16 had died. The apparent causes of death (for all the fatal cases were by no means apparently the worst) were,

1st. The patients were moribund, warmth could not be restored to the extremities, and no effect was produced by the salt.

2nd. Either ardent spirits or laudanum had been taken previously, which prevented the operation of the salt. Those cases have been the most speedily fatal, and the disease has been the least under control of remedies, when the patients became choleric immediately on recovering from intoxication. Of this fact I have met with many striking and awful proofs.

3rd. Ardent spirits and other stimulants had been given after reaction had taken place, subsequent to the operation of the salt, &c., when the patients sank again into a state of irrecoverable collapse.

4th. Proper precaution had not been taken to maintain the external heat after reaction had taken place, in consequence of which the patients relapsed, owing to the evaporation from the surface of the perspiration produced by the operation of the salt, &c.

I may be permitted to say a few words upon the *modus operandi* of the salt, as a remedy in cholera.

1st. It seems to restrain the rice-water evacuations by stimulating to contraction the mouths of the exhalants, which open into the alimentary canal; and hence the injection of a hot solution of salt into the bowels is exceedingly useful.

2nd. It changes the spasmodic eructation and dejection of the rice-water fluid into full vomiting and purging. By rousing into violent action the diaphragm and the abdominal muscles, the gall-bladder is compressed, and the cystic bile is forced through the biliary ducts.

3rd. It operates powerfully upon the whole glandular system, and especially upon the liver and kidneys, as well as upon the skin.

4th. It may, according to the theory of Dr. Stevens, aid in revivifying the blood, the vitality of which is, doubtless, impaired by the depressing influence of the choleric poison.

Under the above-mentioned plan of treatment, not a single case of cholera has terminated in typhus fever. Restoration to health has, in many instances, taken place in a few days after the patients had recovered from the stage of collapse.

In conclusion I would venture to add, as far as my experience extends, that, comparatively, few cases of cholera, under any circumstances however unfavourable,* would terminate fatally, if ardent spirits, opium, camphor, capsicum, ginger, catechu, cardamoms, carbonate of ammonia, the essential oils, and all other stimulants, narcotics, and astringents, were entirely expunged from the Pharmacopœia Cholerica.

TREATMENT OF MALIGNANT CHOLERA AT THE DROITWICH LUNATIC ASYLUM.†

To the Secretary of the Central Board of Health.

Sir,—If you think the following worth publicity, and that it really possesses novelty, perhaps you will mention it to the Board; if, on the contrary, you judge it differently, silence upon the subject will oblige. I remain, Sir, your obedient servant,
J. B. Steward.

Droitwich Asylum, Sept. 14, 1832.

Only two inmates have escaped, and these, I hope, are impregnable, as they have been in the midst of infection hitherto without suffering. During the last five days (viz. on the 9th) we have had only one new case, and that, though a bad one, I am happy to say is convalescent. Those persons in constant attendance, have all been attacked; those only occasionally in attendance at the commencement of the disease, have escaped.

The progress and consequences of the disease from its commencement (the 22d of August last) are as follows:—

	Cases.	Deaths.
Between the 22d and 25th Aug.	6	6
On the 26th (day of my arrival)	2	2
From the 27th (3 a.m.) to the 13th Sept. inst.	45	13
	53	21

The treatment from 22nd to 25th August inclusive, cal. and op. and creta mixture.

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* The trials of the salt, &c., were made under the greatest disadvantages; in the most wretched district of the parish of St. Giles's, among a people dissolute in their lives, intemperate and depraved in their habits, dirty in their persons and habitations, surrounded by filth, destitute of every comfort, and, in many instances, of ordinary clothing, and other necessaries of life.

† Communicated by the Central Board of Health.

The two cases on the 26th, being each between 60 and 70, incurable lunatics, were treated with opium, brandy, and tinct. capsici, and, by this, one attacked the next day, ætat. 70, was saved, and is still well. In many cases it was impossible to give solid medicine, and in three, every thing offered was refused. From the 27th Aug. to the 30th inclusive, the treatment was the same, viz. a large dose of calomel with gr. i opii, followed by small doses, with capsicum and camphor at short intervals, using of course the hot-air bath, enemas, turpentine, fomentations, sinapisms, &c., when necessary. Dissatisfied with the result of this treatment, particularly where vomiting was severe, I was anxious to attempt some plan which might promise more favourable results. I had frequently observed in the bad cases the utter impossibility of checking the sickness by any remedy usually employed. I had tried over and over again,—and as often failed, in administering it—the mustard emetic. I determined, therefore, no longer to attempt to check the sickness by the usual means. I accordingly in all cases, from the 31st of August inclusive, in which vomiting was extreme, gave ant. tart. gr. iij, and after the stomach became, as was the case, rather more quiet, I gave every hour a pill containing ant. tart. gr. iij, op. gr. j. In no cases thus treated from the commencement, did the sickness last beyond four doses; very often both vomiting and purging ceased; but where the last continued, I gave rh. and magnes. c. t. o. ʒss, followed by cal. and capsicum c. op. ¼ gr. 3tiis horis, with a mixture of catechu, opium, and carbonate of soda. In one case which happened on the 9th (the last reported), finding the purging and vomiting did not so readily yield, I gave at one dose ant. tart. gr. vj, opii gr. ij. This person was a woman of the name of Ball, a violent lunatic; it was therefore necessary to give it in coffee; after this the vomiting ceased, but the purging (the true rice-water dejections) continued, the pulse being still imperceptible, or at all events so feeble as to induce a doubt of its presence, I now gave ʒj of tinct. of op., also in coffee, and repeated ʒss in an hour afterwards. All the bad symptoms left, and she is now convalescent without other treatment.

The relative effects of the different modes of treatment to which I have alluded, will be seen by the following statement of cases and results:—

	Cases.	Deaths.
From the 22d to the 30th Aug. inclusive	26	17
From the 31st Aug. to 9th Sept.	21	4

Of these, two were violent lunatics, obstinately refusing medicine.

TREATMENT OF

MALIGNANT CHOLERA

WITH

TARTAR EMETIC, AT MANCHESTER.*

To the Secretary of the CENTRAL BOARD OF HEALTH.

SIR,—In accordance with the instructions of your Board dated No. 2, Council Office, Sept. 3, 1832, I respectfully take leave to inclose for their information, my circular upon the treatment of the cholera. I am, &c.

J. LANGFORD,
Resident Medical Officer,

Cholera Hospital, Knott Mill,
Manchester, Sept. 14, 1832.

My attention having been drawn by my friend Mr. Stott, surgeon, of this town, to the practice recommended by Dr. Reich of Berlin, for the treatment of spasmodic cholera, we determined to adopt it, and the result has been already beyond our expectations, and such as to induce us, without hesitation, to urge public attention to its adoption, as applicable to most forms of the disease, *perhaps to all.* We have found it successful in every case during the early stage of incomplete collapse, and also with a cessation of evacuations.

I am aware the practice, as applied to cholera, is quite novel in this country, but its success has induced me not to delay (for a longer experience) this communication.

The following is the mode of application:—Dissolve ten grains of emetic tartar in seven and a half ounces of water, with half an ounce of rectified spirits of wine, of which give half an ounce every two hours, until the biliary and urinary secretions are *properly* restored, then *gradually* diminish its frequency. *Allow toast and water, or whey, ad libitum;* prohibit all heat and frictions, and give *no other* remedy. A *tolerance* of the medicine is in a short time established.

Aug. 10, 1832.

I continue to entertain the same favourable opinion of the successful administration of emetic tartar for the removal of spasmodic cholera. In the advanced stage of the disease it will be found necessary in the first instance to increase the dose, to produce the action of vomiting, as well as in younger patients, to diminish it; and if this remedy is not successful, in the most malignant and fatal stage of spasmodic cholera, when the nervous energy is subdued, and the powers of life laid prostrate, it is not wonderful.

Sept. 5th, 1832.

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* Communicated by the Central Board.

TREATMENT OF
MALIGNANT CHOLERA
AT THE
ABCHURCH-LANE HOSPITAL.

By Alexander Tweedie, *M. D., London.*[*]

In Reply to the Circular of the Central Board of Health.

I am not of the number of those who "are of opinion that they have been successful in their practice." Nevertheless, having been otherwise called upon, I proceed to present a very brief reply.

In the hospital wherein I reside as medical officer, there has hardly been an opportunity of treating the disease in the two first forms, viz., in bilious diarrhœa, and in rice-water evacuations; but from a pretty ample observation of those states out of doors, as well in my own treatment as in that of competent medical friends, I have arrived at an opinion which is somewhat at variance with that of many most able practitioners. This opinion is, that at any reasonable period before the stage of collapse, even though there be rice-water purging—even though there be some vomiting, and although cramps may have come on, the whole disease may be stopped quickly, *safely*, and with human certainty, by full and energetic doses of opium. Doses proportioned to the age, idiosyncracy, and condition of the individual; but having for their object the principle of stopping all the discharges at once. Small doses do not avail, for it appears to me that some secretions are more easily stopped than others—that the hepatic, for example, is arrested by a smaller opiate than that from the intestines, consequently, if we give small opiates, the bile is arrested, whilst the intestinal secretion goes on, and the patient thus hastens on to collapse; but if a sufficient dose be given to stop every secretion, there has not, so far as I have witnessed, been any but the happiest result from it. There is no advantage (the secretions being thus arrested) in being too hasty to act on the bowels by purgatives; they generally act spontaneously in a day or two, and the whole of the secretions are restored without any active medicinal interference.

If, however, they do not so act, and circumstances seem to require it, some mild warm laxative may be given, an undue operation from which is to be guarded against by a small opiate.

This treatment does not apply to some other premonitory symptoms that more rarely arise, nor to those cases which, without previous diarrhœa, go rapidly into collapse. Plethoric and robust individuals will not bear so much opium as thin persons.

In the stage of *confirmed* collapse, I have seen no treatment so efficacious as to deserve especial praise; but some remedies have permitted patients to die more rapidly than others:—

1st. "The saline treatment" has increased involuntary purging, and done harm; or the powders have been immediately vomited and have done no good.

2nd. Excessive opiates have quieted the sufferings, but have appeared to hasten the deaths, of those subjected to them.

3rd. Spirituous stimulants *in excess* have oppressed whatever power remained, and have been of no avail; in moderation they are not injurious.

4th. Those who have recovered from collapse (a few), have done so under frequent doses of calomel, with or without *minute* doses of opium, according to the degree of purging existing, frequent doses of ammonia, sinapisms, and a moderate portion of brandy-and-water, gin, or port-wine, and the like. This treatment, it will be perceived, is founded on the broad principle, that in the cure of the malady nature is the principal, and the physician her auxiliary, and is directly contraposed to that which seems to assume that nature is the auxiliary, and that a very unimportant one, to medicine. A too-meddlesome practice has been always fatal.

5th. Cold-water, toast-water, barley-water, balm-tea, &c., are freely allowed to those who desire drink, its rejection being no objection.

The consecutive fever bears no relation to the quantity of stimulants or opiates administered, some of the most severe cases of this fever having occurred in persons who had taken neither brandy nor opium.

It appears to me that the spasmodic cholera assumes different types in different localities, and at different periods; thus the same remedy that is useful in St. Giles's shall fail in Whitechapel, and that which may have been beneficial in Clerkenwell shall prove injurious in Newgate. Nay, the very remedy that was useful in a place six months ago shall be useless in the same spot to-day. In this I think the analogy of puerperal fever may with benefit be consulted.

I am acquainted with an extensive parish within the bills of mortality whose surgeon has adopted one, little varied, plan of treatment in cholera from the beginning of this epidemic. For a few weeks nearly all his cases recover, and then again, without any alteration of system, he finds that nearly all his cases for the next few weeks die. This has occurred more than once without his being able to refer to any tangible cause.

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[*] Communicated by the Central Board of Health.

Observations in Surgery and Pathology; illustrated by Cases and by the Treatment of some of the most unfortunate Surgical Affections. By WILLIAM JAMES CLEMENT, Surgeon. Shrewsbury: Watton. London: Whittaker and Co., and Highley, 1832. 8vo. pp. 320.

LONDON, though it includes much of, does not, like Paris, monopolize *all*, the talent of the country of which it is the metropolis. Medical skill, zeal, and inquiry, are conspicuous in every large town of England. We regret, however, that country practitioners, considering their numbers, and their wide field of observation, contribute so little to medical literature. We hail them with the highest gratification when they do.

Mr. Clement's "leading article," is "a critical inquiry into the different opinions respecting the structure of the urethra." The rest of his volume is an intended contribution to operative surgery, in the shape of cases collected at Shrewsbury, where he informs us that he is in a somewhat extensive practice.

The essay on the structure and on stricture of the urethra, is a vigorous attack on the late Sir Everard Home's doctrines, who, as our readers may remember, fixed the particular attention of surgeons on these subjects in three several volumes, and a paper published conjointly with Mr. Baner in the Philosophical Transactions. His first publication appeared in 1795; his last in 1821. Mr. Clement, consequently, comes rather late into the field, and as polemic physic, like polemic theology, is generally poor trash a few months after it is written, we should, in mercy to our readers, have passed this by; but the author has here mustered all his strength, and the question is not settled; so we shall discuss the arguments adduced, as far as they may throw any light on the organization and diseases of the urethra, without pretending in the least to follow the "confutation" throughout.

When an inquirer wishes to determine the nature of a body, he ordinarily subjects it to his own senses, and resorts to every known invention for determining its internal, ultimate constitution. The chemist flies to experiment; the anatomist dissects, injects, macerates, analyses, uses the microscope to distinguish the different animal tissues, and the combinations into which they enter in different organs. In examining the structure of the urethra, Mr. Clement has attempted nothing so arduous. He gives no research, no discoveries of his own. He professes merely to reason on, and "contrast the sentiments" of, others. He supplies us with no original facts, so we proceed, with him, to "consult the best authors."

For the sake of brevity, Mr. Clement divides those who have written on the urethra "into two classes; the one supporting, the other denying, the opinion of the muscularity of the canal." Sir E. Home and Sir Charles Bell are for him, the heads, the prototypes, of these two classes.

We shall not enter into the topographic or descriptive anatomy of the urethra, but, to make the discussion more precise, shall remark, that the urethra is not homogeneous. It is, on the contrary, divisible into four distinct layers, perhaps, say—we may—tissues. 1st. The internal or mucous membrane, as it is called; 2nd, the cellular vascular (Shaw's internal spongy); 3rd, the corpus spongiosum urethræ; and 4th, elastic envelope, somewhat analogous to that which surrounds the corpora cavernosa. No writer of note has pretended that the *internal membrane* is muscular. Sir E. Home says (Phil. Trans. 1829, p. 184), from Mr. Baner's experiments, we find the urethra is "made up of an internal membrane, and an external muscular covering. The internal membrane is exceedingly thin, and no fibres are met with that can give it a power of contraction." One is astonished, after this, to find in Mr. Clement's inquiry several passages in his preface like the following, p. 5.

> "The profession has long been divided in opinion as to the real structure of the membrane of the urethra; but the majority, I believe, are in favour of its muscularity. This doctrine has been established principally upon the authority of Sir E. Home, &c."

The inaccuracy of this will be detected at once, by referring to Baner's beautiful plates, which accompany the above-cited paper. Although the excretory canals, as of the bile, urine, products of generation, &c.

have considerable analogy with the mucous, we think they differ from them very essentially. Blainville, one of the first anatomists of his age, says he found in these canals a *réseau vasculaire*, highly developed; indeed, to such a degree, that when inflamed it became quite apparent in the sheep and ox; vascular anastomoses were distinctly evident. He found no epithelium or epidermis, but, in its place, a membrane resembling fibrous tissue, yet less compact and shining, — properties peculiar to serous membrane. This membrane differed from the mucous in presenting no papillary elevations; whence he concludes it is intermediary between the serous and mucous membranes, and proposes to call it *sero-mucous*—a denomination we willingly adopt. Blainville's *réseau vasculaire*, corresponding to the rete mucosum of the skin, is, we apprehend, Mr. Shaw's internal spongy body. We are not so sure that it is this which Baner depicts for muscular fibre; it *may* be a vascular layer, lying, as in the bladder, betwixt the internal membrane and the muscular coat.

The *corpus spongiosum urethræ* is well known since Ruysch's admirable injections. Some pretend it has elastic fibres diffused through it, but this has not been fairly demonstrated. We know of nobody that has found muscular fibre here, or in the *elastic envelope*.

In reply, then, to the question, Is the urethra muscular? we say this has not been proved. If Mr. Baner discovered a fibrous structure surrounding the internal membrane, it cannot be pretended, with any justice, that his microscope identified it as muscular. Still we contend that the urethra contracts spontaneously. Its contraction is firmer, its canal is less dilatable at one time than at another, and this contractility is put in play by the vital injection of the erectile tissue, which is thus its mediate or immediate cause. The phenomenon seems to have some analogy with the contraction and dilatation of the iris, whose reticulate vascular structure made Blainville and others call it erectile (spongy) tissue. Future research must definitively decide whether the movements in these cases, in the fallopian tubes, &c., are actually accomplished by muscular, erectile, or elastic tissue. Were we disposed to extract the opinions of anatomists on this subject, and lay them before our readers, we should derive but small assistance from Mr. Clement's "Critical Inquiry." He says, indeed, "The opinions and quotations which I propose to offer to the attention of the reader, are from the best authorities;" but the reader will properly appreciate this, when we state that most of the passages adduced were written anterior to Baner's discovery; that their evidence is only negative; that we have pages of extracts from such men as Lietand, Marjolin, Portal, Hip. Cloquet; while not a word is said of Ruysch, of Soemmering, of the immortal Bichat, or of his "Traité des Membranes," or of the indefatigable and philosophic Meckel.*

Is there any-thing in the physiology or pathology of the urethra, to indicate its muscularity? In J. Hunter's day, the fact that the urethra is capable of contracting its canal, so much so as to shut it up entirely, was at least a strong presumptive proof that its "substance was muscular." At present we believe it stands proved, that contractility is possessed by organic substances which have no pretension to the character of muscle, which we can only define physiologically as a fibrous structure, shortening under nervous or electric excitement, supplied only for a limited time, and consequently exhibiting alternate contraction and relaxation. Irritability, with us, expresses neither more nor less than this. We do not know that thus defined it has been positively observed, during the emission of urine or semen, in any part of the urethra, out of the reach of the muscles at its root.

It may be urged, that fluid in an elastic tube, contracted at both ends, would not be expelled,—that the urethra must consequently have some muscular vermicular movement to express the last drops of its contents. It may be replied, that the urinary canal, external to the bulb, is never shut but loosely, and that the urine or semen, compressed uniformly by its elasticity, and impelled forwards by the posterior muscles, would necessarily be affected through the external orifice. The expulsion of an injection or of a bougie may be accounted for in nearly the same way.

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* Handbuch der Menschlichen Anatomie von J. F. Meckel, Halle, 1815-20. There is a French translation of this admirable work, from which some of our late writers have drawn largely. We should like to see it in an English dress.

The grand argument for the muscularity of the urethra, is derived from the phenomena of spasmodic stricture. The urine stops all at once, the bougie is arrested, or, if introduced, is firmly grasped: a short time elapses, antiphlogistics or antispasmodics are resorted to, and the passage is again free. This made J. Hunter say, "The urethra is subject to the diseases of muscles in general, which is indeed the only proof we have of its being muscular." It is highly probable that disordered action of the muscles, at the posterior part of the urethra, produces really *spasmodic* stricture; but with all deference to Hunter and his school, we submit that irritation, producing excessive injection of the erectile tissue, and simple contraction, would produce stricture as effectually and rapidly as could muscular contraction. If we mistake not, it would account for everything observed in dilatable or spasmodic stricture. There are preparations of the urethra, which it is pretended have packets of longitudinal fibres distinctly muscular. The late Mr. Wilson showed one, which Mr. Clement declares is "confronted by another in the College museum," where the "identical part is proved beyond all doubt to be a congeries of minute vessels." "Mr. Wilson exhibited also a preparation of what he termed a spasmodic stricture." We have before stated our views of spasmodic, or, as we shall term it, acute stricture. Permanent or chronic stricture is sometimes the result of general condensation in the urethral substance; it is, however, more frequently formed by coagulable lymph deposited in the internal fibrous membranes, or rather in the cellulo-vascular tissue under it. This lymph tends to assume here, as it does elsewhere, the character of the tissue with which it is in contact; and, when recent, constitutes, for Mr. Wilson and others of his school, a spasmodic stricture. For us it is but the contracted or hypertrophied fibrous membrane. Mr. Clement disposes of Mr. Wilson by ridiculously asking, "Is it possible for a spasm of any muscle to exist after death?" He may be referred to the state of the muscles after death from tetanus or spasmodic cholera.

We think little of the reasons brought forward to prove, that the urethra is *not* muscular; they are useless. The existence of a body may be made manifest or proved, but its non-existence can only be asserted. We give but the following:—Sir C. Bell says, "I cannot imagine with some that the urethra is muscular; first, because I see no end it could answer in the economy." Mr. Clement argues that "the passage of urine through the canal of the urethra, if it were muscular, and possessed an independent power of contraction, would in all probability resemble the flow of arterial blood, which we know escapes from an artery in jets or distinct pulsations," &c. These jets are assuredly not produced by muscular contraction of the arteries, but of the heart; and though all muscles contract for a limited time only, many do not contract in *spasms*, but, like the detrusor urinæ, regularly, continuously. If the urethra be muscular, it contracts like the bladder, with which, when distended, it forms one cavity, or, like the œsophagus, by a movement of contraction propagated from one extremity to the other. Mr. Clement asserts (p. 26), "This mode of reasoning" (opinion, he means) "would in reality give a sphincter to a part where no such structure could by any possibility be wanted." Again (p. 28), "We must account for the formation of stricture on other grounds than the contraction of muscular fibres, *for* we find by dissection, appearances which denote previous inflammation," &c. We refer to the work, and leave Dr. Whateley to find out the logic of this class of arguments.

The view we have taken of acute stricture would induce us in the first stage to employ leeches and antiphlogistics to disengorge the canal. When there have been frequent accessions and resolutions of acute inflammation, it becomes *sub-acute*; and in this stage leeches may still be useful,—narcotics should be employed, as there is irritation; but caustic and moderate pressure (the bougie) will be found of more use than in slow inflammation of any other structure. Chronic stricture may be obliterated by the same means, or exclusively by pressure and the lunar caustic; the latter, we believe, acting in general on the same principles as when it removes nebulæ from the cornea; it diminishes the nutrition of the part, and anormal deposits are thrown off. Sir E. Home's mode of applying caustic is, perhaps, the simplest and best. The processes and instruments invented by M. Ducamp are ingenious, but complicated. In the latest stage of what we have called sub-acute stricture, Mr. Clement denounces the bougie, doubts the efficacy of caustic, advises leeches, and, when the soreness is gone, the application of linim. hyd. c.

Before dismissing the "Critical Inquiry," we may observe, that the disciples of the late Sir E. Home will complain of the spirit in which it is written. Sir Everard is treated with great bitterness. Take the following instances. Sir Everard accounts on his own principles for the formation of stricture in the membranous portion of the urethra. Mr. Clement catches at this and says (p. 11),

> "I am particularly surprised at his attempt to account for the formation of stricture in a part of the urethra, where the existence of stricture has been denied, not only by the best practical writers on the subject, but contradicted also by the preparations in the college museum."

After this (p. 30) there is a quotation from Hunter, where he says that "strictures occur most frequently in the bulbous part:" "we find them, however, sometimes on this side of the bulb, but very seldom beyond it." Home himself says, they "occur most commonly behind the bulb, from six inches and a half to seven, of the external orifice." The situation "next in order of frequency, is four inches and a half from the orifice of the glans," &c. Sir A. Cooper asserts their most common seat is in front of the bulb, and that after this they are most frequently found in "the *membranous* and prostatic portion."* These gentlemen should have made their statements in numbers, but they show clearly enough that Mr. Clement's assertion is unfounded and vexatious. In the "Observations on the Human Urethra" (Phil. Trans., p. 184), it is written—

> "The muscular covering, by which the membrane is surrounded or enclosed, is made up of fasciculi of very short fibres, which appear to be interwoven together, and to be connected by their origins and insertions with one another; they all have a longitudinal direction. * * The fasciculi are united together by an elastic substance of the consistence of mucus," &c. Vide plate 23. "I (Mr. Clement) cannot believe in the justness or accuracy of their opinions * * * for such fibres as are united together by an elastic substance of the consistence of mucus must be inconceivably minute. * * This elastic substance, which unites the fasciculi together, I presume must have the same relation to those fibres where it is attached, as tendons have to muscles in general; but the idea of a tendon of not much thicker consistence than saliva, would appear ridiculous," &c.

If the reader turn to the plate, he will find these fasciculi are represented as connected directly with each other, and only laterally by elastic substance. Besides, cellular tissue is the basis of all animal structure (cellular is perhaps an improper appellation, it would be better to call it albuminous, or, with Borden, Meckel, and others, mucous tissue (*Schleimgewebe*), for such it is in the parenchyma of organs, notably in the muscles examined with the naked eye or microscope), it is not then astonishing that Baner found it in the urethra. Mr. Clement can never have examined the urethra through a microscope.

On the whole, we cannot guess why Mr. Clement wrote on a subject for which he was so ill-prepared. He was, it seems, a pupil of Sir C. Bell, and most likely wished, with the late poor Mr. Shaw, to "bear his humble testimony" to the doctrines developed so luminously in that author's Treatise on the Diseases of the Urethra, &c. There is no reason why Sir Charles should be without his "attendant spirits," but we think there is rather too much of this in the medical world. We have been long tired of "Mr. Abernethy's pupils"—of "Broussais' proselytes,"—and of all the tribe of medical and surgical lion-providers, who, because they dance attendance, hear the roar, and now and then pick up a fragment from the meals of their idol, think themselves entitled to jump furiously on all those who happen to be "o' th' opposite side."

Eight cases of strangulated hernia are next detailed. In five, Mr. Clement performed the operation with success, two ended fatally on the third day, in the eighth the patient sunk soon after she was brought to the table—the incisions were not made. All the individuals operated on, with the exception of one, were females, four of whom were between seventy and eighty years of age. All were subjects of old unsupported herniæ, and the fair proportion of recoveries five-sevenths (Mr. Hey saved seven-ninths), confirmed the general observation of all writers, that "an old hernia is not readily strangulated, and when it falls into this state the danger is not imminent; the distention of the opening, previous to incarceration, has so dilated and weakened the parts, that they can no longer produce a close constriction."[*]

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* Lectures.

In these cases all the ordinary methods of reduction were resorted to, except tobacco, for which the author expresses a rooted, inveterate aversion. He never employs it, and, "as the result only of his own experience," declares, "he never knew an *operation succeed* after the tobacco enema had been given." (p. 54.) In two cases he has seen fatal effects decidedly produced by the tobacco, &c. We believe tobacco, as well as bleeding, will kill, and we believe it is more efficacious than any other means in the reduction of hernia. When it is administered in moderated doses, before the patient has been thrown into syncope by copious bleeding, the taxis, warm-bath—before the intestine is gangrenous—we think little more danger is to be apprehended than from smoking a cigar or pipe till it cause nausea and sickness. Even in small, recent, acute herniæ, it should be immediately and almost invariably employed, its action is so speedy and so critical. If this, with cold applied to the part, and the taxis do not succeed, the hernia may be considered irreducible, and the operation should only be deferred till the effect of the means used have subsided. Bleeding may precede these measures, but, before inflammatory symptoms appear, the tobacco should, we think, in most cases supersede it, for narcotine produces complete but transitory prostration, it probably retracts or diminishes the bowel,[†] at the same time it slackens the stricture; while the debility from loss of blood is lasting, and we do not think that, when carried even to a great extent *before* inflammation has commenced, it lessens its subsequent danger. When, as stated by Mr. Clement, the tobacco enema serves only as an excuse for repeating "the use of the taxis"—when it only delays the operation—when it is considered only "a last resource—a kind of forlorn hope" (p. 56), it is most pernicious.

Our own accords with the experience of all practical surgeons, that "glysters of tobacco constitute most powerful and certain means of relieving incarcerated hernia, independently of the operation;"[*] and if any discrimination is to be shown in the choice of means—if the tobacco enema is likely to succeed at all, it is in cases, like Mr. Clement's, of old, unsupported herniæ. His own cases, indeed, seem to speak in its favour. He is "unconnected with any public institution," and publishes eight cases of strangulated, irreducible hernia, "selected from amongst others;" the first dated 1823, the last 1830; and while they do honour to his skill in operating, discredit the means employed previously. We have seen the practitioner puncture an encysted tumour rather than remove it—allow a shattered limb to rot away rather than amputate—and doubtless the operation for hernia is often most criminally delayed; but we entreat the young surgeon not to discard the means of reduction most likely to succeed, not to be too sanguine for operative glory—too ardent for vivisection.

In the only case of umbilical hernia related (Mrs. A., p. 99), an improvement is suggested in the mode of operating. The protrusion was large, and "an incision was made on the side of the tumour," and then into the sac. Every end would have been answered, and evil avoided, by dividing the stricture without opening the sac. (Vide Cooper, Lawrence, &c.)

Mary Meredith, aged 55, had a large irreducible, inguinal hernia; it became strangulated, an "intelligent surgeon" tried the taxis and the tobacco enema; these did not succeed, but she would not submit to the operation, "because Mr. Clement had, on a former occasion, succeeded in reducing the hernia when it was in a state of incarceration." She died in eighteen hours. On dissection,

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[*] Lawrence on Hernia, p. 29.

[†] We have seen the tobacco employed in epiplocele, but never with advantage.

[*] Lawrence on Hernia, pp. 76 and 76-81. Pott's works, vol. 3, p. 277. Hey's Observations, pp. 122-127. Sir A. Cooper says—"If I were myself the subject of crural hernia, I should only try the effect of tobacco glysters, and if they did not succeed, would have the operation performed in twelve hours from the accession of the symptoms." (On Hernia, Part 2, p. 32.)

> "The sac was opened at its upper part, when a portion of the colon was discovered not much inflamed, but distended with hard, feculent matter, and pressing firmly on the lower contents of the tumour. The colon was followed downwards by enlarging the incision made into the sac, when numerous convolutions of the jejunum and ileum more than a yard in length were exposed, of a black colour and in a state of gangrene."

The symptoms of this extensive gangrene were attributed, as they often are, to the tobacco enema (p. 136). We give this case as an exception to the rule noticed, p. 12.

The author elicits one important general fact from his observations,—advanced age is no essential objection to the operation. Mr. Griffith's (his partner) practice afforded him several old females, which, with those cited, he could adduce as "examples of the good effects derived from the operation, when performed upon patients who had passed the age of seventy" (p. 118). This division of the work before us is perhaps redeemed by the remarks we have briefly brought forward; otherwise we think the publication of cases unattended by any peculiarity. Detached cases connected by no discovery, the enunciation of no general principle might at least be published in the periodicals.

Some of the succeeding cases are not uninteresting. There is one of *spina bifida*, in which the operation of puncturing the tumour was repeatedly tried, but ultimately proved fatal. In a fatal case of cystocele, the whole of the bladder was lodged in the scrotum, &c. Lastly, we have an account of an "immense tumour attached to the lower jaw, and extending nearly to the clavicle." On removing it the "trunk of the portio dura was probably lacerated." The face of the patient on recovery was found singularly drawn askew, and was thus especially when she laughed, sneezed, or cried; a remarkable "illustration of Sir C. Bell's doctrines." The volume ends with some long eulogiums on this well-known physiologist's discoveries, and considerable extracts from his works.

The manner in which this book is got up does credit to the Salopian press.

The Principles and Practice of Obstetric Medicine, in a Series of Systematic Dissertations on Midwifery, and on the Diseases of Women and Children. Illustrated by numerous Plates. By David D. Davis, M.D., M.R.S.L., Professor of Midwifery in the University of London, &c. &c. London: Taylor. 1832.

The parts of this interesting and valuable work up to Part XI. are now before us. From Part VIII. we condense an account of a disease comparatively rare, and treated in a very imperfect manner by former obstetric writers.

Entero-vaginal hernia is the protrusion of some of the abdominal viscera through the parietes of the vagina. The intestine, which most frequently protrudes is the ileum, although, in some few cases, the colon and cæcum have been implicated in the descent. The vaginal swelling occasioned by the intestinal prolapsion of which we are now speaking, is soft and elastic, perfectly compressible on the application of pressure, "increasing by standing, and diminishing, or entirely disappearing, when the patient lies down. The contents may be readily pushed up with the hand, but they descend again if the patient coughs or strains."—*Lawrence; Sandifort.*

The envelopes, or external constituents of the cyst of this variety of intestinal protrusion, are one or more of the tunics of the vaginal parietes of the part affected, greatly relaxed and distended, and the peritoneal lining of the anterior or posterior chamber of the pelvis, as the case may be, produced and still more distended. With respect to the constituency of the hernial sac, Baron Boyer observes, "that it is not yet determined, whether, in all cases of entero-vaginal hernia, both tunics of the vagina are distended at the same time, or whether by opening for itself a route through the fibres of the external tunic, by merely separating them, it effects the distention only of the internal tunic."

The formation of an entero-vaginal hernia has, in a small proportion of cases, been so slow and gradual, that the date of its commencement has not been positively known. But, in the greater number of cases, the intestinal displacements have taken place suddenly, in consequence of strong personal efforts, quick movements, violent shocks from falls, and also as results of certain varieties of difficulties incident to the function of parturition. The first recorded case of entero-vaginal hernia, was indeed occasioned by an awkward and rapidly-effected movement of the body, in the instance of a person rendered predisposed to it by a recent confinement. The subject of the history, which is given by M. de Garengeot, sought his advice for a presumed descent of the womb. About a month after her last confinement, she made an effort to assist in lifting a load upon the shoulders of a porter. At the instant of that effort, she felt a derangement within the abdomen, and an acute pain in the vagina, in which also she had a sense of great fulness. Her midwife advised her to consult a surgeon, but this was neglected. At length the descent into the vagina became so considerable, that it manifested itself at the external orifice, beyond the level of which a tumour protruded about a finger's breadth.

"The patient felt from time to time pains of an internal spasmodic character, which seemed to take their origin from that part, as also pain of the stomach, with sickness. Moreover, she was not able to void her urine, except in the position of lying on her back. Informed of these particulars, M. de Garengeot discovered on examination a whitish tumour, which not only occupied the vagina, but which projected beyond the labia pudendi in such a manner as to admit of the introduction of a finger between it and the vaginal parietes posteriorly. On passing his finger beyond the tumour in that direction, he could feel the orifice of the uterus occupying very nearly its natural situation, whence he concluded that that organ was not implicated in the existing mischief. He also found that the pressure which this examination caused to be made on the tumour, which indeed was unavoidable, had the effect of reducing it to about one half of its previous size. Such a change in the size of the tumour excited at once the suspicion that it consisted in a descent of intestine. With that impression on his mind he placed the patient in a lying position on her bed, and applied the taxis to the tumour with the utmost accuracy of attention. Whilst this operation was performing, he felt its contents as if receding through the superior and right lateral portion of the vagina; and after the retrocession was effected, it was left soft, relaxed, thin, and imperfectly occupying the space which had been previously occupied by the visceral protrusion. In order to strengthen his conviction that the case was one of entero-vaginal hernia, of which however he had never heard before, nor known any author who had described it, he desired the patient to get up and walk, and also to cough strongly. These movements caused the tumour to present itself immediately again. That circumstance served to establish M. Garengeot's conviction that it was actually a hernia. He effected its reduction a second time, and desired the patient to keep her bed until he should be able to supply himself with a suitable pessary. After some failure on this point, in the first instance, a well-adapted contrivance was at length applied, which had the effect of preventing any future descent so completely, that the patient never afterwards sustained any inconvenience from it."

It has been observed that entero-vaginal hernia has more frequently the posterior than the anterior chamber for its seat. An example of this variety of intestinal prolapsion is quoted by Hoin, which was exclusively the result of repeated and violent efforts on the part of a young lady, who was subject to obstinate constipation, to unload her bowels. In this case the tumour occupied the vagina laterally, but somewhat posteriorly, and did not occasion any difficulty in the discharge of urine.

"Comparing the symptoms and facts respectively of his patient's case with those of M. Garengeot's, M. Hoin came to the unavoidable conclusion, that in the former, the prolapsed intestine had effected its escape from the abdomen *at the posterior pelvic chamber*, intermediately between the rectum and the uterus; and that it had insinuated itself into the loose cellular tissue connecting the fleshy and inferior surface of the peritoneum of that part with the ultimate portion superiorly and posteriorly of the vagina. That such was the locality of the intestinal descent in that case, was proved by the hand, when introduced to ascertain the situation of the orifice of the uterus, being obliged to be passed up *anteriorly to the body of the tumour*, whereas in cases of prolapsion of the intestine by the anterior route, as in the case of the carpenter's wife, the examining finger is obliged to be passed up between the tumour and the posterior parietes of the vagina. This is a very simple diagnostic rule for determining the locality of entero-vaginal hernia of each variety. Another, which was first propounded by M. Levret, Observations sur les Polypes, &c., p. 162, is derived from the direction of the changed position of the uterus, occasioned by the one and the other variety of intestinal descent. This rule is however not absolutely to be depended upon, inasmuch as the uterus has remained totally unaffected as to its position during the presence of entero-vaginal hernia. An additional circumstance which may serve to distinguish an anterior from a posterior descent. is the ordinarily inferior extent of protrusion in the former than in the latter variety. In the former the tumour presents laterally and anteriorly relative to the vagina, and for the most part high up near the orifice of the uterus; whereas in the latter it occupies the posterior and lateral portion of the vagina, and extends ordinarily as low down as the vulva, and in some cases even so far as to project beyond the level of the pudendum. It is however a fact common to both varieties, that the tumour should have a bearing towards, and principally occupy, one or the other side of the vagina. It is indeed obvious, that by reason of the occupancy of the median district in front by the strong walling of the urethra, and of the same behind by that of the recto-vaginal septum, the tumour in each case must receive something of a bearing towards either side. * * *

"But it is much more important in practice to establish the proper distinctions between hernial protrusions of intestines into the vagina and other kinds of tumours occupying the same passage, than between the several varieties, at least in their simple forms, of such protrusions themselves. It might be very possible for a practitioner not much experienced in matters of this kind, to mistake, for instance, a case of hernial descent for prolapsion of the vagina. The practical result would of course be, that he would content himself with merely introducing a pessary into the vagina, without previously effecting the reduction of the hernia, and of such a blunder the consequences it is obvious might be very serious. A woman had for some years been the subject of an entero-vaginal hernia, which occupied the vagina anteriorly. She had sought no remedy, because she had considered her case to be one of simple bearing down of the vagina. She was, however, at length induced to consult M. Hacnel on the subject. That gentleman at once recognised the nature of her malady, and by an adroit adaptation of a sponge pessary to it, *after previously reducing the hernia*, effected a speedy cure of it. Gunz de Herniis libellus, p. 84, et seq. The same author refers, however, to another history, where a mistaken diagnosis in a case of the same kind, suggested an operation which proved fatal to the patient. 'A woman was the subject of a tumour which occupied one side of her vagina, and which presented itself at the orifice of that passage. She consulted a surgeon, who, it was believed, mistook it for an abscess, and plunged his bistouri into it. The incision was immediately followed by a protrusion of the cæcum, and of a great part of the colon, which were strongly propelled through the incised aperture by the involuntary efforts of the patient, increased no doubt at the moment by the pain from the infliction of the wound. The protruded bowels were not attempted to be reduced. Gangrene of course supervened, and the poor woman died.' Prof. Gunz. loco citato. * * *

"The existence of entero-vaginal hernia, when the descent takes place from the posterior pelvic chamber, may be ascertained by examination, either by the vagina or by the rectum, inasmuch as the tumour must be considered as being, although somewhat indirectly, placed between those two canals. This point of the pathology is noticed also by Sir Astley Cooper, and is well illustrated by a case which occurred in the practice of the late Dr. Haighton."

In successive chapters, Dr. Davis treats very fully and ably of entero-vaginal herniæ in its different complications, and enriches the history of this disease by interesting cases of his own.

In such a book-making age as the present, it is at once consolatory and refreshing to meet with such a publication as this upon the Principles and Practice of Obstetric Medicine. It is difficult to conceive the motives which urge some writers into printing-offices. Books now every day appear, wherein are exhibited few signs save of the want of information displayed by their authors. In times, not far gone by, it must be admitted that a single new idea was ushered into the world decked with all the imposing foliage of a splendid tome, and far be it from us to assert that the system has undergone any advantageous reformation, for of late it has been the practice to publish whole libraries of books, in which ideas—not wholly useless in mental composition—are altogether wanting. Beset then, as we are, by such loads of paper trash, we rejoice in having an opportunity of recommending to the attentive consideration of the profession, such a sterling work as that of which Professor Davis is the author.

Dr. Davis has enjoyed an ample field of observation, and he now proves by this performance that he has not been an idle man, but a careful, scrutinising, inquirer. Having, during a long series of years, accumulated a mass of important facts, he is entitled to the thanks of established practitioners, and to the gratitude of all the junior members of the profession, for thus presenting to them, in an agreeable form, the results of an experience which it can be the lot of few, even eminent physicians, to enjoy.

THE LANCET.

London, Saturday, September 22, 1832.

If there be any truth in the axiom that experience is the richest source of wisdom, it necessarily follows, that in order to derive the largest portion of knowledge from the greatest number of ascertained facts, those facts should be so arranged or systematised as to be brought with facility within the range of the perceptive and contemplative powers of the mind. The illiterate cavillers against the institution of a travelling clinical commission exclaim—"*Cui bono?*" and affect to found that question upon the unsatisfactory results of all the inquiries hitherto instituted respecting the pathology and treatment of cholera. Instead of exclaiming *Cui bono*, they ought to admit, if they have any spice of benevolence in their bosoms, that the object to be obtained by the establishment of a Moveable Commission, is one of immense magnitude; in short, no less than that of protecting the lives of thousands from the destructive ravages of a terrible disease, and of banishing from millions of human beings the strongly impressed feeling of dread which almost incapacitates them from executing the ordinary occupations of life. If there be any who deny that such purposes are not in accordance with the wants and welfare of society, we must desire that they will address their arguments to others rather than ourselves, for we cannot condescend to comment on motives which bear no relation to public utility, nor to follow a train of observation which does not penetrate more than half way into that field which contains the grossest fallacies of the vulgar errors of the present day.

The malignant cholera, which first broke out in India with such marked and terrible fury in 1817, has now reached in a westerly direction to the back woods of America, and in its frightful course has slayed millions of human beings. In some instances it has desolated whole provinces, in others depopulated villages, in others broken up all the social relations in cities of great magnitude, and yet what know we up to this hour of any successful mode of treating the disease which has obtained for it that confidence in the profession entitling it to, or ensuring to it, a general adoption? We by no means assert that practitioners have not discovered the means of curing this malady, but we do contend, and most earnestly, that there is not yet one uniform system of treatment acknowledged as the most successful, and recognised as such by the great body of medical practitioners. If we are still in this unfortunate, not to say disgraceful, position, after fifteen years of fruitful experience, is it not fair to conclude that our inquiries have not been conducted in the most efficient manner; that we have failed, in the absence of philosophic arrangement and logical induction, to derive the greatest quantity of knowledge from an ample store of facts; in short, that with respect to the successful treatment of cholera, the medical practitioners of England occupy nearly as insecure a ground as did those of India at the first furious outbreak of the disease in 1817? Inquire of fifty physicians or surgeons, residing in fifty of the infected districts, in what manner they treat patients attacked by cholera, the chances are fifty to one in favour of there being fifty different answers. Either the medicines would be fifty times various in quality, or the quantities of the medicines, if the same in quality, would be of fifty different proportions, the results of course being at the same time variously marked, and leaving no well-characterised and certain rule as a guide for the future to any one of the fifty practitioners. Ought we then to rest patiently under such circumstances? Ought we coolly, ought we tranquilly, to witness the still destructive career of such a pestilence, and resort to no *efficient* steps to check such a devastating tide of misery and annihilation? And what is it that we propose with a view to stem the torrent of evil? Is the barrier we have projected difficult as to construction, or extravagant as to cost? Be it remembered, that this is a country in which we boast daily of the liberality of our institutions,—of the reflective, meditative, character of our people,—of the charitable disposition of the nation, and of the general desire manifested on every occasion of misfortune or abuse, to provide a remedy commensurate with the importance of every existing disaster. If the good which we know would inevitably result from the appointment of a clinical commission were in the least degree problematical, then indeed, considering the depressed state of our national finances, there might be the *shadow* of a pretext for the non-establishment of such a medical embassy; but inasmuch as the inquiry would most indisputably lead either to *the adoption of some really useful remedy*, or to the rejection of all those medicines on which *no* reliance should be placed, the expense to be incurred ought not to weigh as a single grain in the scale, while estimating the value of these vast and overwhelming advantages. Ten thousand pounds, we have said, would be the utmost cost of the commission—ten thousand pounds, we repeat, would be the largest amount to be extracted from the public exchequer, whilst the object of the withdrawal of that sum would be to protect the lives of millions. Why, that minister, or public officer, who has a head to think of the blessings which would follow the discovery of a remedy for cholera, or a heart to feel the pangs and calamities which arise from the uncontrolled career of the pestilence, and yet could offer opposition, on the plea of expense, to the institution of such a commission, would deserve to be whipped through the public streets, and exhibited daily on the summit of a pillar formed out of the bones of the victims of the plague, as the most besottedly ignorant, the most brutally prejudiced, the most blindly cruel, of all earthly mortals. We had always thought that medical science was cultivated with a view to secure the public health, and not merely to render our attainments in that branch of knowledge mere matters of ornament or boast;—that it is our duty to apply the experience we possess in that department of science to the laudable object of securing our fellow-creatures from suffering, rather than to plume ourselves upon the possession of a large store of information—that information being extracted from the heart-rending records inscribed on the posthumous monuments of human wretchedness. The natural, and therefore rational, incentive to an appetite for knowledge, is to be found in the desire for happiness; hence, if the attempt to improve even a comfortable condition be a duty, surely it is not less either our duty or our interest to make a well-directed effort to be extricated from a situation of frightful danger. It has been long acknowledged that in science, literature, and the arts, every human being should adopt for his motto—"March on." Are we then in *speculative* matters of inquiry—are we, in those investigations which have for their object the temporary employment of the mental faculties, rather than any great permanent advantage, to exert ourselves night and day in the *possible* attainment of benefit—in a word, are we like the mariner on the distant ocean, to throw out the anchor in the hope of striking some insecure land, when a port stands invitingly in sight, affording at once a solid anchorage, and a safe shelter from the wind's rude blast?

When we recollect the profligate, the wanton expenditure of money in times not long gone by for all sorts of trumpery gewgaws and childish shows, it is scandalous that the Government of England should neglect to seize the first opportunity for devising those means which appear to hold out the most reasonable expectation of furnishing a check to the ravages of cholera. Parliament most judiciously awarded to Dr. JENNER a higher amount of reward than the sum that would be expended by the moveable commission. The small-pox, it is true, slayed its hundreds and disfigured its thousands, but the operation of inoculation was, to an immense extent, a security against both, previously to the general adoption of vaccination. In fact, much of the argument of that grant was founded by its supporters on the ground that vaccination would prevent *disfigurement* occasioned by small-pox; and at once a sensible and a grateful nation admitted the justice of the grant, and gave a unanimous voice in favour of the discoverer. If to obviate the *disfigurement* of our fellow-creatures such an award was justifiable, is it less reasonable or less called for to submit to such an outlay when the object is to prevent the rapid *destruction* of human beings, and that too by a disease which, if it be not killed on the first attack, leaves no power to protect the constitution against a second, third, or fourth visitation?

We are urged, then, still to call for the institution of a moveable Clinical Commission, and we beseech the Government to reflect on what will be their feelings if, after the appointment has been delayed for months, it should then be instituted, and, in the course of its inquiries, either discover an effectual remedy for cholera, or lead to the general adoption of some successful remedy already discovered. May not the sacrifice of human life in the intervening period prove truly awful; and on whom would the responsibility fall? Why, on the bare, the unsheltered, heads of those men who commanded the means of protecting society from the infliction of such an annihilating scourge, but sluggishly and cruelly relying upon their own favourable position for security, neglected to put into operation such an instrument of inquiry, which if it did not lead to the actual discovery of a specific remedy for the disease, would have the salutary effect of inducing practitioners not to risk the lives of their patients by the employment of agents found to be not only worthless, but absolutely injurious.

THE APOTHECARIES ACT AND NAVY AND ARMY SURGEONS.

To the Editor of THE LANCET.

SIR,—Observing in the two last Numbers of THE LANCET, a discussion as to whether naval and military medical officers in the service previous to 1815 are exempt from the penalties of the Apothecaries Act, allow me to say, that being one of a joint committee appointed at that period by a public meeting at the Freemasons Tavern, of the medical officers of the United Service, I had, conjointly with my professional brethren on the committee, several interviews on that subject with Lord Palmerston, then Secretary at War, and Mr. Croker; and we were led to believe (and in this I hope I am not deceived), that A BILL was afterwards introduced by the latter gentleman, whereby all officers, soldiers, seamen, and marines, who had served his Majesty during the war, were to be allowed to carry on their profession, trade, or occupation, in any part of his Majesty's dominions, without let or hindrance, from any corporation or corporate body whatsoever, always saving and excepting the Universities of Oxford and Cambridge. The title or preamble of the Bill I do not recollect, but I am sure it is to be found.

The indignation of the United Service at that period was at its height, at the presumption of the Gogs and Magogs of Rhubarb Hall, in subjecting army and navy surgeons who had gone through the practice of a long war to the degradation of appearing before them for their "*license to practise,*" and for which they were to pay *fourteen guineas.*

I well remember that one or two who had so committed themselves were well nigh "sent to Coventry" by their brother officers. I am, sir, yours,

J. Gooch, Surg. R.N.

H.M. Ship *Ocean*, Sept. 1, 1832.

There may have been such a "bill," but we can assure Mr. Gooch, that no such *Act* is in force. The *Declaratory Act* of 1825, which was passed to protect naval and military surgeons from the operation of the *Apothecaries* Statute of 1815, expired in 1826, as he will find by the last clause but one.

"And be it enacted, that this Act (the Declaratory) shall take effect from and after the passing thereof (dated 6th July, 1825), and shall continue until the first day of August next, in the year one thousand eight hundred and twenty-six."

GRANT OF PUBLIC MONEY TO THE

NATIONAL VACCINE INSTITUTION.

To the Editor of The Lancet.

Sir,—Some years ago his Majesty's Government, under the auspices of Mr. G. Rose, judged it expedient to form a National Vaccine Institution, under their own direction (to be conducted by the President of the College of Physicians, the Master and two Governors of the College of Surgeons, Dr. Jenner, director, and Mr. Carpue, principal vaccinator), to vaccinate gratuitously all persons who presented themselves for the purpose, and Government has hitherto granted 2500*l.* or 3000*l.* per ann. for its support. The vaccinators selected besides Mr. Carpue, were, Messrs. Lease of Duke Street, Manchester Square; Aiken, of Broad Street Buildings; Lane, Guildford Street; Sawrey, Fleet Street; Vincent, West Smithfield; Ward, Wellclose Square; Gillam, Blackfriars Road; Harkness; Ratcliff; and others; but some of the above-named are deceased.

The Board of the establishment was held in Leicester Square. Nothing, however, is now known of its proceedings. We have heard of no report being published lately, and the profession at this time, in parts of the metropolis, have a difficulty to procure ichor of the paid vaccinators, who are now no one knows where, in consequence of removals, deaths, and sinkings into obscurity.

Will you permit me to ask, through the pages of your journal, how the 2500*l.* per annum is disbursed, and if in the parliamentary grants of last session it was continued, it appears to be a useless expenditure of public money. Neither the St. Pancras Hospital nor the London Vaccine Institution, which have equal claims, receive one shilling of it in aid of their funds, although they are establishments as well, if not better, known in the metropolis. They vaccinate and supply matter to more than ten thousand applicants annually. Yet their expenses are paid by public contributions.

During the late administration the parliamentary grant was opposed by Mr. Hume in the House of Commons when the supplies were voted; but possibly the letter from the president and censors of the College of Physicians to Lord Melbourne on the subject at that time was a consideration on the part of ministers.

If the grant be continued, why does not Government order a portion to be given to the two institutions above mentioned, instead of persisting in the present mode of distribution among private individuals by the conductors?

I am, Sir, your obedient servant,

W. W.

Sept. 1832.

NEW MEDICAL SOCIETY

IN SOUTHWARK.

To the Editor of The Lancet.

Sir,—On the other side I beg to hand you a circular now in distribution in this neighbourhood, and at the same time to congratulate the general practitioners of England upon the spirited and independant conduct of their brethren in Southwark. What will the Dubs and Pures say to this? I am, Sir, your obedient servant,

A General Practitioner
of Southwark.

Sept. 13, 1832.

At a meeting of the undersigned General Practitioners in Medicine, in Southwark and its neighbourhood, holden on Wednesday, the 12th day of September, 1832, at the apartments of the Southwark Literary Society, Chester Terrace, Borough-road, Southwark:—

Edward Evans, Esq., in the Chair,

Resolved unanimously,

That in consequence of the grievances constantly experienced by general medical practitioners, it is, in the opinion of this meeting, highly desirable and expedient to form a society for preventing breaches of medical etiquette, and infringement upon their privileges.

That this society shall consist of the general medical practitioners of Southwark and its vicinity, and shall be called "The General Medical Practitioners' Society of Southwark and its Vicinity."

That Mr. John Fenford Thomas, shall be Honorary Secretary to this society.

That these resolutions shall be printed, and forwarded to all the general medical practitioners in Southwark and its neighbourhood; and that their co-operation shall be earnestly requested.

Edward Evans, Chairman.

R. L. Hooper,
J. Coulthred,
J. Cox,
W. Eales,
J. Gellatly,
J. Howell,
J. W. Jones,
J. Ravis,
G. Simmons,
G. Skrimshire,
J. Taylor,
C. Waterworth

THE MEDICAL PRACTITIONERS OF LYNN.

To the Editor of The Lancet.

Sir,—An immediate insertion of the following into your journal will greatly oblige. In your Number for the 8th of this month, there appeared a letter from Mr. J. B. Whiting, of King's Lynn, relative to cholera, to which is added the following postscript:—

"In premonitory cases, the astringent and opiate plan recommended to the medical practitioners of this town by my brother Dr. Whiting, has been marked by the most decided success in arresting the progress of the disease, of which there have certainly been three, if not five importations."

We the undersigned medical practitioners of Lynn do hereby give this postscript our most unqualified contradiction, and totally disclaim having been influenced in our practice by any suggestions communicated by Dr. Whiting to his brother. What, we would ask, are the inferences naturally deducible from the above postscript? They are four.

First. That the medical men of Lynn made application to Dr. Whiting for his advice, in order to know how to stop premonitory diarrhœa.

Second. That he really and truly recommended *astringents* and opiates.

Third. That we the practitioners of Lynn implicitly followed his advice.

Fourth. That in consequence the most decided success ensued.

These are the deductions which would immediately occur to the mind of every reader.

Now, Sir, as it happened that we never stood in need of his advice, so we never thought of making application for it; but the inference giving rise to this observation is one of minor importance in comparison with the others.

The facts are these:—Mr. J. B. Whiting receives a private letter from his brother the Doctor, just when cholera broke out in the Borough of Southwark, and (doubtless with a good intention) he makes its contents public by showing the letter to *some* of the medical men of Lynn. In that letter they read that the disease, meaning cholera, although a "nova pestis," is of no moment if collapse be prevented: they also read of the ease with which this is to be effected by stopping the discharges with opium; of the necessity likewise which there is for a medical man to "go armed with his laudanum bottle and his lancet;" and, lastly, they read of a specific prescription, consisting of sixty drops of laudanum in one ounce of brandy, which it is stated will "often do the business if early given," meaning, we presume, that it will prevent collapse and cure the diarrhœa.

Now, Sir, Mr. J. B. Whiting has cautiously avoided informing the public that his brother recommended *stimulants*, which he clearly has done in the shape of pure brandy; but he *has made* him to recommend *astringents*, a thing the Doctor himself *has not* done, for from the beginning to the end of his letter, not one word is mentioned concerning *astringents* of any kind. He therefore has put language into the Doctor's mouth which he never used, has proffered a plan of treatment in his name which was known long before, and has made us followers of this plan, who never have been.

Mr. J. B. Whiting is fully aware that in the treatment of cholera in this town, there has been less brandy prescribed than in almost any town or village in the kingdom.

Mr. J. B. Whiting is also fully aware, that the great bulk of premonitory diarrhœa in Lynn has been met and cured by cretaceous, astringent, and aromatic medicines only, with the occasional use of laudanum, and that in small doses. How, therefore, could he so far forget himself as to tell the medical world that the medical practitioners of Lynn had been acting upon his brother's advice, and that the most decided success had attended the plan recommended by him; when he knew also at the same time, that his brother had advised large opiates, whilst they were for the most part not giving any, or only in very small doses; when he knew that the Doctor never mentioned nor advised astringents, whilst they and even Mr. J. B. Whiting himself were administering them largely, either alone or in conjunction with absorbent antacids as circumstances required?

In making the above statement public, we have not been actuated by any personal feeling whatever towards Mr. J. B. Whiting, and should not have made it at all but for his refusal to contradict the paragraph in question to our satisfaction, in a tone of contempt and defiance.

We further desire to state most explicitly, that although obliged to refer to Dr. Whiting's letter, in order to repel an unfounded and uncalled-for insinuation, affecting our professional characters as a body, yet that we entirely exculpate the Doctor from any participation in the deed, and intend not the least disrespect towards him.

John Tweedale, M. D.
John Wayte, M.D.
Thomas Ingle, Surgeon.
W. I. Bayes, Surgeon.
Thomas Bullen, Surgeon.
John Murlin, Surgeon.
Edward Hillyard, Surgeon.

Lynn, Sept. 15, 1832.

REPLY TO MR. WHITING.

To the Editor of The Lancet.

Sir,—An opportunity has been afforded me of perusing a letter addressed to you on the 14th inst. by the other medical gentlemen of Lynn. If you think proper to publish it, I request you will append, on my part, the following answer:—

When the cholera was declining in Southwark, before the disease had been seen in this town in its first or precursory stage, and when all were anxious for information regarding the "nova pestis," I received from my brother a letter, dated April 10, 1832, in answer to an application made by me to him at the request of some of his former patients for his opinion on the subject. That letter says, "But you are all too much alarmed; the disease is one of no moment if collapse be prevented, and this is easily done by stopping the discharges from the alimentary canal by opium." And again‘ "Let this be known by all in your town, and you will so mitigate the severity of this visitation that it will destroy but few lives." The writer then gives a very clear statement of the plan of treatment he was at that time in the habit of adopting in each succeeding stage, and for the next advance recommends 60 drops of laudanum in an ounce of brandy. My opponents, in a manifesto sent to me, bearing date Sept. 10th, expressly state that they believe this to refer "to the immediate rather than the premonitory symptoms," although they condescend to argue with me on a contrary supposition, which, however, they will not allow to be correct, and which in reality is not. Yet in their letter to you on the 14th inst., they change the tables, and boldly maintain, that to be true which to me they positively deny, at least with respect to the brandy. Thus they resemble the doctor and the digitalis, "Why did you give digitalis, Sir?" "I have given none, Doctor." "Then, Sir, why did you not?" Really it is difficult to reply to such chameleon gentlemen.

According to the request of the writer, the contents of the letter were made generally known,—almost every medical man in Lynn read it, some *copied* it, and all heard the particulars. The first two to whom I gave it were Dr. Wayte and Mr. Ingle. Each expressed his surprise at the information of the non-importance of the disease if the diarrhœa could be early stopped, and the latter gentleman returned it with a decided air of incredulity. Since that period I have had no correspondence with Dr. Whiting. In the sequel, when the disease got fairly among us, the premonitory diarrhœa was particularly attended to by every practitioner, some giving astringents, some absorbents, and all combining opiates when the urgency of the case demanded it, and with the most happy and extraordinary result. Under these circumstances, I trust the public will acquit me of any intention to deceive or to injure the reputation of my professional brethren. The facts speak for themselves, and of course every one will draw his own conclusions; mine are evident from the postscript of my letter to the Central Board, and I confess at present remain unaltered: and here the affair must rest. I am, Sir, yours obediently,

Jos. B. Whiting.

King's Lynn, Sept. 17, 1832.

DR. CLANNY AND HIS REVIEWERS.

To the Editor of THE LANCET.

SIR,—For three months I have been at different times engaged in ascertaining, by new processes, the relative proportions of free carbon in the blood of persons in whom the sanguineous system was affected, and the contrary. My memoranda are in train for publication, and as I have heretofore presented to you for insertion several papers upon the chemical pathology of the fluids of the human body, I shall continue so to do, though the uncalled-for severity, I had almost written asperity, with which my work on cholera was criticised, by interested individuals, in two London hebdomadal publications, almost deter me from making anything public upon this subject, except under the form of a separate publication. My desire has been to steer clear of personalities and of controversies; at the same time I beg to remark, that I value not personal invective or critical vituperation, knowing that those who deal most largely in this way are the least likely to give satisfaction to their readers, or to afford assurance that, in turn, they may escape that which all honourable men detest, viz., ill-natured animadversions. In conclusion I assert, without fear of contradiction, that what I have heretofore written on chemical pathology is just and correct. I remain, Mr. Editor, your most obedient, very humble servant, W. REID CLANNY.

Sunderland, Sept. 15, 1832.

LATE INQUEST AT PORTSEA.

To the Editor of THE LANCET.

SIR,—I have read the letter inserted in THE LANCET of the first instant, under the title "Non-Medical Coroners;" and I think it right to observe, that your correspondent has mis-stated the proceedings at the inquest, as to what was said by the coroner to the jury, and also as to the verdict.

The coroner did *not* state to the jury "that he had no power to order an inspection," nor did he ever entertain such an opinion.

The evidence of the two witnesses who saw the accident, was so clear as to the cause of the death of the deceased, that it was entirely unnecessary to open the body, and as the parents of the child (who are Jews) were exceedingly averse to it, such examination would have been doing violence to their feelings without any sufficient justification.

Being only a casual reader of THE LANCET, I did not see the letter in question until within these few days. I have, therefore, to request you will insert the above reply in the next number of your LANCET, and you will oblige, your obedient servant,

F. HOWARD,
Coroner for the Borough of Portsmouth
and the liberties thereof.

Portsmouth, 15th Sept. 1832.

ST. THOMAS'S HOSPITAL.

SUCCESSFUL ADMINISTRATION OF PURE NICOTINE, DISSOLVED IN DILUTE SULPHURIC ACID, IN AGUE.

SIR,—I take the liberty of sending you the three accompanying cases, trusting that you will give them publicity through the medium of your widely extended journal, and with the hope that some of my professional brethren may be induced to try the remedy, and report through the same medium how far their experience corroborates my own. I have the honour to be, Sir,

Your humble servant,
H. S. ROOTS.

84, Guilford-street, Russell-square,
September 18, 1832.

It is now between six and seven years since I was engaged in St. Pancras Infirmary, in conjunction with my colleague Mr. Wright, in a series of experiments on the different principles resident in opium, more especially morphia, narcotine, and meconic acid; and having (at least to my own satisfaction) convinced myself that morphia was the only substance which contained the soporific, narcotic, stimulating and constipating powers of that drug; and having found that, in opposition to what has been stated by Dr. Magendie, pure narcotine might be given even in scruple doses to an adult, whether dissolved in oil, sulphuric ether, or acetic acid, without the slightest deleterious result; and having in several instances administered five grains of pure narcotine dissolved in oil, and the same quantity in acetic acid, to two different dogs without any manifest effect,—it was my intention to have offered these experiments to the public. Circumstances, however, prevented my so doing, and it was in the spring of the present year only that my attention was recalled to the subject, by finding that pure narcotine dissolved in dilute sulphuric acid, possesses a bitterness fully as intense as that of the sulphate of quina, and indeed very similar as to its permanency in the mouth. Accordingly, I resolved on trying it as a substitute for that remedy on the first opportunity, in the treatment of ague, and with what success the following three cases will show.

CASE I.

April 5, 1832, David Cox, ætat. 25, a gardener, was admitted into Luke's ward in St. Thomas's Hospital. Lives at Bermondsey, near the river, and states that he had ague ten years ago when living in Hampshire. Has now been ill fourteen days with intermittent fever. Paroxysm every other day, at first about half-past seven in the morning, but has been gradually coming on earlier at each attack, so that the last, which occurred on the 3rd, came on at nine o'clock at night, and he thinks the paroxysms have increased in severity. Expects to shake to-night; has considerable pain in the head during the fit, and some during the intervals, accompanied by a confused sensation of noise in the head. Bowels open; tongue foul; appetite bad; pulse 104, moderately strong.

Let him take four grains of narcotine, dissolved in dilute sulphuric acid directly. Let it be repeated at five o'clock in the afternoon, and every six hours after. Let him take, in the morning, sixteen grains of rhubarb, with four of calomel. Let him have house diet.

6. Had a slight paroxysm last evening, but with much less pain of head, and has lost the sensation of noise. Bowels have operated only very scantily. To take half an ounce of castor oil directly, which operated freely, and having continued the narcotine without any recurrence of the paroxysm, and feeling perfectly well, he was dismissed on the 18th, with a supply of medicine, which he was directed to take twice a day for one month.

CASE II.

May 17, 1832. J. Parrott, aged 81, was this day admitted into Luke's ward of St. Thomas's Hospital. States that he had ague five years ago, of which he was cured in this hospital. Has now been ill three weeks with the same complaint. Paroxysm every other night nearly at the same time, namely, between ten and eleven o'clock, attended by considerable pain of head during the fit, and always by rather violent delirium, occasional pain of head during the intervals; bowels open; motions dark-coloured; appetite bad; tongue foul; pulse natural.

Let him take four grains of narcotine, dissolved in dilute sulphuric acid and water, every six hours; let him have sixteen grains of rhubarb, and four of calomel, to-morrow morning, and every alternate morning. Milk diet.

18. No fit yesterday; bowels freely open; feels comfortable. Let him continue the narcotine.

19. A fit last evening at the usual time, with delirium.

Let him have ten leeches to the temples, and continue the medicine.

21. Fit returned last evening at the usual time, and with delirium.

Let him take the narcotine every four hours.

23. Had a fit last evening with some delirium, but considerably slighter; bowels freely opened by the powder, and secretions natural. Let him omit the rhubarb and calomel; let him take six grains of narcotine every four hours, and a dose of common house physic, if the bowels are confined. To have house diet.

30. Has had no return of the paroxysm; but the bowels are rather confined, as he cannot take the house physic.

Let him have twelve grains of compound extract of colocynth directly, with three of calomel, and let him take the six grains of narcotine every six hours instead of every four.

June 2. No return of paroxysm, and feels quite well. Let him take the narcotine every eight hours.

7. Discharged perfectly well, with directions to continue his medicine for one week.

CASE III.

June 29, 1832. John Anderson, aged 20, a glassblower, states that he has been ill with ague one month; was first attacked whilst travelling up from Birmingham to London after sleeping on the damp grass. Paroxysm every other day, at first it used to come on about 4 or 5 o'clock in the afternoon, but latterly an hour sooner at each recurrence; has no appetite; tongue foul; bowels open; sleeps tolerably.

I did not see him until the next day, the 30th, when he had a severe paroxysm. Let him take eight grains of narcotine dissolved in dilute sulphuric acid and water every six hours. Let him have 16 grains of rhubarb and 4 of calomel every other morning. Let him have milk diet.

July 2. Had a slight paroxysm this morning, but much slighter than that on the 30th.

3. No paroxysm. 6. None.
4. None. 7. None.
5. None. 8. None.

Was discharged on the 12th, on account of some ill behaviour, but feeling quite well, having had no return since the 2d of July. I may add that I have prescribed the narcotine in similar doses in three other cases of intermittent fever, with equal success;

and that I have given it in some few cases of dyspepsia, in smaller doses, with apparent advantage, but of course it can only be expected to prove advantageous where the bitter tonics are admissible.

The narcotine used by me was prepared most accurately by Mr. Morson, of Southampton-row, and the formula for its solution was as follows :—

℞ *Narcotinæ*, gr. xxxii ;
Acidi sulph. dilut. fl. ʒiss ;
Aquæ distillat. ʒviss.

So that each fluid drachm contains 4 grains of narcotine, with about 11 minims of dilute sulphuric acid, though sometimes a less quantity will be sufficient to dissolve it.

H. S. Roots.

PROFESSOR YOUATT'S LECTURES.

When we last year commenced the publication of Professor Youatt's lectures on Veterinary Medicine, we fully expected that we should be enabled to publish the entire course by the conclusion of the present volume. Feeling a highly commendable anxiety that the lectures should be presented to the profession and the public in as perfect a form as circumstances would permit, Professor Youatt undertook not only to be his own reporter, but to examine the proof-sheets. This was part of our amicable arrangement ; thus the *copy* was not at our disposal almost as soon as the words had escaped from the lips of the lecturer,—as has been the case with us in other instances,—and we could not, of course, publish the lectures more quickly than the copy was supplied to us by the Professor himself. Having stated thus much, we are sure that both the lecturer and editor will stand wholly excused from any charge of neglect, or breach of faith, when the reader has perused the following letter to the Eitor :—

"My dear Sir,—I have not forgotten what you endeavoured to impress on my mind, when you first did me the honour to propose the publication of my lectures in The Lancet, and also your pledge to your readers, that the reports of my lectures delivered at the University of London during the session of 1821-2, would not extend beyond the volumes belonging to that session.

"I had stipulated, at the beginning, that I should be my own reporter ; not that I doubted the general accuracy of your reporters, but because they were not veterinarians. Neither you nor I, however, could then foresee the almost constant ill-health and increasing avocations which prevented the punctual transcription of the lectures, and more particularly that laboured condensation of them which both of us had contemplated. Your second volume is near its close, and my task is not completed. I have, nevertheless, brought you to *a resting-place*, and where your reports *may*, not improperly, terminate, and I thank you for having afforded me the opportunity of showing, through the medium of your excellent and widely-circulated journal, that the degraded, and I will maintain, undervalued, profession of the veterinary art, are at length endeavouring to tread that sure road to skilful practice and reputation which the human surgeon has long pursued. I thank you for having allowed me the opportunity of showing that we have it in our power to do something towards the knowledge of general physiology, and more for the elucidation of the grand principles of pathology.

"If, however, a report of the remaining lectures of that session, including the diseases of the nervous system, and particularly the nature and symptoms of rabies in brutes, and also the anatomy and pathology of the digestive system, would be considered interesting to your readers, they shall be regularly forthcoming, and you shall break no second pledge ; but I must crave a respite until the end of the next month. I am, my dear Sir, your faithful and obedient servant, "W. Youatt.

"3, Nassau street, Middlesex Hospital,
Sept. 18, 1832."

Fully impressed with a conviction of the great value and importance of these really practical and scientific lectures, and being at the same time particularly desirous that our readers should not be deprived of the opportunity of possessing the entire course, we most gladly avail ourselves of the Professor's liberal offer to supply us with the unpublished discourses, amounting, we believe, to about forty in number, including the diseases of the nervous system, and an elaborate discourse on the nature and treatment of rabies.

Having determined to continue the publication of these lectures, the next question is the *form* in which they should be given. This probably can be most conveniently and economically arranged by their appearing in *supplements*, to be had either *with* or *without* the current number of the week. This course is the fairest to adopt in respect to our readers, because those gentlemen who may have commenced taking The Lancet for the sake of obtaining possession of Mr. Youatt's discourses will now be enabled, if they should see fit, to discontinue the journal, and at the same time procure the lectures in monthly supplements until the whole are completed. The supplements will amount, we should think, to about *six*. Thus the remainder of the course will be placed at the option of the possessors of the previous lectures at a charge of about four shillings. It is but right to observe that the promise made in the first instance to publish the whole course in a period embracing not quite ten months, could not by any possibility have been fulfilled without printing them in supplements to the full amount of the number now contemplated. The first supplement will be ready for delivery towards the end of November, and as we apprehend that those readers who are in possession of the first part of the course would wish for the remainder, this *first* supplement will be annexed to the number of The Lancet for that period, unless there may be a specific request to the contrary. On the other hand, where demanded, the supplement will be supplied *separately* to any extent; thus both the old and the new readers will be relieved from the necessity of purchasing matter which they may not require.

The pages of the supplements will be starred thus (2*), and there will be a separate index, hence they may, or may not, be bound up with the present volume.

LITERARY INTELLIGENCE.

Mr. George Rogerson, Surgeon, of Liverpool, has nearly ready for publication a Treatise on Inflammations; containing their Pathology, Causes, Consequences, and Treatment, with their Effect on the various Textures of the Body: being an extension of a "Dissertation on Inflammation of the Membranes" to which the Jacksonian prize for the year 1828 was awarded by the Royal College of Surgeons in London.

TO CORRESPONDENTS.

Medicinæ Discipulus. Certainly. According to the *words* of the Act, but possibly not agreeably with the intentions of its framers, every student who "practises anatomy," must obtain a "license" from the Secretary of State for the Home Department. Without such license the party "practising anatomy" will be liable to three months imprisonment, or a fine of fifty pounds.

A Surgeon. Such prosecutions have been instituted, and with invariable success to the prosecutors. Druggists are *not* liable to the penalties of the Apothecaries Act for dispensing and prescribing in their own shops, and this is one of the gross anomalies of that detested measure.

Amicus. The resolution has *not* been forwarded by the Secretary of the Court of Examiners. In relation to the "service" conferred on the occasion to which *Amicus* refers, as nothing was expected or desired, there was no disappointment on nothing being offered. Since naval medical officers are unquestionably entitled to rank with legally-qualified practitioners in surgery, so are they eligible candidates for the Fellowship of the London College of Medicine.

B. C. There is a vacancy in that quarter, and we should not object to communicate, either personally or by letter, on the subject of filling the office.

Enquirer. A circular calling a meeting of the Bat Club has been issued, for some day in *October*, but we have not yet heard the day. Our invisible reporter, however, will not fail to take an account of the proceedings.

Castigator is really too personal. The venom of reptiles may be extracted without cutting them to pieces.

Students, generally, will find a few words of precautionary admonition, applicable to the commencement of the new medical session, in our next Lancet; the *decoys* of the Bats, we understand, are already on the wing. Let them take care of their pockets, and beware of the *traps* in Guy's Hospital. The Borough students should enter for hospital practice at St. *Thomas's* Hospital, where there is some punctuality, and where the pupils are at any rate received and treated as gentlemen. Besides, an entrance to the surgical practice at St. Thomas's entitles the student to witness that of Guy's, and it is to be regretted that there is not a similar arrangement with regard to the *medical* practice of the two hospitals.

A General Practitioner. The refusal to pay would most probably have involved the victim in an action at law twice as expensive as the enormous fee, and the costs of which action would very likely have gone with a verdict even of five pounds.

F. W. The papers are too numerous. We shall not have room for them.

Surely such another noodle is not to be found as the writer of the paper signed *A Philosopher*. He has adopted a masquerade name. The mantle is under his feet, not on his shoulders.

We have received the account of the "Cholera in the Gorbals." Also Mr. Radcliffe's communication, and the account of the cholera from Bethnal Green, which we will use.

Perhaps *Mr. E. Lloyd* can favour us with his observations in writing.

J. S. H. A reply was written long since, and unintentionally omitted. The paper was not inserted, because it did not contain any facts which had not either already been amply discussed, or possessed inferior claims to publication. We shall always, however, be glad to hear from our correspondent when his researches afford him matter worth communicating.

Zeta's attack is unjust. The medicine may be dear, but it is not the poor who alone are afflicted, and the argument that a remedy for a disease should not be made known because its price will speedily be doubled, is one which need not be published. If the proposal be inefficient or inconvenient, it will soon find its grave without the trouble of fretfully killing it.

END OF VOL. II.—1831-2.

INDEX

TO

VOLUME II.—MDCCCXXXI—XXXII.

A

Abscess in the meatus auditorius, 348 ; loins, 350 ; phlegmonous, of the thigh, 446 ; in the axilla, 702.

Acid, prussic, effects of, on the crane-fly, 116 ; hydrogen, experiments on, 215 ; nitric, or sulphate of indigo, as a test for, 302 ; oxalic, new test for, 401 ; hydrocyanic, in cases of vomiting, 441 ; carbonic, in venous blood, 662 ; citric, in scurvy, value of, 764.

Acidity of the saliva, 313.

Acids, mineral, employment of, 350.

Act, Apothecaries, a specimen of imbecile legislation, 698 ; extracts from the, 699 ; for schools of anatomy, copy of the, 712.

Act, Apothecaries, effect of, on the privileges of naval surgeons, 793.

Act, the Declaratory, operation of the, 794.

Action, animal, Mr. Dobson's Essays on, 17, 46, 144, 202.

Acupuncture in rheumatism, Dr. Elliotson on, 489, 672.

Addison, Dr., reflections on the chemical knowledge of, 95 ; vindication of, 114.

Ague, Dr. Roots' cases of nicotine in, 799.

Air in the heart, after venous injections, 440 ; risk of admitting in venous injections, 441.

Alexander, Dr. J., on inflammation of the spinal chord, 111.

Alkaloid principle, Mr. Bird's discovery of a new, 172.

Ammonia, hydrosulphuret of, in disease of the heart, 213.

Amputation, spontaneous, of the fœtal limbs, 175 ; affection of the median nerve after, 252 ; of the leg, 221, 511 ; arm, and forearm, 223 ; penis, 315 ; toe, 315.

Anasarca and ascites, cases of, 689.

Anatomists, Dr. Somerville's circulars to, 716.

Anatomy-Bill, remarks on the passing of the, 537 ; effect of, on the supply of skeletons, 670 ; Act, revised, copy of the:—licenses, appointment, duties, and salaries, of inspectors, 713, 717 ; regulations respecting dead bodies, 714 ; bodies of murderers ; limitation of actions ; offences against the act, 715 ;

Anatomy, morbid, facilities for its study shamefully neglected by hospital surgeons, 24 ; contrast of Mr. Lawrence's "principles" and "practice" on, 25 ; pathological, improvement of, in England, 702.

Ankle, diseased bones, of the, 314.

Anderson, Dr., cases of venous injection by, 369 ; letter from, 597.

Andrews, Mr., letters respecting, 765.

Aneurism, circumscribed brachial, cured by compression and digitalis, 33 ; cases of, 174 ; compression in, 175 ; of the subclavian, cases of, 119 ; axillary, treated by ligature of the subclavian, 120 ; in the chest, diagnosis of, 201 ; inguinal, case of, 261 ; of the aortic arch, 221 ; aorta descendens, 507 ; popliteal, treated by compression, 510.

Angina pectoris, arsenic in, 244.

Antlers of deer, 385.

Aorta, aneurism of the, 214 ; of the arch of, 221 ; descendens, aneurism of the, 507.

Apothecaries Act, institution of *inquisitions* under the, 309 ; a perfect jumble, 698 ; remarks on certain clauses of the, 699.

Apostates and Bats, *Castigator* on, 631.

Arm, amputations of the, 223 ; violent separation of from the body, 114.

Arsenic, alternate use of with colchicum in chronic rheumatism, 131 ; effects of, as a medicine, 132 ; curious case of poisoning by, 133 ; in angina pectoris, 244 ; in tic doloreux, 617.

Arsenious acid, test for, 614.

Artery, lingual, death from hemorrhage from the, 255.

Arteries, effect of ligature of on union in fractures, review of, 167; Manec on the ligature of, 664; obliteration of, 665; rules to be observed in ligature of, 691; on torsion of, 692.

Arum maculatum, discovery of a new alkaloid principle in the, 172.

Ascites and anasarca, singular case of, 1.

Astringents, use of, in cholera at Lynn, 795; as preventives of cholera, 751.

Auricle, left, tuberculous masses in the, 215.

Auscultation, value of to veterinarians, 37, 99; tabular view of the signs of, 121; Dr. Billing on, 198; advantages of in cases of vomiting of blood, 205; in phthisis, Dr. Billing on, 424; Sharpe's Translation of Laennec's Manual of, 540; and percussion, Mr. Chavasse on, 760.

Ayre, Dr. J., on the pathology and treatment of cholera, 43; on the malignant cholera, 105; on small and frequent doses of calomel in cholera, 271, 458, 615.

Axilla, abscess in the, 702.

B

Barry, Sir D., on the cholera in Coldbathfields prison, 455, 492.

Barry, Madame de, bequest of her body for anatomical purposes, 669.

Bats, attempts of the, to put down the London University, 658.

Bayonet, fatal punctured wound by a, 305.

Bed, hydrostatic, description of, 746.

Bedingfield, Mr. E., on gangrene of the mouth from mercury, 304.

Bellows sound, remarks on, 67, 199, 213.

Beriberi, cases of, and remarks on, 740.

Biggs, Mr. T., reply of to Mr. Costello, 464, 598.

Billing, Dr., on auscultation, and treatment of affections of the heart, 198; clinical lectures by:—on scurvy and its appearances, 70; on chronic hepatitis, 72; on diseases of the heart, 327; diseased heart with dropsy, 327; operation of hydragogues; disease of the heart with acute inflammatory affection, 329; hypertrophy of the heart with severe diarrhœa; enlargement of the heart produced by acute rheumatism, 330; tubercular consumption, 421; nature and variety of tubercles, 422; advantages of auscultation, 424; petechial fever, 426, 521; hysteric diseases, 518; amenorrhœa; vicarious menstruation, 520; concluding remarks to the course, 522.

Bird, Mr. G., on the discovery of a new alkaloid principle, 172.

Birmingham School of Medicine, anniversary dinner of the, 351; prizes at the, 351.

Birtwhistle, Mr. R., on the cholera at Leeds, 671; on citric acid in scurvy, 765.

Bismuth, nitrate of, in gastrodynia, 136.

Bladder, destruction of stones in the, by percussion, 234, 362, 462, 493, 531, 567, 570, 624; female, case of stone in the, 499; fracture of a catheter in the, 442; fracture of a bougie in the, 767; cases of stone in the, treated by lithotrity, 649, 679.

Bleeding, employment of in cholera, on board the *Circassian*, 718.

Blistering in cholera, effect of, 629.

Blood, Clanny's experiments on the, 154; composition of the, in diabetes mellitus, 117; Dr. Elliotson on vomiting of, 205; Dr. Thomson's analysis of the, in cholera, 225, 251; Dr. Clanny's ditto, 232, 251; accumulation of, in the uterus, 382; Dr. O'Shaughnessy's new analysis of the, 603; state of in healthy persons during the cholera epidemic, 603; experiments on the, 623; review of Stevens on the, 659, 722; cause of the red colour of, 661; effect of oxygen on the, 724; nature of the globules in, 724.

Blotches, scorbutic and syphilitic, on the distinction between, 70.

Board of Health, charges against the, 22; sanitary regulations of the, 652, 657; non-responsibility of, for the theories of their correspondents, 701; circulars of the, 759; remarks on circulars of the, 762.

Boddington, Mr. B., on a stroke of lightning, 742.

Bone, diseased, treatment of, 313.

Bones of children, fractures of the, 116; the ankle, disease of the, 314.

Booth, Mr. A., on a new mode of preparing pure sugar, 599.

Botanic Garden, Chelsea, a morning at the, 416.

Botany, "sacred," M. Virey on, 217.

Bougie, fracture of a, in the bladder, 767.

Bougies, propriety of leaving in the urethra, after amputation of the penis, 770.

Brain, hydatid in the fourth ventricle of the, 168; in quadrupeds, Mr. Youatt on the, 417; chemical composition of the, 449.

Brains of domesticated animals, 449.

Breast, scirrhus of the, 30; cancer of the, 286.

Bree, Mr. C. R., on cold water in cholera, 722.

Bronchitis in horses and cattle, 99; with disease of the left ventricle, 591.

Brodie, Mr., clinical remarks by on,—the cause of fracture of the olecranon;

division of the sphincter ani, 35; convulsions from injuries of the head; bleeding from the ear; injury of the spinal chord, 36; salivary fistula; amputation of the inferior maxilla, 37; use of mezereon; continuance of stimuli to drunkard patients; injury to the spine; compound fracture of the leg; hemorrhage, 166; erysipelas; ventilations in St. George's Hospital, 167; chronic inflammation of the testicle; use of mercury therein, 168; pulsating tumours not always aneurismal, 263; ligature of the external iliac; its origin, 264; operation, 265; secondary aneurismal hemorrhage, 266; new treatment of ranula, 312; division of inflamed bursæ; wounds in dissection; diseased bone; acidity of the saliva; pulsating tumour in the neck, 313; enlargement of the thyroid gland; disease of the ankle bones, 314, 414; cure of fistula; division of sphincter ani; stricture instruments; disease of the knee, 314; abscess in the meatus auditorius, 348; mode of opening abscesses in the loins; mineral acids; disease of the lower jaw; turpentine in rheumatism; old customs in medicine; fractured radius and dislocation of the wrist, 350; dislocation of the metatarsal bone of the great-toe, excision of the bone, 351; fracture of a catheter in the bladder, 442; fracture of the neck of the femur, 443; nature, causes, and treatment of inflammation, 477; ligature of the thyroid artery for enlargement of the thyroid gland, 479; abscess in the axilla; fungoid testicle; disease of the urinary organs, 702; lithotomy; necrosis, 703; appointment of, as serjeant-surgeon to the King, 764.
Brougham, Mr. S., on a case of calculus in the female bladder, 499.
Brown, Mr. H. I., letter from, 736.
Bruit de soufflet, causes of, 67; remarks on the, 213.
Bruits, nature and causes of the, 199.
Bryan, Mr. E. L., on a case of osteo sarcoma, 336.
Burn, case of, 315.
Bursæ, inflammations of, 313.

C

Calculi, extraction of, by Weiss' urethra-dilator, 770.
Calculus, extraction of a, from the female bladder, 499; extraction of a, by an Indian lithotomist, 656.
Calculi, vesical destruction of, by percussion, 234, 362, 462, 493, 531, 567, 570, 624; new mode of bringing away fragments of, 296, 366; Mr. Costello's cases of, 430, 493, 649, 679.
Caldwell, Dr., on hydrocephalic operations, 741.
Calf, thymus gland of the, 342.
Calomel, small doses of, in cholera, 271, 460, 615; exclusive use of in cholera, 721.
Cancer of the breast, 286; review of Aldis on, 311.
Carbonic acid in venous blood, 662.
Carcinoma of the uterus, successful treatment of, 717.
Carotid, common, ligature of the, in epilepsy, 648.
Carruthers, Dr., on venous injection in cholera, 461.
Carson, Mr. J., on the mode of action of the pneumogastric nerve, 13.
"Castigator," letter from, to Dr. Ryan, 631.
Cataract, operation for, 447.
Catarrh, in brutes, 10; epidemic amongst horses, Mr. Youatt's remarks on, 77; recent treatment of, 117.
Catheter, for bringing away calculous fragments, 297; Heurteloup's evacuating, cases illustrative of, 366; fracture of a, in the bladder, 442.
Cattle, Swiss, peculiar character of, 197; diseases of (see Youatt, Mr.)
Caustic, use of potassium as a, 454; lunar, use of in erysipelas, 738.
Charter of the College of Physicians; abuses of the, 694.
Chavasse, Mr. P. H., on auscultation and percussion, 760.
Chelsea Botanic Garden, a morning at the, 416.
Chlorine, new mode of obtaining, 216.
Chlorosis, severe case of, 590.
Cholera, the malignant:—on the preliminary diarrhœa and consecutive fever common to, in India and England, 14; Mr. Simpson, on the identity between it and the "congestive fever" of Armstrong, 19; discussions on, at the Westminster Medical Society, 21, 51, 85, 146; contagion of, 21, 62, 125, 146, 150, 178; appearance of, in France, 27; review of Green's Anatomical Pathology of, 27; Dr. J. Ayre on the pathology, and treatment of, 43, 105; ravages of in Paris, 61; Mr. Prebble's cases of, 125, 621; Dr. Green, on the anatomical pathology of, 83; Mr. Turner's suggestions for raising the temperature of the body in cases of, 94; remarks on the opinions of THE LANCET on, 122; mode of propagation, 124; abolition of hospitals for, 125; appearance of at Havre, and in Ireland, 126; Dr. Gilkrest's opinions on, 127; review of Lizars' Anatomy of, 150; summary of twenty dissections in cases of, 151; review of Clanny on, 153; experiments on the blood in, at

Sunderland, 155 ; on the phenomena of in London, 156 ; on the vote against the contagion of, 157 ; Dr. Knight's claims to originating saline treatment in, 170; Mr. Miskin's treatment of; Mr. Protheroe Smith's account of his own case of, 171 ; editorial opinions on the contagion of, 178; Mr. Tweedie's cases of, 178 ; Dr. Heberden's paper on, 183 ; Mr. Wilkinson's supposed cases of, 191 ; Dr. Johnson's views of the propagation of, 209; Nicole's idea of animate contagion in, 216 ; Dr. O'Shaughnessy on the chemical pathology of; Dr. T. Thomson's analyses of the blood in, 226, 251 ; Dr. Clanny on his analysis of the blood in, 232, 251; Dr. Lewins on venous injections in, 243 ; Mr. J. Waters on the alleged discoveries of Delpech in the pathology of, 266 ; Dr. J. R. Lynch on the origin, progress, and treatment of at Paris, 268 ; Dr. Weatherill on cases of at Liverpool, 270 ; Dr. J. Ayre on the efficiency of calomel in, 271, 458, 615, 640 ; on the treatment of, by copious venous injections, by Drs. Latta, Lewins, Craigie, and Mackintosh, 275, 286 ; Dr. O'Shaughnessy's proposal to combine stimulants and astringents with the saline injections in, 281 ; treatment of, in Turkey, 281 ; editorial remarks on injections in, 284 ; appointments to the hospital of, at Dublin, 304 ; case of, at Cawston, 338 ; Mr. Fergus on the origin, progress, and treatment of, in Vienna, 353, 393 ; Mr. T. J. Murphy's cases of, at Liverpool, 368 ; cases of injections in, by Dr. Anderson, 369 ; Dr. Lewins, 370, and Dr. Latta, 371, editorial remarks on the quack " remedies" for, 377 ; suggestions for investigating the saline treatment of, 378 ; adoption of the saline treatment of, in the Marylebone Infirmary, 379 ; Dr. Venables' case of injection in, 398 ; Dr. Latta's reply to strictures on the injection treatment of, 428; editorial remarks on Dr. Stevens's saline treatment of, 437 ; cases of in the Coldbath-fields prison, 438 ; occurrence of amongst the poultry at Choisi-le-Roi, 439; Dr. Green on air in the heart after injection in, 440; Mr. Little's case of treated by saline, and subsequently spirituous, injections, 454 ; proposal to transfuse blood after injections in, 455 ; papers relating to the cases of at Coldbath-fields prison, 455, 492, 503 ; Dr. Carruther's case of venous injection in, followed in three days by the birth of a child, 461 ; apparatus for injecting in, 491 ; remarks on the supposed cases of in Coldbath-fields, Dr. Stevens's saline treatment, 503 ; death of Mr. J. Wood from, 512 ; saline injection treatment of not warranted by the pathological condition of the blood, by J. M'Cabe, 528 ; unsuccessful trial of Dr. Stevens's saline treatment in, 530 ; Barbadoes naphtha in, 530 ; Mr. A. Robertson on the treatment of, on board the *Cumberland*, 557 ; Dr. Wright on the treatment of, at St. Margaret's, Westminster, 559 ; case of, treated with corrosive sublimate and acetate of lead, by Mr. Hart, 561 ; Mr. Tytler on mercurial frictions in, 562 ; letter on the consumption of fruit during the prevalence of, 566 ; remarks on the reappearance of, in London ; reasons for renewing the reports of cases of, 571 ; document of the market-gardeners respecting, 572 ; Mr. Tod on large doses of calomel in, 585 ; stimulants injurious, efficiency of calomel and counter irritants, and danger of bleeding in, 587 ; Dr. Girtwood's cases treated by venous injection, 594 ; Mr. Harrison on the treatment of, in the guards, 597 ; Dr. Anderson on venous injections in, 598 ; remarks on Dr. Girtwood's cases of, 602 ; Dr. O'Shaughnessy's new experiments on the chemical pathology of, 603 ; state of, in London, 604 ; proposal to adopt serum for venous injections in, by Mr. H. Searle, 619 ; Dr. Hancock, on disinfection by heat, and saline treatment, and opium in, 620 ; modifications of, in England, 627 ; deviations from the general symptoms of, 627 ; debility or deficient nourishment not predisposing causes to ; invariable symptoms in ; sulphate of copper or nitrate of silver in, 628 ; blistering in ; nitrous oxide in, 629 ; rapid spread of in England, 636 ; Mr. Dobson on large doses of calomel and opium in, 651; Board of Health's sanitary regulations respecting, 652 ; and remarks thereon, 657 ; Mr. Birtwhistle on the treatment of, at Leeds, 671 ; of its irruption amongst the sixty-eighth regiment at Clare ; probable immediate cause of its origin, 681 ; failure of the saline injections in ; explosion of gunpowder for purification of the air during prevalence of, 683 ; Dr. Shute's memorial on the internal use of cold water in, 685 ; new mode of friction in, 687 ; four hundred and eighty ounces of fluid successfully injected into the veins in a case of, by Dr. Weatherill, 688 ; non-responsibility of the Central Board for the published theories of their correspondents, 701 ; treatment of on board the *Circassian* convict ship, with cases, 718 ; successful employment of cold water in, by Mr. Whiting, 719; treatment of with calomel, opium, brandy and hydrocyanic acid, by Mr. Foster, 720; treatment of with calomel, by Dr. Stanley, 721; on the use of tin,waer cold 722; progress of in Great Britain, and suggestion for a travelling commission to investigate modes of treatment in, 731; variety of the treatment of, 732; brief summary of the treatment of, 735; treatment of, by comp. infus. of horse-radish, 747; Mr. G. Meikle on venous injections in, 748; Dr. Wayte on the prevention of, 751; case of three days after parturition, 752; treatment of with the nitric acid bath, 753; Mr. Laming on musk in, 754; Mr. M'Divett on purgatives in, 754; Board of Health's circular, 759, and remarks thereon, 762; proposed moveable board for investigating the treatment of, 763; administration of oil in, by Dr. T. O. Ward, 773; Dr. Shute on cold water in, 774; Drs. Venables and Pidduck on salt water emetics in, 778; Mr. J. B. Steward on the treatment of in Droitwich asylum, 780; Mr. Langford on tartar emetic in, 781; Dr. Tweedie on cholera in London, 782; further remarks on the appointment of a moveable clinical commission, 791; on arrest of by the astringent treatment at Lynn, 795.

Chord, spinal, concussion of the, 93; inflammation of the, 111.

Chorea, case of, 207.

Children, fractures of long bones of, 116.

Circassian convict ship, treatment of cholera in the, 718.

Clanny, Dr., on his analyses of the blood in cholera, 232.

Clanny, Dr. W. R., letter from, 797.

Clare-castle, irruption of cholera amongst the soldiers at, 681.

Clay, Mr. R., letter from, correcting a statement of Dr. Ayre, 173.

Climate, effect of change of, on brute animals, 197.

Colchicum employed alternately with arsenic in rheumatism, 131.

Cold, on the application of in convulsions, 176; in various complaints, 176.

Coldbath-fields-prison, treatment of cholera in the, 438; papers relative to the cholera at, 455; Sir D. Barry on the cholera in, 492; remarks on Dr. Stevens's treatment of the cholera in, 503.

Colds, external application of emetic tartar in, 544.

College of Physicians, papers and proceedings at the——; Dr. Heberden's paper on cholera, 183; Sir H. Halford on phlegmasia dolens in males, the Earl of Liverpool's case, 184; Dr. Samuel M'Duffal on the treatment of cholera at Constantinople, 281; Dr. Heberden the elder, on perspiration, concluding conversazione, Harveian oration, 506; petition against abuses of the, 693, 701; anomalies in the petition against the, 761.

College of Surgeons, worthlessness of the diploma of the, 736.

Colon, Mr. Salmon's lecture on stricture of the, 102.

Commission to investigate Dr. Stevens's "saline treatment," proposal of a, 378; moveable, clinical proposal of a, 731; remarks on, 762.

Compression, successful use of in circumscribed brachial aneurism, 33; in the treatment of aneurism, remarks on, 175.

Concussion of the spinal chord, 93, 605; post-mortem appearances in, 212.

Constantinople, treatment of cholera at, 281.

Constipation and vomiting, absence of in intus-susception, 647.

Consumption in brute animals, nature and treatment of, 193; tubercular, Dr. Billing on, 421; Dr. Elliotson on tanner's liquid in, 593.

Contagion in cholera, discussions on, 21, 51, 146; evidences of, 62; remarks on, 125; Dr. Gilkrest's opinions on, 127; *vote* passed against, at the Westminster Med. Soc., 145, 150; curious version of the doctrine of, 155; remarks on the Westminster *veto* against, 157; editorial remarks on, 178; Dr. Johnson's view of, 209; limitation of by quarantine, 375; evidences of, 557; effect of heat in destroying, 620.

Contractility of the spleen, 766.

Copper, sulphate of in cholera, 628.

Cornea, hydriodate of potash in nebulæ of the, 501.

Corollaries, Manec's surgical, 691.

Coroner, non-medical, curious opinion of a, 92; freaks of, 693.

Coroner of Portsmouth, letter from the, 797.

Coronerships, non-medical, on the anomalies connected with, 92.

Corrosive sublimate and acetate of lead in cholera, 561; test for, 614.

Costello, Mr., correspondence of, and with, on Heurteloup's percussion instrument, 362, 430, 462, 531; on cases of stone, with lithotritic operations, 679.

Cough, chronic, in brute animals, 12.

Cow-pox, Dr. Venables on, 331, 374.

Cow's milk, proposal to inject the serum of, in cholera, 619.

Cox, Dr. W. T., on the cholera at Cawston, 338.

Craigie, Dr., notice of the pamphlet of, 574.

Crampton, Surg.-Gen., communication of cholera reports by, 681
Crane-fly, effects of prussic acid on the, 116.
Cranium, fracture of the, 190, 251, 605.
Croton oil, in cholera, 773.
Cusack, Mr., on a case of circumscribed brachial aneurism, 33.
Cut throat, cases of, 255, 704.
Cyanurets, experiments on the, 215.

D

Dagleish, Mr. G., on a case of hydatid in the fourth ventricle of the brain, 168.
Debility, not a predisposer to cholera, 628.
Delirium tremens, Mr. Whittaker on, 392.
Dermott, Mr., on the anatomy bill, 670.
Divett, Mr. M., on purgatives in cholera, 754.
Diabetes mellitus, composition of the urine and blood, in, 117.
Diaphragm, hernia of the, 115; Mr. Dobson on the, 202.
Diarrhœa, preliminary, common to the cholera of England and Asia, 14; nitrate of iron in, 766.
Dickson, Mr., on a case of ventral pregnancy, 396,
Digitalis and compression, successful use of, in circumscribed brachial aneurism, 33; in diseased heart, effect of, 329.
Dilatation, artificial, on the use and abuse of, in midwifery, 48.
Dinner, medical, at Birmingham, 351.
Dinners, "hospital," farces of the, 218.
Diploma of the College of Surgeons, worthlessness of the, 736.
Disinfection by heat, letter from Dr. Henry respecting, 92.
Dislocation of the wrist, 350; of the metatarsal bone of the great-toe, 351.
Dissecting-rooms, filthy state of some, 320; facilities afforded for, on the continent, 56.
Dissection-wounds, cause of pain in, 313.
Dissections of cholera subjects, summary of twenty, 151.
Distortions of the spine, review of Stafford on, 210.
Dobson, Mr. W., essays by, on animal action, 17, 46, 144, 202, 294, 404; on arsenic in tic doloreux, 617; Sir R., on a case of stone, 335.
Dog, hydrophobia from the bite of a, 446; anatomy and diseases of the (see Youatt, Mr.)
Droitwith Lunatic Asylum, treatment of cholera in, 780.
Dropsy, universal, Dr. Elliotson on a singular case of, 1; and disease of the heart, Dr. Billing on, 327; cases of, 689.
Dublin Journal, review of the, 116, 174.
Dublin, Cholera Hospital, juvenile appointments to the, 304, 629; College of Surgeons, illiberal by-law of the, 491.
Dubs, end of the system of, 600.
Duncan, Dr. A, death of, 249; short memoir of, 250; on the election of a successor to, 251.

E

Ear, abscess in the, 348; anatomy and physiology of the, 728.
Edinburgh, Med. and Surg. Journal; note from Dr. Gregory; 160; Hunterian Med. Society, meeting of the, 187; University, on Dr. Duncan's successor at the, 251.
Education, medical, observations on, 632.
Edwards, Mr. D. O., on a case of diaphragmatic hernia, 115; communications from, 234, 335, 462; on the name *lithotripsy*, 296; lecture by, on lithotripsy, 345.
Elaterium, on the employment of, 327.
Electricity, evolution of, from magnetism, 246; experiments on, by Faraday and others, 306, 523.
Elliotson, Prof., letter from, 670; clinical lectures by, on cases occurring at St. Thomas's Hospital:—Case of anasarca with singular morbid appearances, 1; saline treatment of fever, 65; cases of pericarditis, 66; palsy from lead, treated with strychnine, 69; disease of the external surface of the liver, stomach, intestines, &c., with absence of symptoms, 129; chronic rheumatism, treated alternately with arsenic and colchicum, 131; on the effects of arsenic, 132; curious case of poisoning by arsenic, 133; on the diseases and sounds of the heart; case of Dr. Hopkins, 204; advantages of auscultation in cases of vomiting of blood, 205; diagnosis between hæmoptysis and hæmatemesis, 206; case of cholera; case of hysteria, 207; hysteria, with extreme tenderness of the surface of the body; deceptions practised by hysterical patients, 208; varieties in epilepsy; partial and imperfect, 486; value of the muriate of morphia, 487; rheumatism, acute, 488; atonic, 489; acupuncture in rheumatism, 489; purpura hæmorrhagica, 490, 589; porrigo lupinosa; eczema, 513; jaundice; gastrodynia, 514; abscess of the labium; hypertrophy of the left ventricle and auricle, 515; scirrhus uteri—malignant cholera, 515; inflammatory leucorrhœa, 589; chlorosis; bronchitis; disease of the left ventricle of the heart, 590; inflammatory headache and epistaxis, 592; hypertrophy

the left ventricle of the heart, 593; phthisis; inhalation of tanner's liquid; conclusion of the course, 593.

Emetic, tartar, in cholera, 781.

Emetics, salt-water, in cholera, 778.

Epidemics, malignant, amongst horses, accounts of, 82.

Epilepsy, varieties of, 486; singular cases of, 487, 507; ligature of the common carotid in, 648.

Epistaxis, inflammatory, case of, 592.

Epps, Dr., on valerian, 180.

Ergot of rye, new test for, 401.

Erysipelas, check to, at St. George's Hospital, 167; Mr. Keate's treatment of, 167; successful use of mercurial ointment in, 480, 739; practical remarks on, 737.

Espy, Mr. J., on meteorology, 553.

Essays on animal action, 17, 46, 144, 202, 294, 404.

Examination, scholastic, mode of, adopted by Dr. Elliotson, 180.

Examinations, post-mortem, irregularity of, at St. Bartholomew's, 25.

Evidence before the "Anatomy Committee," extracts from the, 56.

Evidence, medical, in the case of Kinnear v. The Rock Insurance Office, 500.

Eyes, congenital deficiency of both, 169.

F

Fainting, method of friction in, 687.

Femur, fracture of the neck of the, 443.

Fergus, Mr. J. F., on the cholera at Vienna, 354, 393.

Fergusson, Dr. D. B., note from, 54.

Fees of hospital surgeons, enormity of, 764.

Fever, consecutive, common to the cholera of Asia and England, 14; trial of the saline treatment in, 65; the "congestive" of Armstrong, similarity of to malignant cholera, 19; petechial, Dr. Billing on, 427.

Fevers, malignant, opium in, 622; state of the blood in, 726.

Fibula and tibia, compound fracture of the, 190.

Fistula, the "horse-shoe," 223; cases of, 314, 767; recto-vaginal, case of, 510.

Flooding, value of the plug in, 380; from irritation of the rectum, case of, 690.

Fluids, crispations of, on vibrating surfaces, 283.

Fœtus, spontaneous amputation of the limbs of the, 175.

Foster, Mr. W. P., on a case of malignant cholera, 720.

Fox, Mr. L. O., on a case of spinal irritation, 183; hydrophobia from the bite of a fox, 445.

Fracture of the cranium, 190; compound of the tibia and fibula, 190, 253, 316; of the skull, 251; compound, of both legs, 448; of the thigh, 511; of neck of the femur, 443; olecranon, 639; of long bones in children, 116; prevention of union in, by taking up arteries, 167; treatment of, in St. Bartholomew's, 318.

Franking of medical journals, proposal for, 256.

Friction, proposed mode of employing in cholera, 687.

Friction, mercurial, in cholera, 562.

Fungus of the testicle, 702.

G

Gangræna, pulmonum, 254; oris, from mercury, 158, 304.

Garrie, Dr. J., on a case of intus-susception, 647.

Gases, escape of, through membranes, 661.

Gastrodynia, use of magnesia in, 136.

Gin-drinkers, effect of low diet upon, after accidents, 166.

Girtwood, Dr., on venous injections in cholera, 594, 603.

Gland, thyroid, enlargement of the, 479.

Glands, mesenteric, Dr. Elliotson on disease of the, 129.

Glans penis, phagedenic ulcer of the, 768.

Glasgow Infirmary, admirable regulations at the, 600.

Glysters, tobacco, efficacy of, in carcerated herniæ, 787.

Gooch, Mr. J., letter from, respecting the Apothecaries Act, 793.

Gooch, Mr. J., on erysipelas, 737.

Goodwin, Mr. G., reply of, to Mr. Costello, 463, 598.

Goss, Mr. Weston, on inoculation for small-pox in Devon, 383.

Granville, Dr. Bozzi, execution of, 604.

Green, Dr., review of his anatomical pathology of the cholera, 27; on the anatomical pathology of cholera, 83; on air in the heart after venous injection, 440.

Gregory, Dr. W., note from, 160.

Guards, treatment of cholera amongst the, 597.

Gunpowder, explosion of, for circulation of the air, 683.

Guy's Hospital, cholera in, 22.

H

Halford, Sir H., twistings of, 764.

Halma Grand's styptic, failure of, 315.

Hancock, Dr. J., on the disinfecting power of heat, 620; note from, 689; on nitre, 766.

Hancorn, Mr. J. R., on uterine hemorrhage, 617.
Hand, wound of,—hemorrhage from the palmar arch, 189.
Harrison, Mr., on the cholera in the Guards, 597.
Harrison, Dr., and others, petition of against the abuses of the Coll. of Phys., 693, 701 remarks on the petition of, 761.
Hart, Mr. F. J., on corrosive sublimate, and acetate of lead in cholera, 561 ; note from, 672.
Harvey, Dr. A. A., on a case of carcinoma uteri, 717.
Head, injury to the, 607, 316.
Headache, inflammatory case of, 592.
Hearing, physiology of, 730.
Heart, cause of bruit de soufflet in disease of the, 68 ; hypertrophy of the, 187 ; with enlarged cavities, 330 ; of the left ventricle of, 593 ; Dr. Billing on affections of the, 198 ; causes of the beat and sounds of the, 198 ; hypertrophy of, with and without dilatation, 199 ; causes of, 200 ; Dr. Elliotson on diseases and sounds of the, 204 ; sudden rupture of the, 214 ; disease of the, with dropsy, 327 ; with acute inflammatory affection, 329 ; enlargement of, from acute rheumatism, 331 ; hypertrophy of left ventricle of the, 593 ; Dr. Green on air in the, 440.
Heat, disinfection by, 92 ; disinfecting power of, 620.
Henry, Dr., reply of, to Mr. Wallace, 92
Hepatization of the lungs, remarks on, 6.
Hepatitis, chronic, Dr. Billing on, 72.
Hernia, diaphragmatic, singular case of, 115 ; strangulated femoral, 316, 444 ; cerebri, 605.
Hernia, entero-vaginal, description of, 788 ; interesting cases of, 789, 790.
Herniæ, old, not easily strangulated, 787.
Heurteloup, Baron, on cases of lithotripsy, 234 ; on cases of stone treated by percussion, 493, 430, 567, 624.
Hiley, Mr. J. S., on some new chemical tests, 401.
Hip, disease of the, 607.
Hoare, Mr., letter from, respecting his history of a man who would not speak to males, 20.
Hæmatemesis, colour of the blood in, 206.
Hæmoptysis and pneumonia, remarks on, 6 ; diagnosis of, 206.
Hemorrhage from the palmar arch, 189 ; from the lingual artery, 255 ; secondary aneurismal, 266 ; uterine, review of Ingleby on, 379 ; uterine, cases of, 373, 617 ; modes of treating, 692.
Hone, Dr., professional acquirements of, 203.
Hook, elastic prehensile, to supply the absence of the forearm, 187.
Hoose, or cough in cattle, danger of, 195.
Hopkins, Dr., Prof. Elliotson's remarks on the case of, 204.
Horns of quadrupeds, Mr. Youatt on, 321.
Horse, anatomy and diseases of the, (see Youatt, Mr.)
Horseradish, infusion of in cholera, 747.
Horses, malignant epidemics of, 82.
Hospital of the Lond. Univer., proposed erection of, 376 ; North London, address respecting the, 376 ; operations, failures of, 630 ; surgeons, enormous fees of, 764.
Hospitals, neglect of autopsies at, 24 ; cholera, injudicious breaking up of, in London, 125.
Howard, Mr. F., letter from, 797.
Hunterian Med. Society, (Edinburgh,) meeting of the, 187.
Hudson, Mr., on nitrous oxide in cholera, 629.
Hydatid in the fourth ventricle of the human brain, case of, 168.
Hydragogues, operation of, in disease of the heart, 327.
Hydrochlorate of tin, antiseptic properties of, 771.
Hydrocephalus, operations for, 741.
Hydrogen acid, experiments on, 215.
Hydrops articuli, case of, 288.
Hydrophobia from a fox bite, 445 ; the bite of a dog, 446.
Hydrostatic bed, Arnott's description of, 746.
Hydruret of sulphur, Thenard's remarks on, 541.
Hyperanthraxis, Dr. Clanny's work on, 153.
Hypertrophy of the large viscera accompanying dropsy, singular disposition to, 2 ; of the heart with its cavities enlarged, 187 ; Dr. Billing on, 199 ; of left ventricle of the heart, 593.
Hysteria, Dr. Seymour in cases of, 134 ; Dr. Elliotson on, 208 ; with great tenderness of the surface, case of, 208 ; deceptions of patients in, 209.

I

Iliac, external, ligature of the, 263.
Improvisatoires, education of, 248.
Indigo, sulphate of, as a test for nitric acid, 302.
Inflammation of the spinal chord, case of, 111 ; chronic, of the testis, Mr. Brodie's remarks on, 168 ; Mr. Brodie on, 477 ; review of Jameson, 539 ; of the tongue, 639.
Infusoria, on the structure and functions of the, 126.
Ingleby, Mr. I. T., on a case of uterine hemorrhage, 373.
Inspections, morbid, value and neglect of, 24 ; in cholera, by Dr. Green, 27.

Institutions medical, connexion of, with old toryism, 220.

Injections venous, in cholera, Dr. Lewin's cases of, 243; Dr. Latta on, 275, 370; Dr. Lewins on, 277, 279, 370; Dr. Craigie on, 277; Dr. O'Shaughnessy on the combination of stimulants and astringents with, 281; historical account of, 285; remarks on, 284, 379; trial of, in Liverpool, 368; Dr. Anderson's cases of, 369: Dr. Venables' case of, 398; reply to objections against, 428; air in the heart after, 440; followed by spirituous, 454; proposal to transfuse blood after, 455; Dr. Carruther's case of, 461; apparatus for, 491; not warranted, 528; failure of, 557, 683; Dr. Girtwood on, 594, 602; failure of, 683; successful case of, 688; Mr. Meikle's cases of, 748; effects of, 766.

Insanity, cure of, 629.

Inspectors of anatomy, duties of, 717.

Institution, Royal (see Royal Institution).

Insurance money, important action to recover, 468.

Intestines, extensive disease of the external surface of, 129.

Intoxication, a predisposer to cholera, 754.

Intus-susception, without vomiting or constipation, 617.

Iodine, Mr. Brodie's opinion of, 703.

Iron, carbonate of, in neuralgia of the mamma, 177; elasticity and bearing properties of, 186; nitrate of, in diarrhœa, 766.

J

Jackson, Mr. W., on a case of melæna, 497.

Jalap and scammony, distinction between the resins of, 216.

Jaw-bone, upper, extirpation of a tumour from the, 30; lower, disease of the, 350.

Jenner, Dr., reward of for his discovery, 793.

Johnson, Dr. J., on the propagation of cholera, 209; note from, respecting Dr. Ramadge, 441.

Joint, insertion of the needle in a, 672.

Jones, Mr. R., on arsenic in angina pectoris, 244.

Juice, gastric, application of, to medicinal purposes, 305.

Jurisprudence, medical, trial important in, 468.

Jury, coroner's, singular verdict of a, 28.

K

Keate, Mr., and the eel-backed Sir Henry, 764.

Kidneys, disease of the, 702.

Kilby, Mr. R., on external application of emetic tartar in colds, 544.

Kinnear, Mrs., v. Borrodaile and others, 468; v. the Rock Insurance, remarks on the evidence in the case of, 500.

Knee joint, disease of the, 221; disease of the, 288, 315.

Knight, Dr. P. S., on the saline treatment of typhus and cholera, 170; on alkaline salts in gastric irritation, 300.

L

Laming, Mr. R., on musk in cholera, 751.

LANCET, note from Dr. O'Shaughnessy on the sub-editorship of the, 84.

Larvæ, in and near the horns of animals, 324.

Laryngitis, in the horse, 37.

Langford, Mr., on tartar emetic in cholera, 781.

Latta, Dr., on the treatment of cholera by aqueous and saline injections, 275, 370; reply of, to strictures on venous injection, 430; notice of the pamphlet of, 574; note from, 640.

Lawrence, Mr., his inattention to morbid inspections at St. Bartholomew's, 25.

Lead, case of palsy from, treated with strychnine, 69; combination of opium with the superacetate of, 95, 113; acetate of, combination of, with opium, 384; in cholera, 561.

Lee, Dr., on the structure of the human placenta, 406.

Legislation, medical, anomalies of, 698.

Leith, professional dissensions at, 574.

Lemon-juice, value of, in scurvy, 727.

Le Neve, Mr., on horseradish in cholera, 747.

Lepra vulgaris, case of, 257.

Leucorrhœa, inflammatory, case of, 589.

Lewins, Dr., on venous injections, 370.

Libels on general practitioners, 634.

Library regulations at the London Hospital, abuses in the, 30.

License to practise anatomy, form of application for, 716.

Ligature of the penis, 136; of nævus maternus, 222; of the external iliac, 263; of common carotid artery in epilepsy, 648; of arteries, Manec on the, 664, 691; eligibility of the small round, 665; where preferred to torsion, 692.

Lightning, effects of a stroke of, 742.

Life, animal, experiments on the laws of, 185.

Lithotrity, review of Mr. King's work on, 122; alteration of the name, 234, 237; Heurteloup's instrument in, 362, 430; simple method of evacuating the fragments after, 650.

Lithotripsy, adoption of the name, 296; Sir R. Dobson on, 335; history, origin, and progress of, 345; controversy respecting, 598; great success of, 679; general applicability of, 679.

Lithotrite, cure of stone by a single application of the, 649, 679.

Lithotomy, fatality of, at St. George's, 245; by an Indian operator, 656.

Little, Mr. W. J., on saline and spirituous injections in cholera, 454.

Liver, on chronic inflammation of the, 72.

Liverpool, case of the late Earl of, 184.

Loins, abscess in the, 350.

London, reappearance of cholera in, 571.

———— College of Medicine, bright prospect of the, 340; inadvertent testimony in favour of the, 503; prospects of the, 697; rank of army and navy medical officers in the, 699, 701; letter on the prospects of the, 697; equality of rights enjoyed by Fellows of the, 701.

——— Hospital, Mr. Andrews's speech at the, 704; letter on ditto, 736; Mr. Brown's note respecting the speech, 736.

——— University, medical examinations at the, 179; Hospital suggestions to the council respecting the, 411.

——— Hospital, clinical lectures at, (see Billing, Dr.).—Reports of cases at:—scirrhus of the mamma operation, 30; fatal cut-throat, 30; curious injury,—fracture of the skull, 190; compound fracture of the tibia and fibula, 190, 253, 316; injury to the side, 191; compound fracture of the skull with depression, 251, 252; affection of the median nerve after amputation, 252; nævus maternus, 287; hydrops articuli, 288; burn; amputation of the great-toe, 315; injury to the thigh; injury to the head, 317, 607; removal of the testicle, 318, 544, 608; strangulated femoral hernia, 444; hydrophobia following the bite of a fox, 445; hydrophobia following the bite of a dog, 446; phlegmonous abscess of the thigh, 446; operation for cataract, 447; compound fracture of both legs, 448; recto-vaginal fistula, 510; fracture of the thigh; amputation for diseased knee; amputation of the leg after fracture, 511; femoral aneurism—operation, 544, 608; fracture of the humerus, 544; diseased hip; strumous ophthalmia; strictured urethra, from external violence; fracture of the olecranon, 638; inflammation of the tongue, 639; retention of urine, 703; cut-throat, 704; phagedenic ulcer of the glans penis, 768. Abuse in the library regulations at the, 30; a *mouvement* in the system at, 55, 60; charge of inaccurate reporting, 96; reply thereto, 191; election of Dr. A. Frampton, 566.

Lunar caustic, use of, in erysipelas, 738.

Lungs, Dr. Seymour on hepatisation of the, 6; inflammation of the, with hæmoptysis, 6; of cattle, structure of the, 7; physiology of, 8; diagnosis of diseases of the, 121; inflammation of the, in horses, 137; value of auscultation in, 139; in cattle, 143; œdema of the, 224; gangrene of the, 254; disease of the, 421; stethoscopic signs of diseases in the, 760.

Lynch, Dr. J. R., on the cholera in Paris, 268.

Lynn, letter from the medical practitioners of, respecting Mr. Whiting's letter, 795.

M

Macfarlane, Dr., on a case of imperforate vagina, 625.

Macnee, Mr. J. W., on cases of dropsy, and flooding, 689; on cases of Beriberi, 740.

M'Cabe, Dr. on venous injections, 528.

Magnetism, evolution of electricity from, 246; experiments on, 527; as the effect of lightning, 743.

Malingering, singular cases of, 135; supposed case of, 166.

Malformations, visceral, in Smithers, the murderer, 540.

Mamma, scirrhus of the, 30, 286; relief of neuralgia of the, by the carbonate of iron, 177.

Manec's singular corollaries, 691.

Maw, Mr. J. H., on venous injection apparatus, 491.

Market-gardeners, communication from the, 566, 572.

Marshall, Mr. W. B., letter from, 127.

Mash, Mr. J., on a fatal case of punctured bayonet-wound, 305.

Maxilla, superior, removal of an osseous growth from, 30; osteo sarcoma of the, 336; removal of part of, 337.

Meath hospital, distribution of prizes at the, 320.

Mechanism of respiration, remarks on the, 145.

Medicine, London College of (see London Coll. of Medicine).

———, pretended improvements in, 632.

Meikle, Mr. G., on venous injection, 748.

Melæna, Mr. Jackson on a case of, 497.

Membrana tympani, description of, 728.

Mercury, gangrene of the mouth following the abuse of, 158, 304; in ointment, success of in erysipelas, 480.

Meteorology, importance of in the etiology of diseases, 553, 575.

Mezereon, use of in periostitis, 166.

Midwifery, on artificial dilatation of the vagina in, 48.

Midwifery, review of Dr. Davis on, 788.

Milk, secretion of in cholera, 7; proposal to inject the serum of in cholera, 619.

Millard, Mr., on blistering in cholera, 620.

Monopoly corporate, deplorable results of, 26.

Morphia, muriate of, in epilepsy, 487.

Mouth, mortification of the from mercury, 158, 304.

Murphy, Mr. T. J., on the cholera in Liverpool, 368.

Muscles, on the motions of, 17; structure and properties of, 18; contraction of, 46; intercostal, observations on the, 145.

Musk in the treatment of cholera, 751.

Mustard plaster in cholera, 656; emetics, in cholera, efficacy of, 557.

N

Naphtha, of Barbadoes, in cholera, 530.

Narrative of a man who would not speak to males, 20.

National Vaccine Institution, grant of public money to the, 791.

Navy surgeons, degrading classification of, 312, and army surgeons, comparative advantages of, 542.

Neck, pulsating tumour in the, 313; sarcomatous tumour of the, 509.

Necrosis of the tibia operation for, 703.

Needle, acupuncture, insertion of the, in a joint, 672.

Negroes and whites, difference of temperature in, 552.

Nerve pneumogastric, mode of action of the, 13; median, affection of the after amputation, 252.

Neuralgia of the mamma relieved by carbonate of iron, 177.

Nicotine, pure, successful use of in ague, 797.

Nitrate of silver in cholera, 628, in vaginal discharges, 618; iron in diarrhœa, 766.

Nitre, Dr. Hancock on, 766.

Nitrous oxide in cholera, 629.

Nævus maturnus, ligature of, 222; treated by ligature, 287.

Nose, polypus of the, 767.

Nourishment, deficiency of not a predisposer to cholera, 628.

Nurse, singular revenge of a, 136.

O

Œdema pulmonum, with aortic aneurism, 224.

Oil, croton, in cholera, 773.

Ointment, mercurial, successful use of in erysipelas, 480.

Oke's Examinations, review of, 311.

Olecranon, fracture of the, 639.

Osteo-sarcoma of the upper maxilla, 336.

Opium, on the combination of with superacetate of lead, 95, 113, 384; exhibition of, in large doses, 175; good effect of in a singular case of ptyalism, 176; new test for, 403; in malignant fevers, Dr. Hancock on, 622.

Ophthalmia, strumous, case of, 607.

O'Reilly, Dr., on the original country of the potato, 575.

O'Shaughnessy, Dr., notes from, 81; on Dr. Thompson's analyses of blood in cholera, 225; on aqueous injections in cholera, 281; on sulphate of indigo as a test for nitric acid, 302.

Oxalic acid, new tests for, 401.

P

Palsy, from lead, treated with strychnine, 69.

Paralysis, 606.

Paracentesis, operation on horses, 161.

Paris, irruption of the cholera at, 27; Dr. Lynch on the cholera in, 268.

Parliament, reformed, on the influence of the profession in a, 340.

Pathology, anatomical, of the cholera, 27, 43, 83; Mr. Waters on the, 267; of cholera, Dr. Ayre on the, 105; chemical, of cholera, Dr. O'Shaughnessy on the, 225; contributions to, by Dr. Alexander, 111; surgical, Cloquet's, translated, 701.

Penis, ligature of the, 136; vascular sarcoma of the, 315; phagedenic ulcer of the, 768.

Penis, injury, and amputation, of the, 769.

Pennington, Mr., and his tory toasts, 219.

Percussion, tabular view of the signs of, 121; treatment of stone, 624.

Percuteur courbe à marteau, observations on the, 567.

Perfumes, sacred, 217.

Periodicals, medical, proposal for franking, 256.

Periostitis, mezereon in, 166.

Pericarditis, two interesting cases of, compared, 67.

Perspiration, Heberden's paper on, 282.

Petition of Dr. Harrison and others, against the Coll. of Phys., 693; remarks on the, 701, 761.

Pharmaceutical Society of Paris, liberality of the, 215.

Phlegmasia dolens, Sir H. Halford's paper on, 184; in the male, case of, 185, 637.

Phrenology of the horse, 421.

Physicians, Royal College of (see College of Physicians).

Pidduck, Dr., on salt-water emetics in cholera, 778.

Pills, stony hardness of, at St. George's, 349.

Placenta, hemorrhage from attachment of the to the cervix uteri, 373; Dr. Lee on the structure of the, 406.

Plants, sacred, 217.

Pleuræ, effusion between the, 188.

Pleurisy in horses, 161; cattle, 165 ; dogs, 165 ; curious case of, 165.

Pleuritis, with effusion between the pleuræ, 188.

Plug, employment of in uterine hemorrhage, 373 ; value of the, in cases of flooding, 380 ; efficacy of the, in uterine hemorrhage, 617.

Pneumatosis thoracis, 254.

Pneumonia, in horses, 137, cattle, 143, swine, 144, dogs, 144 ; value of auscultation in, 144.

Poisons, mixed, Orfila on, 614.

Polypus narium, case of, 767.

Porteous, Mr. W., on the treatment of cholera in the *Circassian*, 718.

Potassium, use of as a caustic, 454.

Potash, hydriodate of, in nebulæ of the cornea, 541.

Potato, original country of the, 575.

Poultry, irruption of cholera amongst, 439.

Practitioners, medical, detention of, as witnesses, 96 ; general, libel on, 634.

Prebble, Mr., cases of cholera by, 62.

Pregnancy, ventral, case of, 396 ; frequent cause of abortion in, 381.

Preston, Mr. J. R., on ligature of the carotid artery in epilepsy, 648.

Prizes, distribution of, at the London University—at the Birmingham School of Medicine, 351.

Profession, remarks on the causes of the present distressed state of the, 88.

Prostate gland, disease of the, 702.

Phthisis pulmonalis, in horses, 193 ; in cattle, 195 ; Dr. Billing on, 422; case of, with recoveries, 423 ; marked case of, 424 ; "laryngea," 426 ; tanner's liquid in, 593.

Ptyalism, spontaneous, effect of opium on, singular case of, 176.

Purgatives, use of, in cholera, 754.

Purpura hemorrhagica, 490 ; Dr. Elliotson on, 589.

Q

Quackery of "regulars," the bane of the profession, 88; in cholera medicines, 377.

R

Radius, fracture of the, dislocation of the wrist, 350.

Ramadge *v.* Wakley, 408 ; remarks of the press on, 433.

——— *v.* Ryan and others, 408; remarks on the trial, 409 ; letter from defendant in, 496; note from Dr. Johnson on the cause, 441.

Ranula, new treatment of, 312.

Rectum, remarks on the diseases of, 182 ; Mr. Salmon on simple and malignant stricture of the, 238 ; case of flooding from irritation of the, 690.

Reform, parliamentary, effects of, on medical institutions, 309 ; medical remarks on, 339 ; medical, necessity for exertion in, 384.

"Repository," envy of the sexton of the, 91.

Resins of scammony and jalap, distinction between, 216.

Respiration, influence of the pneumagastric nerve in the mechanism of, 13 ; Mr. Dobson on the mechanical action of, 144, 404 ; experiments on, 185 ; physiology of, 294 ; changes in the colour of blood during, 662.

Retention of urine from enlargement of the prostate, 703 ; interesting case of, 703.

Revenge, curious instance of, 136.

Reviews, of: — Green's Anatomical Pathology of the Cholera, 27 ; Dublin Journal, No. 1, 116 ; Townsend's Tabular View of the Signs furnished by Auscultation and Percussion in Pulmonary Diseases, 121 ; King's Lithotrity and Lithotomy compared, 122 ; Lizars' Anatomy of Cholera, 150 ; Clanny's Hyperanthraxis or the Cholera in Sunderland, 153 ; Dublin Journal of Medical and Chemical Science, No. 2, 174, 213 ; Stafford on the Injuries, Diseases, and Distortions of the Spine, 210 ; Journal de Pharmacie Avril 1832, 215 ; Astley Cooper on the Thymus Gland, 342 ; Ingleby on Uterine Hemorrhage, 379 ; James on Inflammation, 539; Sharpe's Translation of Laennec's (Jun.) Manual of Auscultation, 540 ; Stevens, Denis, and Lecanu, on the Blood, 660, 722 ; Manec's Treatise on the Ligature and Torsion of Arteries, 664 ; Tod on the Ear, 728.

Review of Clement's observations in surgery, 783.

Review of Davis's Principles and Practice of Obstetric Medicine, 788.

Rheumatism, connexion of, with pericarditis, 67 ; chronic, treated with arsenic and colchicum alternately, 131 ; a cause of enlargement of the heart, 330 ; turpentine in, 350 ; acute, case of, 488 ; atonic, 489 ; acupuncture in, 672.

Rhubarb Hall, examination of students at, 309.

Roaring in the horse, causes and treatment of, 39 ; hereditary predisposition to, 42.

Robertson, Mr., on cholera, 554.

Roots, Dr. H. S., on cases of ague cured by pure nicotine, 797.

Royal Institution, papers and proceedings at the ;—Mr. T. Bell, on the structure and functions of the infusoria, 126; experiments relating to the laws by which animal life is regulated; irritability and respiration, 185; elasticity and bearing properties of cast-iron. 186; prehensile elastic hook, 157; Mr. Faraday on volta-electric induction, and the evolution of electricity from magnetism, 246; education of improvisatoires, 248; Dr. Richie's repetition of Faraday's magnetic experiments; sparks from magnets, 283; Mr. Faraday on the crispations of fluids upon vibrating surfaces, 283; Mr. Edwards on the origin, progress, and present condition of lithotripsy, 345.

Ryan, Dr., himself the undisputed author of his legal misfortune, 436; and Co., reasons of the verdict against, 473, 476; important omissions in the defence of, 475; letter from, on the trial, 496.

Rye, ergot of, new test for, 403.

S

Saliva, acidity of the, 313.

Salmon, Mr. F., on stricture of the colon, 102; on stricture of the rectum, 218; note from, 221.

Sarcoma, vascular, of the penis, 315.

Sarsæ, Hudson's syrup of, 765.

Salt, effect of, on venous blood, 663.

Salts, alkaline, of, in cases of gastric irritation, 300.

Salt-water emetics in cholera, 778.

Scammony and jalap, distinction between the resins of, 216.

Scarification in erysipelas, 738.

Scarnell, Mr., on a case of violent dismemberment of the arm, 114.

Scarlatina, clinical remarks on, 5.

Scirrhus of the mamma, case of, 30.

School of medicine, Birmingham, dinner and prizes of, 351.

Schools, foreign, remarks on the facilities afforded for studying morbid anatomy at, 55.

Scurvy, Dr. Billing on, 70; value of lemon-juice in, 727, 764.

Searle, Mr. H., proposal of, to inject serum of cow's milk in cholera, 619.

Shute, Dr. Hardwicke, on the internal use of cold water in cholera, 685.

Seymour, Dr., clinical remarks by;—on scarlatina, 5; on hepatisation of the lungs, 6; on inflammation of the lungs with hæmoptysis, 7; on phthisis and its varieties, treatment, &c., 75; cases of hysteria, 134; gastrodynia, 136.

Shute, Dr. H., on cold water in cholera, 774.

Sigmond, Dr., imputed neglect of duty by, 77; letter from, 128.

Silver, nitrate of, in vaginal discharges, 618.

Simpson, Mr., on the identity of cholera, and Armstrong's "congestive fever," 19.

Skeletons, effect of the anatomy bill on supply of, 670.

Skull, fracture of, in a curious manner, 190; fractures of the, 251, 252.

Sligo, fatal ravages of cholera at, 763.

Small-pox, on vaccination as a modifier of, 331; varieties of, 333; contagion of, 334; inoculation for, in Devonshire, 383.

Smith, Mr. P., on his own case of cholera, 171.

Smithers, the murderer, visceral malformations in, 540.

Society, Medical, establishment of a new, in Southwark, 791.

Society, medical, formation of a new, 437.

Solanum tuberosum, inquiry into the origin of, 575.

Somerville, Dr. J. C., circulars of, 716.

Sounds of the heart, Dr. Billing on the, 198; Dr. Elliotson on the, 204.

Southwark Cholera Hospital, attendance at the, 160.

Southwark, establishment of a medical society in, 794.

Sparks, magneto-electric, 283.

Spina bifida, 210; Abernethy's treatment of, 212.

Spine, concussion of the, 93, 105; remarks on affections of the, 113; case of injury to the, 166; review of Stafford on the, 210; p. m. appearances in concussion of the, 212.

Sphacelus of the mouth from the use of mercury, 158.

Sphincter ani, division of the, 314.

Spleen, contractility of the, 765.

Stanley, Dr. J., on calomel in cholera, 721.

Stevens, Dr., strictures on, 504.

Steward, Mr. J. B., on the cholera at Droitwich Lunatic Asylum, 780.

Stricture, spasmodic, of the urethra, 785; chronic, 785.

Stimulants, on the use of in catarrh, 117; spirituous administration of to gin-drinkers after accidents, 166.

St. Bartholomew's Hospital, reports of cases at:—removal of an osseous growth from the superior maxillary bone, 30; concussion of the spinal chord, 93; syphilitic disease—fumigation, 94; diseased knee joint, amputation, 221; ligature of a nævus maternus, 222; fistula in perineo; amputations of arm and fore-arm, 223; cut throat, hemorrhage from the lingual artery, death; cut throat, 255; cancerous breast, extirpation, 286; cases of lepra vulgaris, 287; intruders in the operating theatre at, 31, 287; neglect of post-mortem

inspections at, 31, "annual dinner," of, 218; treatment of fractures of the arm, at, 318.
St. George's Hospital, cases and clinical remarks at (see Brodie, Mr., and Seymour, Dr.), Clinical curiosities at, 349.
Stomach, disease of the external surface of the, 129.
Stone in the bladder, destruction of by percussion, 234, 360, 493, 531, 567; evacuation of fragments of, 296; Sir R. Dobson's case of, 335; former method of relieving, 345; of the female, case of, 499; lithotritic treatment of, 649, 679; case of, in which cutting ought never to be resorted to, 650; extraction of, by an Indian lithotomist, 656.
Stricture, temporary dilatation of, 314; of the urethra, 638; colon, Mr. Salmon on, 102; rectum, Mr. Salmon on, 338.
Strychnine, in palsy from lead, 69.
St. Thomas's Hospital, clinical lectures at (see Elliotson, Dr.), treatment of vomiting by the hydrocyanic acid at, 442; case of diseased heart, Dr. Elliotson's diagnosis, 506; singular case of epilepsy 507.
Students, neglect of the interests of, by the hospital surgeons, 55; examination of at Rhubarb Hall, 309.
Styptic, Halma Grand's, failure of, 315.
Subclavian, aneurism of the, cases of, 119.
Sugar, process for separation of, from the urine, 118; presence of, in the blood, 118; pure, new mode of preparing, 599.
Sulphur, hydruret of, 541.
Sulphate of copper in cholera, 628.
Surgeons, on the waste of their time in courts of justice, 96; to small vessels, inadequate remuneration of, 160; naval, degrading classification of, 312; military, exemption of, from the operation of the Apothecaries Act, 698.
Surgeons, of navy and army, on the exemption of, from the penalties of the Apothecaries Act, 793.
Surgery, Oak's examinations in, 311.
Symptoms of cholera, variations in the, 627; invariable in cholera, 628.
Syphilis, sarsaparilla in the secondary symptoms of, 703.
Syrup of sarsæ, Hudson's value of, 765.

T

Tanner's liquid in phthisis, 593.
Tartar emetic, new tests for, 403; external use of, in colds, 543.
Tauffier, Dr., on the antiseptic properties of hydrochlorate of tin, 771.
Taylor, Mr., on artificial dilatation of the vagina, 48.
Temperature of the body in cholera, suggestions for raising the, 94; in whites and negroes, difference of, 552.
Testicle, on chronic inflammation of the, 168, 315; removal of the, 318; fungoid disease of the, 606, 702, 767.
Thacker, Mr. R. W., on sulphate of indigo as a test for nitric acid, 302.
Thomson, Dr. (of Glasgow), analyses of the blood by, 225, 251.
Thorax, punctured wound of the, 604.
Throat, cut, cases of, 255, 509.
Thigh, injury to the, 316; fracture of the, 511; phlegmonous abscess of the, 446.
Thymus, gland, anatomy of the, 342; best mode of injecting and dissecting, 412, 414; physiology of the, 414; diseases of the, 415.
Thyroid gland, enlargement of the, 314; artery, ligature of the, for enlargement of the gland, 479.
Tibia and fibula, fracture of the, 190, 253, 316; necrosis of the, 703.
Tic doloreux, arsenic in, 617.
Tin, antiseptic properties of the hydrochlorate of, 771.
Tinctures, poisonous, Mr. Hiley on, 404.
Tipula, singular mechanism of the ovipositor of the, 116.
Tod on the Ear, review of, 728.
Toe, great, amputation of the, 316; dislocation of the metatarsal bone of the, 351.
Tonics, on the modes of administering, 177.
Tongue, inflammation of the, 639.
Toole, Mr. E., on hydriodate of potash in nebulæ of the cornea, 541.
Torsion of arteries, Manec on the, 664; cases in which it is preferable, 692.
Toryism, medical, rapid display of, 218.
Tracheitis, in the horse, 38.
Tracheotomy, in the horse, affection termed roaring, 40.
Tubercles in the left auricle, 215; Dr. Billing on, 422.
Turner's, Mr. J., suggestions for raising the heat of the body in cholera, 94.
Turpentine in rheumatism, 350; fomentation in cholera, 656.
Tumour, osseous, removal of, from the upper jaw-bone, 30; aneurismal, of the aorta, 214; pulsating not always aneurismal in the neck, 263, 313; osteo-sarcomatous of the upper jaw, 336.
Tumour, sarcomatous, of the neck, 509; of the thyroid gland, the artery tied for, 703; adipose, extirpation of a, 767.
Tweedie, Dr., on cholera in the City of London, 782.
Tympanum, muscles of the, 729; directions for exposing the apparatus of the, 731.

Tytler, Mr. J., on mercurial frictions in cholera, 562.
Typhus, Dr. Knight on the saline treatment of, 170; following cholera, cases of, 684.

U

Ulcer, phagedenic, of the glans penis, 763.
University of London, mode of examination at the, 179; distribution of prizes at the, 319; prospect of an hospital at the, 320; Dr. Harrison's offer to the, 376; on the establishment of its hospital, 375, 658.
University of Edinburgh, professorship of Mat. Med., at the, 250; appointments to medical chairs in the, 573.
Urea, presence of, in healthy blood, 603.
Urethra, new means of breaking fragments in the, 366; stricture of the, from external violence, 638, 702.
Urethra, structure of the, 783.
Urine, composition of, in diabetes mellitus, 117; retention of, from enlargement of the prostate, 703.
Uterus, hemorrhage from the, case of, 373; Ingleby on hemorrhage from the, 381; accumulation of blood in the, 382; connexion of the placenta with the, 406; absence of the, 625; carcinoma of the, case of, 717.

V

Vaccination, Dr. Venables on, 331, 374; thickness of the skin in, 619.
Vagina, on artificial dilatation of the, 48; nitrate of silver in discharges from the, 618; imperforate, 625.
Valerian, remarks on, by Dr. Epps, 180; different opinions of the virtues of, 181.
Vapour, atmospheric, relative and absolute quantity of, 554.
Vegetables, on the effect of consumption of, during prevalence of cholera, 566.
Venables, Dr., on vaccination, 331, 374; on a case of venous injection, 398.
Venables, Dr., on salt-water emetics in cholera, 778.
Ventilation, in stables, value of, 197.
Ventricle, fourth, of the human brain, hydatid in the, 168.
Verdict, curious one of a coroner's jury, 693.
Veterinary schools in England, cause of the institution of, 82.
Vienna, history of the cholera irruption at:—origin and progress of the disease, 354; predisposing causes, 355; exciting causes, 356; symptoms and forms of complaint, 356; prognosis, diseases in which it terminated, 358; morbid appearances, 359; treatment of the different stages, and sequelæ, 361; statistic details, 393.
Volta-electric induction, Mr. Faraday on, 246.
Vomiting of blood, advantage of auscultation in cases of, 205; treated by hydrocyanic acid, 441; and constipation, absence of, in intus-susception, 647.

W

Walker, Mr. J., on cases of congenital absence of both eyes, 169; on nitrate of silver in cholera, 628.
Ward, Dr. T. O., on croton oil in cholera, 773.
Water, cold, in collapse after parturition, 215; internal administration of, in the third stage of cholera, 582, 719.
Water, cold, in cholera, 774.
Waters, Mr. J., on Delpech's investigations into the cholera, 266.
Wakley, Mr., defence of, in Ramadge *v.* Wakley, 434.
Wayte, Dr., on the prevention of, by astringents, 751.
Weatherill, Dr., on the cholera in Liverpool, 270; cases of cholera, 271; on saline injections in cholera, 688.
Westminster Hospital, reports of cases at the:—Ambiguous case of luxation of the hip, 63; strangulated umbilical hernia, 64; mortification of the mouth from the abuse of mercury, 158; hypertrophy and enlarged cavities of the heart, 187; pleuritis, with effusion between the surfaces of the pleuræ, 188; jagged wound of the hand—hemorrhage from palmar arch, 189; œdema of the lungs with aneurism of the aortic arch, 221; gangrene of the lungs, vomicæ, and pneumatosis thoracis, without communication with the bronchiæ, 254; amputation of the penis; failure of Halma Grand's quack styptic, 315; aneurism of descending aorta, 507; throat cut, 509; sarcomatous tumour of the neck, 509; popliteal aneurism treated by compression, 510; punctured wound of the thorax, 604; concussion of the spine; fracture of the cranium—hernia cerebri, 605; phlegmasia dolens in the male, 637; adipose tumour; polypus of the nose; fistula; hare-lip; fungoid testicle; bougie in the bladder, 767; rebuilding of the, 630.
Westminster Medical Society, papers and discussions at:—rambling debates on the cholera, 21, 51, 85; contagion or non-contagion of cholera, 146; vote against contagion, 150; letter from Mr. Greenwood on the proceedings at, 150;

editorial observations on the vote and a voter, 157.
Whittaker, Mr., on delirium tremens, 392.
Whiting, Mr. J. B., on the use of cold water in cholera, 719.
Whiting, Mr. J. B., reply of, to the medical practitioners of Lynn, 795.
Wilkinson, Mr. E., on cases of supposed cholera in, 191.
Williams, Mr. W. R., on a case of injury, and amputation, of the penis, 769.
Wood, Mr. J., memoir of, 512.
Woods, aromatic, 217.
Worcester, on the formation of a new medical society at, 437.
Wound, jagged, of the hand, 189: punctured, by a bayonet, 305; of the thorax, 604.
Wounds in dissection, 313.
Worms in the bronchial passages of animals, 102.
Wright, Dr., on the cholera at Westminster, 1.
Wrist, dislocation of the, 350.

Y

Youatt, Mr., course of Lectures by, on veterinary medicine: — Lecture 24 — On the structure of the lungs; the pulmonary capillary circulation; the change effected in the blood; catarrh in the horse, ox, and dog; chronic cough, 7 to 12. Lecture 25—On laryngitis; tracheitis; roaring, its causes, and treatment, 37 to 43. Lecture 26—On epidemic catarrh in horses; the malignant epidemic, 77 to 83. Lecture 27—Ditto in cattle; bronchitis in horses and cattle; on worms in the bronchiæ of animals, 98 to 102. Lecture 28—Inflammation of the lungs in horses, cattle, swine, and dogs—symptoms, causes, and treatment—value of auscultation, 137 to 144. Lecture 29—Pleurisy in horses; operation of paracentesis pleurisy in cattle; in dogs; curious case, 161 to 165. Lecture 30—Phthisis pulmonalis in the horse and cattle; causes, treatment, post-mortem appearances; Swiss cattle, 193 to 198. Lecture 31—Chest affections; thick-wind; diagnosis by auscultation; broken-wind, causes, progress, and treatment, 257 to 263. Lecture 32—The head of the horse—its anatomy, physiology, and phrenology, 289 to 294. Lecture 33—The frontal bones and horns of horses and cattle; distinction of breeds, age, and sex, by the horn; the fly in the frontal sinuses of sheep, 321 to 326. Lecture 34—The frontal bones and antlers of deer; frontal bones of swine and dogs; parietal, temporal, occipital, sphenoid, and ethmoid bones; the cranial cavity, 385 to 392. Lecture 35—The brain in brutes; functions of the cineritious and medullary portions; intellectual principle in animals, 417 to 421. Lecture 36—On the brains of domesticated animals; chemical composition of the brain; difference of size; description of its various parts, 449 to 454. Lecture 37—Structure and functions of the cerebrum and cerebellum; experiments of Fleurens; spinal chord, and cerebral nerves, 481 to 486. Lecture 38 — Sketch of the nervous system; nerves of peculiar sensation; nerves of pure motion, 542 to 545. Lecture 39—Double origin and function of the fifth pair; structure of the spinal marrow, and double root of the spinal nerves; experiments of Bell and Mayo, 577 to 582. Lecture 40—Membranes of the spinal marrow; origins and distributions of the spinal nerves, 609 to 613. Lecture 41—Ninth and tenth pair of nerves, their origin, course, and functions, 641 to 646. Lecture 42—Nerves connected with the stomach; the cerebro-visceral nerve; experiments demonstrating its influence in digestion, 674. Lecture 43—The seventh pair of nerves; the sympathetic, or great organic nerve; universal influence of it over the functions of nutrition, secretion, &c.; conclusion of the nervous system, 708 to 712.
Youatt, Prof., Observations on the lectures of, 799; letter from, 799.

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